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On the Crossed Actions of the Spinal Cord. By A. Von Bezold.

10. *Course of Lectures on the Physiology and Pathology of the Central Nervous System.* By C. E. Brown-Séquard, M.D. 1860. Also in 'Journal de Physiologie.'

The difficulties met with in the study of the structure and functions of the central nervous organs depend chiefly on the great complexity of their organization. To the anatomist, the softness and delicacy of the component elements obstruct and render difficult microscopic elucidation, whilst the physiologist studying the results of vivisections on animals has to contend with, 1st, the difficulty of ascertaining phenomena in themselves *subjective;* and 2ndly, in instances when this is no obstacle, and in which the results are sufficiently objective, he has still the difficulty of being able to divide or to irritate one part without at the same time operating on others. Remembering these things, it will not appear strange that, in reference to the structure and functions of the spinal cord, opposite and conflicting opinions should still be entertained.

In regard to the method of investigating structure, there is but one plan which, with more or less modification of detail, is now generally adopted. This consists in hardening the organ by chromic acid, or alcohol, so that thin sections can be made, which may be rendered transparent by turpentine, chloride of calcium, &c. By maceration in carmine, observation may in some points be facilitated. By the study of many hundreds of such preparations, something like a general plan of the structure of this complex organ may be arrived at.

We feel that to enter into an account of anatomical details would only perplex and weary the general reader; but an idea of the plan or type of structure to which these details lead cannot fail to interest the most practical mind, since some conception of structure is essential to an understanding of healthy and diseased function.

In histological language, the spinal cord may be defined as a reticulated column of connective-tissue, containing in its substance blood-vessels, and in its meshes nerve-fibres and nerve-cells. It consists, in fact, of nervous and non-nervous elements, the latter being subservient and secondary to the former.

It is only quite recently that anatomists have recognised the existence of a considerable amount of connective-tissue in the spinal cord, and here as everywhere we owe much to the researches of Virchow. Although Keuffel, in the year 1811, demonstrated by a sort of maceration of the cord that a framework of connective-tissue permeated every part, subsequent investigators (Henle, Stilling, Arnold, Gerlach, Kölliker) overlooked it. The history of connective-tissue, and the controversies amongst its investigators, Reichert, Henle, Remak, Virchow, &c., would form subject-matter for many pages of writing. Suffice it here to say that in 1853* Virchow, in writing on the corpora amylacea, described the ependyma ventriculorum as a species of connective-tissue. Further, he pointed out that the central grey matter of the spinal cord (the substantia grisea centralis of Kölliker) is a continuation of this connective-tissue, and that the nerve elements of the central organs are everywhere permeated and held together by connective-tissue. In 1854, Owsjannikow, in his thesis 'On the Spinal Cord of the Fish,'† demonstrated that both in the anterior and posterior fissure of the spinal cord a process of the pia mater penetrates to the very centre of the cord and surrounds the central canal; and Kupffer,‡ in similar researches on the frog, showed that fibres of connective-tissue are connected with cells of connective-tissue. Finally, Bidder insisted that the grey matter is a matrix for the nerve-cells, consisting of connective-tissue in different stages of development and of numerous blood-vessels. In the white substance, too, there is found a framework of connective-tissue, connected at one margin with the grey substance, on the other with the pia mater. Each white nerve-fibre is encircled by connective-tissue, and in some transverse sections, in which the nerve-tubes have been dislodged by washing, the skeleton of connective-tissue present in the white substance is beautifully distinct. If the theory of Virchow, that all morbid cell-growths originate in the corpuscles of the connective-tissue, be true, the demonstration of this tissue in every part of the nervous system is pathologically important.

There can, we think, be little doubt that a large part of the grey substance of the spinal cord does consist of connective-tissue, but we do not feel disposed to agree with Bidder and his disciples in believing that all the cells of the posterior cornua are of this character; we should rather regard them (as Clarke, S. van der Kolk and others do) as undoubtedly nervous, whilst those of the *smallest* size are probably not so.

In studying the minute structure of this and other parts of the nervous system, the physiologist looks for some plan or type of structure which will harmonize with what experiment has taught him relative to the function of the organ. To us it appears that the time for such a generalization has not yet arrived, and that the many discrepancies amongst observers show the need for much patient inquiry. Nevertheless, some investigators have given such a connected and unhesitating account of their researches, that it is not difficult to lay before the reader a plan of the structure of this complicated organ.

The various views entertained on the structure of the spinal cord now presuppose that all nerve-cells give or receive nerve-fibres, and that no such thing as an apolar nerve-cell exists. In a functional point of view, cells may be regarded as—1st, organs of excitation or stimulation (motor cells); 2nd, as organs through which this excitation is brought into contact with the conscious principle (sensory cells); 3rd, as organs which communicate and modify this irritation in its passage from one fibre to another (reflectory or sympathetic cells). But supposing that cells are functionally distinct, is this difference in their function characterized by any structural peculiarity by which we could recognise them? A Russian observer, Jacubowitsch by name, asserts that they are morphologically distinct, and that in the spinal cord three forms may be distinguished—1st, large multipolar cells which he terms motor cells; 2nd, small cells with three or four processes, which become extremely ramified—sensory cells; 3rd, larger cells than the sensory, round and with only two processes—sympathetic cells. These views require much corroborative observation, and at present can scarcely be accepted on the strength of one observer's opinion.

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* Virchow's Archiv, Band. vi.

† Owsjannikow: Disquis. Microscop. de Med. Spin. Text. in Piscibus. Dorpat, 1854.

‡ Kupffer: De Med. Spinalis Textura in Ranis. Dorpat, 1854. Bidder: Untersuch. über die Textur des Rückenmarkes. 1857. Clarke: Further Researches on the Grey Substance of the Spinal Cord. 1859.

As regards the course and arrangement of the nervous fibres in the cord very various views are entertained. The simplest and most diagramatic is that of Bidder, and his pupils, Owsjannikow, Kupffer,* &c. These observers believe that the only nerve-cells in the cord are situated in the anterior cornua. These cells are multipolar, and they give off processes which have five distinctive relations—1st, there are processes which connect adjacent cells in the same half of the cord; 2nd, processes which connect together cells of opposite sides of the cord, forming the anterior commissure; 3rd, processes which course upwards to the brain and in the white columns, which are the aggregate of these processes; 4th, processes which go off to form the fibres of the posterior; and 5th, the anterior nerve-roots. According to this view the cells of the spinal cord are structures superimposed on the fibres of the anterior and posterior nerve-fibre in their course to the brain, serving on the one hand to connect the one side of the cord with the other, and on the other to connect anterior with posterior roots with each other and with cells above and below.

Stilling,† than whom no one has wrought more at the structure of the cord, takes a view not very different from that of Bidder. He supposes, however, that the posterior cornua contain true nerve-cells which, like the cells in the anterior cornua receive fibres from the corresponding nerve-roots, and send off other fibres to the brain in the white substance of the cord. The cells on the same and on opposite sides are connected in every direction.

In this country important and careful observations have been made by Mr. Lockhart Clarke, but these are of a character much more complex than the observations of most German writers. For a condensed account of Mr. Clarke's views, we would refer to a paper published by him in 'Beale's Archiv,' and to a recent paper in the 'Natural History Review,' in which may be found also an abstract of the latest observations.* Before leaving the subject of structure, we should like to give the generalization of German observations adopted by Funke in the last edition of his 'Text-book on Physiology.' These are:

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* Bidder und Kupffer: Untersuch. über die Textur des Rückenmarkes. Leipsig, 1857.

† B. Stilling: Neue Untersuchungen über den Bau des Rückenmarkes. 1859. Henle und Meissner's Bericht for 1859, p. 198.

"1. The nerve-fibres, which leave the cord in the anterior roots, take their origin in all animals (as in man) in the ganglionic cells, which are placed throughout the whole length of the cord opposite the points from which the roots go off. No root goes direct to the brain without the interposition of a ganglionic cell. This is the most certain result of all observations on the cord, and few will now dispute it. Even Kölliker, who at first stoutly denied this fact, and held that in the human spinal cord the cells were quite isolated, and had no connexion with the root-fibres, is now inclined to believe in the origin of a part of the fibres of the anterior roots in the cells of the anterior horns. I myself have not only in the frog, but also in the cord of the mammal, distinctly observed processes of the ganglionic cells run deep into the white substance, so that I have no doubt as to the above statement; and especially as no observer has been able to follow an isolated fibre through that cell-group into the white substance of the anterior or lateral columns. When we see a root fibre coursing amongst these cells without entering them, it is not at all improbable that the fibre terminates in a cell on a higher or lower level. Still less may one assume with Lenhossék, when we see a fibre apparently end free between the cells, that a free ending occurs in that spot, but must rather suppose that what appears to be the end of a fibre is merely a section of one as it is bending upwards.

"2. All the fibres of the anterior roots stand in *mediate* connexion with the brain, through the processes issuing from their ganglionic cells into the white substance. As a rule, a great number of root-fibres appear to end in a group of anastomosing cells, from which only one or two channels of communication go off to the brain. The existence of anastomosing systems of nerve-cells in the anterior horns, is, in my opinion, an undoubted fact not only in man and mammals, but also in the cord of the lower vertebrata, as I have convinced myself, contrary to the opinion of Kupffer and Owsjannikow. I have isolated cells from the anterior grey substance of the frog with more processes than the Dorpat school have described, in which some of the processes, more especially those which run inwards and backwards, divided in a fork-like manner. I have also most distinctly seen in the frog that the cells were connected with each other. How it happens that such a master of observation as Kölliker has observed no undoubted cell-anastomosis is an enigma. It remains to be proved whether the processes of these anterior cells ever end free.

"3. There is no crossing of the anterior root-fibres in the cord, but a mediate connexion of the anterior roots of both halves of the cord takes place through the anterior grey commissure, which is nothing else but the connecting fibres of the cells of both sides. What Kölliker has taken for a crossing of the anterior columns, and what others hold to be an anterior white commissure, is nothing else than the crossing of the connective-tissue fibres of the pia mater in the substance of the cord, which was first discovered by Arnold, correctedly described by Blattmann, and particularly so by Kupffer. We shall see that physiological experiment is also opposed to the idea of the crossing of the anterior roots. After several careful examinations of fresh and hardened preparations of the cord of the frog, I have been unable to

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[*] Goll, Trask, Reissner, Stieder, Dean, Traugott.

convince myself of the nervous nature of the system of cross fibres behind the anterior fissures.

"4. In regard to the relations of the posterior roots, we have less sure data. Bidder and his pupils hold that in a frog and in a fish all the posterior roots enter the ganglionic cells, from which the anterior roots spring. From my own studies in the subject I am convinced that this is the case with a *part* of the fibres, but not with all. Certainly, in the higher animals, in which the greater part of the posterior roots does not enter the ganglionic cells, Bidder's idea does not hold. Whether these fibres pass through ganglionic cells in the grey substance, before they pass over into the central channel of the white substance, and whether a crossing takes place behind the central canal, are still undecided questions. Schröder van der Kolk has lately convinced himself of the ending of the sensitive root-fibres in ganglionic cells of the posterior horn, and supposes that the farther conduction of impressions proceeds to the other side, and in this to the brain. He bases this supposition also on the analogy of the sensitive nerves which terminate in the grey nuclei of the medulla oblongata. The existence of a posterior grey commissure, and its importance as a cross-passage of the posterior nerve-roots, I hold as very probable; and I believe it occurs even in the frog, as Kölliker and others have lately testified. From a physiological point of view, the existence of fibres which go to the ganglionic cells of the anterior roots as the direct passage of the greater part of the posterior root-fibres in their passage to the brain, and, finally, decussation of the posterior root-fibres, may be supposed. A direct transition of the posterior root-fibres into the anterior I hold as improbable.

"5. The longitudinal fibres of the white substance come collectively out of the grey substance, and represent mediate or immediate continuations of the anterior and posterior root-fibres. All processes of the first rise from the ganglionic cells of the anterior grey substance in which the anterior root-fibres originate. How far the continuations of the posterior nerve root-fibres communicate directly, or through the medium of cells, with these fibres, is yet to be ascertained.

"6. The grey substance consists of a stroma for ganglionic cells, in which these connect themselves with root-fibres or with each other. It is doubtful whether in the grey substance nerve-fibres exist which are not related to its ganglionic cells."

In connexion with the structure of the spinal cord, we would refer to a very interesting paper by Dr. T. S. Clouston on the 'Minute Anatomy and Physiology of the Nervous System in the Lobster.' In this animal it is found that, in every essential point, the ganglia and interganglionic cord correspond to the spinal cord of the vertebrate animal. The origin of fibres from cells, the connexion of cells with each other, a connexion between the various groups of cells in different ganglia, and a correspondence between the number of ganglionic cells and that of the muscles which they minister to, are facts (Dr. Clouston shows) equally susceptible of confirmation in the invertebrate as in the vertebrate class of animals.

In analysing recent observations on the *function* of the spinal cord, it will be convenient to regard the organ in a three-fold character: 1st, as a centre for the reflection of impressions made on its afferent nerves; 2nd, as a sensorial centre; 3rd, as a conducting organ.

I. The spinal cord is a reflecting centre—i.e., an organ capable of converting impressions made on its afferent nerves into motor impulses, without and independent of the co-operation of the will. One of the earliest to perceive this fact was Prochaska, but it was left for our distinguished countryman, Marshall Hall, to indicate the importance and to explain the conditions of the reflex endowments of the spinal cord. Since his time (1833–43) the subject has been specially considered by Müller (1834); Volkmann (1838); Valentin (1839); Arnold (1842); Grainger (1837); Spiess (1844); Weber, Wagner (1854); and last, but not least, by Edward Pflüger (1853).

The study of reflex action can be conducted only under circumstances in which the influence of the will is excluded. Accepting the conclusion that the brain is the exclusive organ of the psychical functions; that in it only occurs perception of sensitive impressions (sensation); that from it alone proceeds the influence of the will, it would follow that all the motions produced in a decapitated animal were of a reflex character. But this conclusion has not only been doubted, but strenuously denied; and in the modern text-books of physiology we meet with the query, "Sensorium im Rückenmark?" A little consideration of this question may interest the reader.

In 1853, Pflüger published a work* in which it was contended, from experiments on animals, that the spinal cord is the seat of sensorial functions. In earlier times this idea was hinted at, and even distinctly indicated, by Prochaska, Legallois, Cuvier, and Volkmann. One of Pflüger's most striking experiments is the following: a frog is decapitated, and acetic acid is placed just over the internal condyle of the femur. The animal constantly bends the limb, and with the dorsal surface of the foot of the same side wipes off the acid by alternate movements of ad- and ab-duction. This we have often seen. The foot is now cut off, so that "wiping" is no longer possible, and the acid is reapplied. The animal bends the thigh as formerly (for it still supposes it possesses its foot), but it soon gives up this movement, becomes restless "as it seeks after a new method," and finally using the limb of the other side, bends it, so that by the sole of the foot the acid is removed. If other modes of irritation are employed, the movements which follow are said always to present an appearance of purposiveness (*Zweckmässig keit*).*

But mere *purposiveness* of action would not prove the existence of psychical activity in the cord, for are not all reflex arrangements *purposive* in their object? We must look to some other characteristic. Supposing that each sensitive fibre is connected mediately with a system of motor fibres, so that the former may, when excited, call forth a determined harmonious muscular action (such as the will would call forth), then we must expect that the action called forth by the excitation of a centripetal fibre must always be the same when the exciting agent is similar, and applied under the same conditions—in fact, with such a mechanical arrangement as we have supposed, we would expect a degree of regularity in the results of an excitation. It is urged by those who contend for the sensorial functions of the cord,† that a different reaction is observed when different modes of irritation are employed, and that each reaction is purposive in relation to the excitant. For instance (say they), if we pinch the thigh of a decapitated frog at a certain spot, he will either draw up the limb energetically, and place it under his belly, or he will stretch the limb as if to push away the instrument; or, if the medulla oblongata is retained, he will hop away. If instead of pinching this same spot we apply strong acetic acid, none of these movements follow, but the animal, by a to-and-fro movement of the foot of the same or of the opposite side, contrives to wipe off the offending acid. Now, supposing this experiment to be true to the letter, it might be urged that even with a determinate reflex mechanism different irritants might call forth different muscular actions. However, if this experiment gives results as decided as those just mentioned, we think it difficult to imagine such a possibility; for if the resulting muscular movements depend upon the construction of a mechanism, one of the limbs of which goes to the part at which our excitant is applied, it is scarcely probable that the play of this mechanism will be different (to the extent implied in the experiment) when the nature of the exciting application is different. In a former paper* we showed that, in their effect on the molecular mechanics of the nerve-fibre, chemical and mechanical excitation acted alike. Those who have had experience in such experiments say that a decapitated frog does not always act in the same way when acetic acid is applied to his thighs, but that it now adopts one expedient to remove the acid, now another. The first experiment we quoted shows a degree of accommodation to circumstances which can with difficulty be explained by the supposition of a reflex mechanism.

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* Die sensorische function des Rückenmarkes der Wirbelthiere nebst einer neuen Lehre über die Leitungsgesetze der Reflexionen. Berlin, 1853.

† Pfluger, Funke, Auerbach.

These experiments on decapitated frogs are certainly curious and interesting, but we must remember that their aim is to establish the possession of faculties purely *subjective* in character. They attempt to prove that animals far removed from man, and deprived of their brains, and consequently of the organs of special sense, are capable of sensation (consciousness of sensibility), perception of external conditions, and of adaptation of means to an end. The latter, of course, implies the exercise of what in the frog must constitute the highest psychical faculties. All these matters are of a subjective character. We *suppose* that another man feels the same degree and character of pain as we do when a needle is pushed into his skin; but we cannot prove it, much less can we dogmatize on the feelings of a decapitated frog. On the other hand, it appears to us worthy of remembrance that in fish and frogs anatomical observers (Kupffer and Owsjannikow) have pointed out that both sensitive and motor fibres of the cord arise from one and the same nerve-cell, suggesting a generalization of function. Does not this fact somewhat harmonize with Pflüger's experiments? Professor Schiff, whose opinion on neurological subjects is of the highest value, winds up a searching criticism on the sensorial functions of the cord with the following conclusions:

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* On General Nerve Physiology: British and Foreign Medico-Chirurgical Review, July, 1862.

"1. There is no reason why we should deny to the spinal cord of an animal recently decapitated the faculty of sensation.

"2. It is rather highly probable that in such a cord true sensation occurs after irritation.

"3. The fundamental principle, by which sensitive impressions become converted into movements, is the same in the brain and spinal cord.

"4. The spinal movements are distinguished from the cerebral essentially in this, that in the latter, as Cuvier indicated, the central spheres of the higher organs of sense (sight, hearing, perhaps also smell) co-operate in themselves and on the motory nerves, as reflecting excitants. It must, therefore, appear that the withdrawal of these centres must have a most important influence on the whole character of the motory and sensational functions of the spinal cord."*

The idea of consciousness and volition being present in the spinal cord Schiff entirely renounces, and he regards all the movements of decapitated amphibiæ as the mechanical consequences of irritation of a nervous reflecting organ. The operation of the irritant he believes to be determined by its *strength*, not by its *character*.

On the other hand, Funke avers that, "according to his opinion, from a purely physiological stand-point, the spinal cord possesses sensorial functions." The real difficulty of the question seems to consist in this, that we at present possess no certain indication by which movements of a reflex character—(i.e., purely mechanical), can be distinguished from those originated by a psychical impulse. Accordingly, any attempt to determine the laws of reflex action must prove unsatisfactory.

In the work of Pflüger to which we have referred, such an attempt has for the first time been made; and as his results are in part founded on an analysis of pathological observations on man, an account of them may interest the reader. They are shortly as follow:

1. The law of conduction on the same side for one-sided reflection. If, on irritation of a sensitive nerve, reflex action follows on one side only, the latter occur without exception and under all circumstances on that side only which corresponds to the irritated sensitive nerve. This law was recognised by John Müllar.

2. The law of symmetrical reflection. If a sensitive irritation has produced reflex movements on one side, and by further extension has aroused motor action also on the other half of the cord, only those nerves will be affected which correspond to those originally irritated on the first half.

3. The law of unequal intensity of reflex action on both sides of the body in double-sided reflexes. When double reflex occurs, it is always strongest on the side corresponding to the sensitive fibre irritated.

4. The law of intersensitive-motor movement, and of reflex irradiation.

By the first term Pflüger understands the channel by which the irritation travels from the sensitive to the motor fibres, in the central organ. By the latter term he understands the further progress of reflex action from the nerves in which it was localized to neighbouring nerves. If the excitation of a sensitive cerebral nerve is transferred to motor nerves, we see that the roots of both nerves originating on nearly the same level are affected, or the nerves lying behind, never those lying before the sensitive root to which the excitation was applied. Irritation of the optic nerve, for example, produces contraction of the iris —i.e., reflex from the optic to the oculo-motor nerve, intersensitive-motor movement from before backwards. In the spinal cord, the primary affected motor nerve is more or less on the same level with the root of the irritated sensitive fibre. When the reflex excitation extends itself in the spinal cord, it does so from behind forwards towards the medulla oblongata—never from before backwards. Excitation of a cutaneous nerve of the finger awakens reflex actions in the brachial plexus, next in the cervical plexus, the accessories, the vagus, &c., not in the dorsal or lumbar nerves. The irradiation first travels backwards after it has reached the medulla oblongata.

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* Lehrbuch, p. 221.

5. The law of threefold origin of reflex. The reflex motions which the excitation of a sensitive fibre produce can appear only and absolutely on three parts of the body, whether they are on one side or on both sides. (*a.*) Reflex appears in those motor nerves which lie more or less on the same level as the excited sensitive fibres. (*b.*) If the reflex appears in motor nerves at a distance from the sensitive fibres, these motor nerves are always such as arise in the medulla oblongata. This is exemplified in trismus after wounds of certain parts of the skin, hysterical laughings, convulsions. (*c.*) A reflex may occur in all the muscles of the body. The principal centre of radiation for this general reflex is the medulla oblongata. In proof of this, Pflüger adduces extensive pathological evidence.

The theory of reflex action is that it is effected by multipolar ganglionic cells, but it is still undetermined whether there exists a special set of centripetal fibres whose function is to conduct excitations to the motor nerve-cells, or whether this is effected by the same fibre whose function it is to conduct sensitive impressions to the brain. Nor has it been shown how excito-motory acts are confined to certain muscles or groups of muscles, so as to produce that precision of result which we witness in all the ordinary reflex acts.

Very interesting is the study of the agencies, psychical (the will, sleep) and physical (strychnia, opium, chloroform, &c.), which modify reflex action; but space forbids our entering upon these.

6. The spinal cord as a conducting organ. The labours of two contemporaneous physiologists have entirely revolutionized and remodelled our notions regarding the conduction of sensitive and motor impulses. To Brown-Séquard and Schiff we owe almost exclusively our present knowledge of the cord as a conducting organ.

Since Sir Charles Bell discovered that the conductors of sensation and volition are distinct from each other from the brain to the periphery, it has been a problem to show how they are disposed of in the spinal cord. For many years the theory originally propounded by, and naturally following the discovery of Sir Charles Bell, that the posterior columns conduct sensory, the anterior motory impressions, was supported by Longet, and all but universally received by physiologists. Through the wonderfully exact researches of Schiff and Brown-Séquard, this theory has been shown to be quite false. We propose in what follows to state what is now known regarding conduction in the spinal cord, adopting as a text the work of Schiff,[*] which, in point of minute detail, is the most learned and precise work on the subject. And first, in reference to sensory impressions. The posterior columns are the only sensitive parts of the spinal cord. This fact was first fully recognised by Longet. But it was not known to him that these columns owe their sensibility to the branches of the nerve-roots which traverse them, and not to the proper longitudinal fibres of the columns. The discovery of this fact is specially due to Schiff, and it has since been confirmed by Brown-Séquard.

Although these columns are the only sensitive parts of the cord, their complete section by no means arrests the conduction of sensation above or beyond the divided portion. Fodera, and afterwards Stilling and Van Deen, had noticed this fact in frogs, even after a considerable portion of the posterior column was removed. Section of the posterior columns produces hyperæsthesia of the parts behind the section. The first to indicate this fact was Fodera, and its subsequent elucidation has been claimed respectively by Brown-Séquard and Schiff.

That the grey substance conducts sensation is proved by dividing all but the grey substance. The conduction of sensibility after section of the posterior columns appears to take place in the grey substance only, as the anterior and lateral columns do not, in Schiff's opinion, conduct sensitive impressions, although Brown-Séquard, and, earlier, Schoeps, Calmeil and Budge, have claimed for the anterior columns the power of conducting such impressions.

What parts of the grey substance conduct sensitive impressions? Van Deen was the first to observe that sensitive impressions are conducted as long as any part of the grey substance is left undivided. In order to prove the capability of conduction in the different parts of the grey substance, these parts must be isolated from each other, and each tested when it alone forms the sole bond of connexion betwixt the anterior and posterior parts of the body. Brown-Séquard believes that it is essentially the *central* part of the grey substance—i.e., the lateral masses, the bases of the anterior and posterior horns, and all the substance around the central canal—which is employed in the transmission of sensitive impressions.[†] Schiff is of opinion that no good grounds exist to support such a view, but that any part whatever is capable of conducting sensibility, and that so long as a mere bridge of grey substance is left to unite the anterior with the posterior half of the body, evidences of impressions made on the latter can be readily obtained. It is interesting to observe that the *posterior* horns can conduct sensitive impressions, as from the anatomical views of Bidder, referred to in the first part of this review, we should be led to suppose that they would not do so, were they composed, as Bidder and his pupils affirm, of cellular tissue only. Schiff has also observed that the smaller the amount of connecting grey substance left, the more *slowly* are impressions transmitted. This explains an observation made by

[*] Lehrbüch der Physiologie des Menschen.

[†] Lectures, p. 23.

Cruveilhier,* who remarked, in a case of paralysis of motion, that fifteen to thirty seconds elapsed ere the patient perceived irritations of the paralysed parts. The relation of the parts of the grey substance to the parts of the body behind them is such that every segment of grey substance conducts the sensibility of all, rather than of special parts of the body behind it.† On the other hand, it must follow that impressions made on one small portion of the body are transmitted to the brain by conducting elements present in all parts of the grey substance of the spinal cord. This fact explains the retention *generally* of sensibility in many cases of partial softening, also why in hyperæsthesia or anæsthesia these alterations of sensibility are general and not partial. From personal experiment the writer can corroborate most of these observations on the conducting properties of the grey substance, and in connexion therewith it is interesting to recollect the anastomosing character of the nerve-cells.

The grey substance is in itself not sensitive. Although this fact was remarked by Longet, he failed to observe that it is the conducting agent of sensibility, whilst Stilling, Eigenbrodt, and others who recognised the latter fact, did not recognise its own want of sensibility. The first to perceive both properties was Schiff (1853). He terms the grey substance "æsthesodic" (from ὁδὸς, way). Strychnine, which increases the reflex irritability of the grey substance, does not impart to it sensitive properties.

As regards the power of conduction possessed by the posterior white columns, the experiments of Brown-Séquard and Schiff agree in proving that the white fibres are not capable of conducting, for any great length, sensitive impressions towards the brain, but that the transmission of such impressions, after it has traversed for a short distance the white substance, is then transferred to the grey matter. This rule applies in both directions—i.e., the white matter conducts a certain distance backwards (towards the tail of the animal) as well as forwards. These experiments harmonize with what is known anatomically of the course of the posterior nerve-roots.

One of the most curious results of Schiff's researches on the cord is that which relates to the condition which he terms *analgesia*. If in a rabbit every part of the spinal cord be divided, excepting the posterior columns, a peculiar state is induced, in which all parts behind the section are sensitive to a mere touch, but not to what in other circumstances would induce pain. In reference to this subject Schiff remarks:

> "Analgesia is a state which we observe in many persons during an advanced stage of chloroform inhalations, in which it perhaps always occurs, although it is often unobserved, because the patient is not sufficiently intelligent to give a clear account of his feelings. He accurately feels the hand of the surgeon which holds his limbs; he feels the impression of the knife as it is placed on the skin, but experiences no pain during division of the part; he feels the shaking during the sawing of the bone, and may even be busy with

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* Anat. Pathologique, xxxviii. p. 9.

† Brown-Séquard: Journal de Physiologie, i. p. 144.

pleasant dreams which perhaps this very shaking may have called forth or interrupted. A patient who is undergoing tooth-extraction is quite aware that the instrument is pressed into his jaw; he feels the tooth loosening from the gum, but the pain is absent, and he has only the indifferent feeling of its being done. A similar condition has been often (and first by Beau) observed in paralysis. A Swiss physician, Vieusseux, who had a disease in the central nervous system, for a long time remarked that he could feel with one hand impressions of touch and the pulse, but compression or pricking of the hand produced no pain."

Schiff then found that he could produce in rabbits a condition similar to that described by a rapid, but moderate bloodletting. This being done, every part of the spinal cord except the posterior white columns was divided. The sensibility of the parts behind being now tested, it was found that even a slight touch of the tail would cause the animal to raise its head, open its eyes, and move its ears, whilst the breathing, at the same time, became more hurried. If the tail, instead of being merely touched, was pinched until the nails reached the bone, none of these signs of disturbance occurs. From these and similar observations, Schiff concludes that the conductors of tactile sensibility are functionally and structurally distinct from those of sensibility to painful impressions; further, that these conductors reach the brain through the posterior white columns of the spinal cord; lastly, that each white column contains only the nerve-elements which convey the tactile sensibility of the corresponding half of the body, and that in this case the law of isolated conduction holds good, so that partial destruction of the white columns produces a lesion of sensibility in a corresponding part of the body.

The question of a decussation of the conductors of sensitive impressions in the cord is one on which there is still much difference of opinion. The simplest experiment on the question is that of division of one half of the cord. According to Brown-Séquard, the invariable result of this experiment is hyperæsthesia behind the section, whilst on the opposite side sensibility is lost, or extremely diminished. He thence infers that there is a decussation of the conductors of sensitive impressions very nearly, if not absolutely, complete (p. 35). In regard to the loss of sensibility on the side opposite the section, Van Deen, Stilling, Valentin, and Schiff, have found that, in many cases, sensibility is not at all diminished, and in none wholly suppressed. Indeed, Brown-Séquard himself has found that sensibility frequently returns or remains on the side opposite the section, but gives as an explanation, that by a reflex action the muscles of the side of the section (which is in a state of hyperæsthesia) contract spasmodically, and thus produce pain. To this view Schiff raises objections which it would occupy too much space to enter into. Nor does he agree with Brown-Séquard as to the results of the other experiment on which the latter argues that a decussation of the sensitive conductors takes place. From a very great number of vivisections on all species of animals, as well as from the study of pathological cases, Schiff has come to the conclusion that the principal mass of grey matter serves to conduct sensitive impressions from either one side of the body or the other. To the extreme right of the grey matter a very small portion exists, which conducts sensitive impressions from the left side of the body only, and *vice versâ*. The grey matter conveys impressions in all directions; and the agents employed in this omnilateral transmission are the ganglionic cells. To meet Schiff's experimental results, anatomy has to show that on either edge of the grey matter to the extreme right and extreme left, a group of cells exists, the process of which is not connected with the sensitive root-fibres of the same, but of the opposite side. As far as we are aware, no observer has yet ascertained whether such a relation holds. Schiff also maintains that pathological cases favour his ideas as to decussations much more than they do those of Brown-Séquard.

Several years ago, Von Bezold made an extended series of experiments on the question of decussation, and the result was equally adverse to Brown-Séquard's views.*

All experiments on the conduction of motory impulses are much more difficult than those on sensation, principally because one must wait until the animal makes a voluntary movement. No irritation, save that of the will, can be relied on. Another source of difficulty lies in the greater delicacy and vulnerability of the parts of the cord which conduct movement, a very slight degree of compression serving to produce a paralysis of motion.

Continuing our analysis of the experiments of Schiff, we find that in agreement with Van Deen, he believes that the anterior and lateral white columns transmit movement in the direction of the long axis of the cord. If in a frog all but the white anterior and lateral columns be divided, voluntary movements are still possible. In consequence of the difficulty of avoiding injury of these columns, the movements are always weaker than normal. In mammals, Schiff has also succeeded in observing voluntary movements when all but these columns were divided. As anatomy would indicate, and as pathology sanctions, these fibres do not go out of the cord through the motor roots, but join the cells in the anterior grey matter, which also serves for the conduction of movement. If the anterior and lateral columns are divided, after a time voluntary movements are strongly and harmoniously produced behind the section, so that the view of Longet, that the white anterior columns are the sole conductors of movement, is thus proved to be false. The question which next arises is—What portions of the grey substance conduct motor impressions? The answer is not such as former notions would lead us to expect, for Schiff found that when all the anterior parts of the cord were divided, and only a thin segment of the posterior grey substance (a segment so small that it could not be seen with the naked eye!) was left, spontaneous movements of the parts behind still took place.

Other experiments showed that the central grey substance, as well as the posterior, conducts motory impulse, whilst experiments instituted to ascertain the *direction* in which conduction of motor impulse could occur, showed that in transmission of motion the grey substance effects this *in every direction*. We have seen that with sensitive impressions, the same law holds. How and why two such different elements as white and grey matter should both serve to conduct motor impulses we know not; nor do we know how the isolated transference of the impulse of the will is conveyed to determinate muscles. Equally ignorant are we, whether the cell processes which transmit motion are also capable of transmitting sensation.

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* Ueber die Gekreuzten Wirkungen des Rückenmarks: Zeitschr. für wissensch. Zoologie, ix. p. 307.

Betwixt the sensitive and motory properties of the cord, analogy would appear to exist on more points than one. Thus, as in the case of sensation, the elements which conduct motion, white as well as grey, are in themselves not motory. To distinguish this property, Schiff proposes to term these elements "kinesodic." The experiment of Longet, in which it was supposed that galvanic irritation of the white columns produces motion in the parts behind, shows nothing beyond the fact that the muscles supplied by the nerve-roots going from the point when the excitation is applied, may be excited to action. The only motory parts of the cord are fibres of the anterior roots, which traverse obliquely, or at right angles, the anterior white columns. As already indicated, the fibres of the latter are not prolongations of the anterior roots, but originate in the cells of the grey matter.

On dividing one-half of the spinal cord, it is found that although there is no complete paralysis of motion, certain groups of muscles have their action weakened, and some are paralyzed. Amongst the latter are the muscles of respiration on the same side, and if the section is made above the origin of the phrenic nerve, the respiratory movements on that side are wholly suspended; but if the section is lower—say opposite the last cervical vertebra—the paralysis is confined to the external respiratory muscles. This result is specially due to division of the lateral columns.

Space forbids us entering upon the causes of the hyperæsthesia which is observed after section of parts of the spinal cord, and we can do no more than remind the reader of the remarkable proneness to convulsive movements which Brown-Séquard found to follow certain lesions of this organ.

In concluding this exposition of the views of Schiff, it may be of interest and service to the practical reader to put here verbatim the pathological corollaries which that physiologist deduces from his researches in the laboratory, so that by comparative observations at the bedside, and in the post-mortem theatre, the truth of these conclusions, as well as their diagnostic value, may be tested:

"1. Although, contrary to a common supposition, a limited disorganization of the posterior columns does not produce insensibility to pain in the parts behind, and a corresponding limited lesion of the anterior columns does not produce loss of movement, a *total* disorganization of these columns produces in the former case anæsthesia, in the latter paralysis.

"2. When perfect anæsthesia is the result of an injury which affects the spinal cord at a limited spot only, somewhere in the neighbourhood of only one or of some dorsal or cervical vertebræ, not only the posterior columns, but the whole of the æsthesodic substance (including the anterior horns) must be affected at the injured part.

"3. Complete paralysis in all parts of the body behind a diseased portion of the cord in the long direction may occur—

"*a*. Without any lesion of sensibility, save only constricting pain at the level of the diseased part, in cases of mere compression of the cord through dilatation of the vessels, with effusion, exudation or affection of the sheath.

"*b*. Without lesion of sensibility, and without any pain, perfect paralysis must occur in disease of the antero-lateral columns and of the total kinesodic substance, if one may suppose this substance to be affected independent of the æsthesodic.

"*c*. Should it be certainly proved that the central parts of the grey substance contain no kinesodic elements which conduct longitudinally, one would expect perfect paralysis of the posterior parts of the body, if anywhere, in the long direction, the antero-lateral columns, the four grey cornua and gelatinous substance of Rolando are affected, spontaneous pain is absent, sensibility to pain remains, but on account of the simultaneous limitation of the transverse extent of the æsthesodic substance, perception of pain is retarded.

"*d*. Paralysis occurs in disease of the anterior column and the whole grey substance; in this case sensibility to pain is lost, but sensibility to touch remains.

"*e*. Disease of the kinesodic substance, and of some parts of the anterior column, is accompanied with paralysis affecting certain parts and passing by others.

"4. Contraction and convulsions in parts of the body behind the injured spot can never be the consequence of an isolated irritative affection of the antero-lateral columns, or of the grey substance, since no motor fibre—i. e., none which upon irritation call forth movement—traverse the spinal cord in the long direction.

"5. These symptoms, however, may occur in a transitory form in disease of the anterior columns, exclusively in the muscles supplied by the nerves traversing the diseased portion that is on a level with the injury.

"6. Convulsions of the posterior extremities, with cramp, and generally with contraction, in very great irritability, with extension, also, which makes their free movement impossible, occurs in diseases of the higher divisions of the cord in the dorsal or cervical regions. In such a case, it is from disease of the posterior columns or the nerves traversing them, which act reflectively on the motory parts. Some change of the reflecting grey matter, analogous to that produced by certain narcotic poisons, might also give rise to contractions of the feet.

"7. Contraction may also be produced by slight pressure on the cord. In such a case, the contraction depends on a paralysis of the extensors, and it disappears in sleep, or if the person lies very quiet, and also in the early stages of ether narcotism; whilst in cases of cramp from irritation of the posterior columns, a much higher degree of narcotism is required.

"8. Disease of the posterior columns can also (through the laws of reflexion) call forth convulsion and contracture in parts higher up, towards the head.

"9. Disease of one posterior column, which causes slighter reflex contractions, has its effects confined to the limbs of the corresponding side.

"10. Isolated degeneration of the posterior columns, a little way above the cauda equina acts:

"*a*. In an *irritative* way, by producing pain (spontaneous or on pressure) in those nerves which traverse the diseased part, and often a constricting pain at the part affected, and a little above it. The longitudinal fibres of the posterior columns call forth, in the part of the body behind, a constant sensation of touch, described as formication with varying sensation of heat and cold. Sense of pain in the parts behind continues, and is even exalted, as long as the posterior columns are hyperæmic.

b. When such a lesion acts in a *paralysing* way, we find at the level of the paralysed roots a circle of the body which is quite *anæsthetic*, whilst sensation to pain is present above and below this ring. This anæsthetic circle is not sharply defined, and appears of different breadth at different times. Below this circle, sense of tickling, touch, and temperature, is absent. Pain or pressure is very well perceived, but is imperfectly *localised*.

c. When a disease at first acts irritatively (softening, for example), and extends along the cord from below upwards, the painful circle travels with it, leaving an extending anæsthetic circle. Below this, objective sense of touch is absent, but instead of it, there is subjective and perverted sense of touch (formication).

"11. When, with motion unaffected, a painful ring is present without alienation of sense of touch, we should find that the nerve-roots alone, outside the cord or within it, were affected.

"12. When, besides the posterior columns, the whole æsthesodic substance is diseased, we shall find *behind* the diseased spot perverted sensations of touch and no sense of pain.

"13. A disease of the spinal cord which first produces convulsions and then a perfect loss of voluntary movement in the provinces of nerves which go off behind, extends to the anterior and posterior columns and the kinesodic substance; it does not however require to go so far into the parts conducting sensitive impressions in order to produce perfect anæsthesia.

"14. A disease of the cervical portion of the cord, which, with paralysis of the extremities and the trunk, permits the respiratory movements to take place, does not affect the lateral columns.

"15. Isolated degeneration of one lateral column suspends the respiration on this side only. If both lateral columns below the fourth vertebra are diseased, the lower ribs are not used in respiration, which becomes strongly abdominal.

"16. When the lower part of the cord is paralysed, the motions in the different muscles of the upper part of the body become more frequent and stronger.

"17. Since only a small bridge of grey matter is sufficient to transfer sensitive or motory impressions, it is possible that in many cases, especially when this grey substance is surrounded by softened parts, softening occurs after death, and the connecting link of grey matter is not observed, and fluid matter fills up the gap.

"18. The above physiological experience may be applied to cases of wounds which scarcely disturbed the function of the cord (a bridge of grey matter being left?).

"19. It is rare to find that after a lesion of the spinal cord, feeling or motion is permanently lost on *one* side only."

Space does not permit our alluding to the influence which the spinal cord exerts on the functions of respiration, circulation, nutrition, heat, and digestion. We have been principally anxious to expose the views of our German brethren on the structure and functions of the cord, and must offer this as a reason for having made but slight reference to writings which can be readily found in the English literature of the subject.

Review II.

On Rupture, Inguinal, Crural and Umbilical; the Anatomy, Pathology, Diagnosis, Cause, and Prevention; with New Methods of effecting a Radical and Permanent Cure; embodying the Jacksonian Prize Essay of the Royal College of Surgeons, London, for 1861. With numerous Illustrations. By John Wood, F.R.C.S. Eng. (Exam.), Demonstrator of Anatomy at King's College, London; Assistant-Surgeon to King's College Hospital; Surgeon to the Lincoln's Inn Dispensary.—*London*, 1863. 8vo, pp. 326.

Mr. Wood's book upon Rupture is one which deserves at our hands more than a mere passing notice. Whether we consider the number of those who are afflicted with hernia, or whether we look to the good hope which is held out of curing many of these, the subject is full of interest to the practical surgeon, as well as to all those who are anxious to see the healing art making progress in the great contest which it is carrying on against death and disease.

It has been estimated by Malgaigne that in France one man in thirteen, and one woman in fifty-two, are affected with rupture; and there is no reason to think that the proportion would be very different among our own population. And not only is hernia a very common complaint, but it is one which is always attended with great inconvenience, and which may at any time put life in the utmost peril; moreover, it often forms a serious disability, preventing some from entering trades or professions for which they are otherwise well suited, and obliging others to relinquish the occupations by which they obtain their livelihood. Boys are frequently unable to gain admission into naval or military schools because they are ruptured, and cases have already arisen in the brief history of Mr. Wood's operation in which it has enabled lads to pass the inspection of a medical board by which they had previously been rejected. This is one of the best proofs that could be given of the satisfactory results of the operation.

It would appear, from official reports, that about three men in every thousand are annually lost to our army and navy by ruptures. Governments have always been anxious to prevent this waste of material, and on one occasion, at the beginning of last century, as we learn from Mr. Wood, our own Sovereign bestowed the honour of knighthood, together with substantial pecuniary rewards, on an individual who undertook to cure ruptures by the application of oil of vitriol—a cure which must have been so far from being complete or permanent that it is difficult to understand how it gained any credit at all. This serves, however, to show how important it is that there should be some means of curing ruptures, and preserving the efficiency of those whose maintenance and instruction has cost the country a large sum of money.

Again, rupture is not only a complaint which affects a large number of the community, and deprives us of the services of many of our soldiers and sailors, but it places the lives of those who are the subjects of it in constant jeopardy. At any moment the hernia may become strangulated, and give rise to the most alarming symptoms, frequently terminating in death. It is true the patient may wear a truss, and if the rupture is of moderate size a well-fitted truss may serve to control it; but then the truss must be worn almost constantly. It can never be laid aside except when the patient is in bed. It is always troublesome and inconvenient, and unless it fits closely the rupture will be liable to come down behind it; while if the protrusion is of large size, it is hardly possible to keep it in check at all by means of a truss.

Here, then, is the disease that we have to deal with—a complaint which is very common—which constitutes a serious disability, but which is not necessarily fatal, and which frequently admits of a palliative treatment. This palliative treatment, however, must be lifelong; it is apt at the best to be irksome, and to many cases it is not applicable at all. Mr. Wood has instituted a very fair comparison between hernia on the one hand, and varicose veins, hæmorrhoïds, and such other complaints as admit of both palliative and radical treatment on the other, and he draws the just conclusion that, if an operation is justifiable in the one case, it is at least equally allowable in the other, provided always that such operation is not attended with any extraordinary risk.

In the present treatise, which is an expansion of the Jacksonian Prize Essay for 1861, the author describes the operation for inguinal hernia which he has been in the habit of performing for the last five years, and which has met with so much success that it may fairly be considered to have taken its place among the resources of operative surgery.

The volume before us is divided into three parts. In the first, the author treats of inguinal hernia, and after describing the anatomy of the parts concerned and the pathological changes which accompany rupture, he proceeds to explain his own operation for effecting a radical cure. This, together with the appendix, which contains an account of 60 cases that have been operated on, is by far the most important portion of the book. In the second part the author deals with femoral hernia. First of all, the reader is reminded of the anatomy of the region, and then Mr. Wood describes an operation which he believes would effect a permanent cure, but which he has never yet performed on the living body. We are inclined to think that this operation will never meet with so much favour as that for inguinal hernia, partly because it is more difficult to perform, partly because it is attended with greater danger, and partly because femoral rupture is generally met with in women whose occupations are usually light, and whose cases call for less active treatment. Umbilical hernia is the subject of the third part, and for this variety also Mr. Wood has an operation to suggest, which he has performed several times with perfect success.

All the operations which our author recommends are based upon the same principle, and consist in stitching or pinning together subcutaneously the edges of the hernial opening—be it inguinal, femoral, or umbilical—and getting up a degree of inflammation which leads to consolidation and closure of the aperture through which the bowel protrudes. Of course, the steps of the operation vary in each particular case with the anatomy of the parts, but the principle is the same in all. For the details we must refer the reader to Mr. Wood's descriptions, and to the excellent drawings by which they are illustrated. By the help of these, anyone who is tolerably familiar with anatomy can soon make himself master of the operation, though the skill, which is so necessary to distinguish the different tissues which are felt when the finger is introduced along the track of the rupture, is a thing which can only be acquired by practice. In pointing out the leading principle of Mr. Wood's operations we have said enough to show how they differ from all others which have been proposed for the same object, particularly from Wutzer's, which is the only one that has obtained any favour of late years.

> "It will be seen that in the various methods originated and practised by the author for the cure of hernia, this novel principle of compression and closure of the tendinous sides of the hernial canal in its entire length prevails throughout. In this important particular they differ entirely, both from the older and more modern operations; all of which either deal with the sac almost solely, or rely upon the principle of dilatation or plugging of the canal." (p. 90.)

Mechanical appliances have now been carried to such perfection, and so much has been done to make ruptures tolerable by improved trusses, that we are not justified in recommending any operation attended with much danger, and hence it happens that various heroic proceedings which have from time to time been proposed have fallen into disuse, and are now only remembered as interesting facts in the history of hernia. An account of some of these will be found in the volume before us.

Mr. Wood's operation is a good example of the triumphs which we may fairly expect will result from the attention which is being paid now-a-days to the details of every branch of medical and surgical science. It is not a remedy which has been thrown in our way by chance—a haphazard cure, like some of the older methods—but it is a deduction based upon a thorough knowledge of anatomy and pathology. Every now and then we may be fortunate enough to hit upon a specific remedy like quinine or iodide of potassium; but the only progress which we have a right to look for and to expect is that which results from our own minute observation. Careful study and research cannot fail to lead to important conclusions; indeed, by these means many triumphs have been already achieved, and among the most recent we may place the operation under consideration.

The results which Mr. Wood has obtained are, on the whole, very favourable. He wisely abstains from making himself answerable for the operation in the hands of others, and the conclusions which he draws are founded entirely on the cases which have been treated by himself. Out of the 60 cases on which he has operated, and of which the appendix contains a full account, there has been only 1 death, and that from pyæmia, three weeks after the operation. In 11 cases the rupture has returned after a longer or shorter time; in 6 more the result is doubtful, while in the remaining 42 it has been successful. We have therefore a proportion of 65 or 70 per cent. of cures; and in estimating these results, we must remember that the cases have not been selected, they have been taken almost indiscriminately. Wherever a case presented itself in which an operation was not clearly forbidden, Mr. Wood has been willing to put his method to the test. And further, we must bear in mind that the operation has gone through several modifications, and perhaps it has hardly yet reached its full development. Here, as it so often happens in surgery, the credit of the operation will greatly depend upon a judicious selection of cases; but when this is made, and the operation is properly executed in its most approved form, we may expect a still larger proportion of successful results.

Is this balance of success such as to induce those who are affected with rupture to incur the risk of the operation, and to justify us in recommending it? Among the upper classes—those whose occupations are very light, and who can afford the best of trusses—the opportunities of performing the operation will probably not be so numerous as among the middle and lower classes. But wherever we meet with a rupture that is not easily controlled by a truss, and that is apt to descend when any unusual exertion is made, giving rise to painful and dangerous symptoms, there we may fairly set before our patient the operation for a radical cure, and we have no doubt that many would be glad to purchase future comfort and safety at the cost of a slight present risk. Among children the operation is so very successful, and, as far as one can judge, so free from danger, that we may well join Mr. Wood in hoping that some day it will be as commonly performed upon infants of all classes as the operations for hare-lip or club-foot now are.

The treatise before us contains some useful sections on trusses. Mr. Wood points out the disadvantages of those now in use, and the true principles upon which pressure ought to be applied; and he gives drawings of the "ring" and "horse-shoe" pads which he has introduced, and which he has found very efficient in practice. The convex pad of the ordinary truss tends to dilate rather than to contract the hernial opening, and when applied after operation the pressure promotes the absorption of the adhesions upon which the cure depends. This led Mr. Wood to devise the "ring" and "horse-shoe" pads for direct and oblique hernia respectively, and by these means pressure is exerted upon the sides of the aperture, and not immediately over it. This improves our chance of curing ruptures by truss-pressure alone, and after operation it avoids the danger of absorbing the recent adhesions.

The appendix is, as we have said, one of the most important parts of the volume before us. It contains a detailed account of 60 cases operated on by the author, and a review of these furnishes us with several interesting observations.

It may be asked, how far the cures effected by Mr. Wood deserve to be called "radical" and "permanent"? Have they stood the test of time and the wear and tear of life? The best answer that we can give to this question is supplied by the first case in the appendix. The patient, a printer, aged twenty-five, was operated on in April, 1858, for a direct inguinal hernia:

> "After seven months the use of the truss was entirely discarded. His occupation has necessitated the frequent lifting and removal of heavy 'forms' of type, which often weigh as much as a hundred-weight. On the last occasion," when he was seen, "(a few months ago), the groin operated on could not have been distinguished from the other, except by the presence of the small scars. No impulse whatever, or any bulging, was observable." (p. 264.)

Again, as we have before mentioned, among the 60 cases there has only been one death, and that from pyæmia, three weeks after operation, and when the patient was convalescent. This seems to show that the proceeding is not attended with any special or peculiar dangers.

Again, we hear little or nothing of peritonitis, which one might have expected to have been a fruitful source of anxiety. On two or three occasions some slight symptoms arose, but they were easily met and speedily disappeared. This confirms the opinion which is now gaining ground, that we may deal with the healthy peritoneum with more impunity than was formerly thought possible.

Again, it is remarkable how little, on the whole, the patients seem to suffer; indeed, several of the children, on whom the operation was performed with "rectangular pins" could hardly be said to *suffer* at all.

In a few places the diction of Mr. Wood's book might be improved; but we are not disposed to find fault with it, for as a whole it belongs to the highest class of surgical literature. The author, moreover, commends himself to our minds by the candour with which he lays before us the history of his operation. Nothing seems to be kept back; everything is told, and we are put in possession of all the data for forming an opinion. We expect to see Mr. Wood's operation take its place among the recognised means of dealing with rupture, and we shall be glad to note its further progress, and to observe what results it yields upon a more extensive trial, and in the hands of other surgeons.

REVIEW III.

Physiological Memoirs. By WILLIAM HAMMOND, M.D., Surgeon-General United States Army, late Professor of Anatomy and Physiology in the University of Maryland, &c.—*Philadelphia*, 1863. pp. 348.

THE memoirs which form this volume, we learn from the author in his modest preface, are a reprint, and that they were for most part first published in the 'American Journal of Medical Sciences.' As a whole, they impress us favourably with the candour, accuracy, and ability of the writer. He enters on his inquiries in the true spirit of the experimental physiologist, and duly impressed with their importance. He justly remarks: "It is only by actual experiment we can ever hope to lay the foundations of true physiological science;" that "the day of extravagant theories, unsupported by observation, has gone by, and that he who has nothing better to offer than the unsustained creation of a dreamy mind meets with but little attention, and merits still less than he receives."

Would that those philanthropists, and more especially philanthropists of the medical profession who take an active part in the great temperance agitation now going on, were of his way of thinking; then we should have better hopes of a successful issue of their exertions, and we should be at least spared the reading of much idle declamation and erroneous doctrine—the latter little creditable to the zealots who, like bigots in religion, fancy themselves almost infallible, and write and speak oracularly.

The subjects of Dr. Hammond's researches are various, and many of them are highly interesting and important. The zeal he has displayed in conducting them is most meritorious,—not made *in corpore vilo*, many of them, such as his trials on certain articles of diet, having been made on himself, at the risk of health, and persevered in, in one or two instances, even after the production of disease. He justly observes:

> "There are many obvious advantages to the physiologist, and also to the science which he represents, in basing his conclusions, whenever practicable, on investigations instituted upon himself. He is assured of their correctness, and knows fully the conditions under which they are performed. On the contrary, when others (such at least as are most likely to come under his observation) are the subjects of his researches, he can never be certain that his directions have been complied with, or that he has not been otherwise deceived."

In expressing the high sense we entertain of the work, we must not omit to point out the goodness of its style, so clear and simple, and so appropriate to the matter, nor, though briefly given, the information he affords of what has been accomplished by others in the same field of research.

We shall, so far as our limits permit, notice the several memoirs seriatim, oftener than we could wish giving merely the principal results or conclusions.

In the first memoir he has endeavoured to determine whether the theory of Liebig, in regard to the formation of urea from uric acid, could or could not be sustained by further investigation. The conclusion he arrives at from his experiments is rather affirmative, but is not absolutely conclusive. According to that theory, the more the blood is oxygenated the more should urea predominate in the urine, and the less the uric acid. In trials on himself he found that when using active exercise there was a diminution of uric acid and an increase of urea in the urine; and, as agreeing with this, he refers to the well-known fact that the urine of certain animals remarkable for activity—lions, tigers, and dogs—abounds in urea; whilst of certain others of sluggish habits—serpents, for instance—feeding also on animal food, it consists almost entirely of uric acid or urate of ammonia. In an experiment on a snake, he found, in accordance also with the theory, that whilst its activity was augmented by giving it oxygen to respire, urea, not before present, then appeared in its urine. The result is a curious one, and were it confirmed on repetition of the experiment—the experiment was a single one—it would be a significant fact. Opposed to the theory, as he admits, there are other facts—for example, the urine of birds, creatures of high temperature and extreme activity, yet consisting chiefly of the acid in question. We will add another instance, one he does not mention—the urine of the batrachians, which, not excepting even that of the indolent toad, bears a nearer resemblance to the urine of the lion and the other felidæ, than to that of the snake, its principal ingredient being urea. Whether, as he thinks, future investigations will do much towards the establishment of Liebig's theory, seems to us, we must confess, very doubtful.

In his next memoir, the second, entitled "Urological Contributions," he considers the effects of coffee and tea upon the urine, and incidentally upon the system generally. From well-conducted experiments on himself, the conclusions he comes to, and we think in a very satisfactory manner, are:

> "1. That both tea and coffee are excitants of the brain and nervous system, and at the same time considerably retard the metamorphosis of the tissues.
>
> "2. That tea possesses a more powerful influence in restraining the destruction of the tissues than coffee; whilst this latter substance is a greater stimulant of the cerebral faculties and of the nervous system."

His third memoir, "On the Excretion of Phosphoric Acid by the Kidneys," we pass over, his experiments, though deserving of the attention of the physiologist, being, as he admits, not sufficient in number to allow of definite conclusions from their results.

His fourth memoir is on the important subject of "The Physiological effects of Alcohol and Tobacco on the Human System." Admitting with other inquirers that alcohol contributes nothing to the production of tissue, he concludes from his experiments "that it increases the weight of the body by retarding metamorphosis of the old tissues, promoting the formation of new, and limiting the consumption of fat." The effects he observed on his own person during five days, whilst he was using four drachms of alcohol diluted with an equal quantity of water at each meal, were the following:

> "1. The carbonic acid and aqueous vapour given off in respiration were lessened in quantity.
>
> "2. The amount of fæces was diminished.
>
> "3. The quantity of urine was reduced.
>
> "4. The urea, chlorine, and phosphoric and sulphuric acids were diminished in amount."

He adds—and his words are especially deserving of attention, as pointing to the principle on which hinges the use and abuse of alcohol and of all alcoholic fluids:

> "These effects occurring when the amount of food was below the quantity required to maintain the weight of the body under the mental and physical exercise taken [in one series of experiments] were productive of no deleterious results to the system. On the contrary, when the food was sufficient to balance the waste from the exertions, and still more so when an excess of aliment over the demands of the organism was ingested [as in another series of experiments], the healthy working of the system was disturbed, and actual disease almost induced."

From his experiments on tobacco, smoking one hundred and fifty grains—nearly two cigars—after each meal, for five days, the following conclusions were deducible:

> "1. That tobacco does not materially affect the excretion of carbonic acid through the lungs.
>
> "2. That it lessens the amount of aqueous vapour given off in respiration.
>
> "3. That it diminishes the amount of fæces.
>
> "4. That it lessens the quantity of urine, and the amount of urea and chlorine.
>
> "5. That it increases the amount of free acid, uric acid, and the phosphoric and sulphuric acids eliminated through the kidneys."

He states that "tobacco, when the food is sufficient to preserve the weight of the body, increases the weight; and when the food is not sufficient, and the body in consequence loses weight, tobacco restrains the loss." He adds: "Unlike alcohol, this influence is unattended with any unpleasant effects upon the circulating system, though its action on the brain and nerves is certainly not such as is always to be desired." He thinks that when used in greater moderation than in his experiments, this influence would doubtless be greatly diminished —an opinion which accords with our own experience. He further adds in a note, that after further observations, continued during the last four or five years—these verifying the preceding—he is convinced that, in a great majority of cases, a moderate use of alcohol and tobacco is useful. One effect of tobacco he particularly insists on—its action on the salivary glands—it being a well-known fact that whatever increases the amount of saliva increases the quantity of gastric juice; and thus, he thinks, that a cigar after dinner may not only have a soothing influence on the mind, but also a beneficial effect on the stomach; and we would say on the bowels also, as what stimulates the one commonly by sympathy has a like effect on the other. In one of our earliest physiological experiments we remember how, when the stomach was galvanized in a recently-killed animal, the intestines were excited into action.

His fifth memoir is entitled, "Experimental Researches relative to the Nutritive Value and Physiological Effects of Albumen, Starch, and Gum, when singly and exclusively used as Food." These researches are admirable examples of the kind, and they bear, in the manner in which they were conducted, such marks of exactness and accuracy in all their details as to inspire much confidence in their results, and in the conclusions deducible from them. He prefaces the account of his experiments by an introduction, in which he briefly reviews the received doctrine of nutrition, describes the processes he employed, analytical and statical, and the results he obtained—these for comparison—under his ordinary course of diet. The trial he made of albumen occupied ten days uninterruptedly, and that with starch as long; but in the instance of gum he was obliged to abridge the time to five days, from the illness which this substance occasioned. The albumen used was from the serum of bullock's blood, after boiling, and was ingested consequently in the coagulated form; the starch was prepared from maize, and was that kind commonly known in the country as corn-starch; the gum was pure gum-arabic, and was ingested dissolved in water. Interesting as are all the details, we must restrict ourselves to his *résumé*—his conclusions. These, as he thinks, fairly deducible from the experiments, and applicable to the human subject, are the following:

"1. That albumen may be assimilated into the system in such quantity as to furnish a sufficiency of both nitrogen and carbon to the organism.

"2. That under the use of an exclusively albuminous diet, the nitrogenous constituents of the urine are increased over the ordinary average amounts, though not in proportion to the quantity of albumen absorbed into the circulation.

"3. That either some other means than the urine exist for the elimination of the nitrogen from the system, or the excess (over two-thirds) is retained in the organism, even when the body is rapidly decreasing in weight.

"4. That the continued use of albumen as an article of food increases the proportion of this substance (and of fibrin) in the blood, and in a short time causes it to appear in the urine.

"5. That while pure albumen cannot be regarded as of itself adequate to supply the several wants of the system, there is no reason why, when associated with suitable inorganic matters, it should not support both life and health.

"6. That starch can be assimilated by the absorbents in more than sufficient quantity to sustain the respiratory functions.

"7. That under its use the nitrogenous constituents of the urine are very much reduced in amount, even below what would probably occur during inanition; and that although starch is not capable of nourishing the tissues, it is yet serviceable, aside from its heat-producing power, in retarding their destructive metamorphosis.

"8. That the continued use of highly amylaceous food causes the appearance of sugar in the urine.

"9. That under the use of such aliments the nitrogenous constituents of the blood are diminished, and the carbonaceous increased.

"10. That gum is altogether incapable of assimilation, and therefore possesses no calorifacient or nutritive power whatever, but is, on the contrary, a source of irritation to the digestive organs.

"11. That, in consequence of the above fact, the solids of the urine, during the immediately-preceding researches, were entirely derived from the waste of the tissues of the body, and the carbon exhaled by the lungs from the consumption of its fat.

"12. That gum, when exclusively used as food, from the irritation it causes in the intestinal canal, and the fact of its non-assimilation, induces more constitutional disturbance than either starch or albumen, and that under a similar condition starch is more productive of ill consequences than albumen."

The sixth memoir is "On the Alterations induced by Intermittent

Fever in the Physical and Chemical Qualities of the Urine, and on the Action of the Disulphate of Quinine." The remarks with which he introduces it are so just and so widely applicable that we are tempted to quote them. He says:

> "We know but little at present concerning the modifications produced by diseases in the function of regressive metamorphosis of tissue; and yet it is very obvious that here our observations, if properly directed, can hardly fail to lead to results of very great importance. The exhalations from the skin and lungs, the urine and fæces, are so many points upon which to concentrate our efforts; and by carefully studying these several excretions, a vast amount of knowledge may be obtained relative to the pathological actions going on within the system. The facility and exactness with which such injuries can be prosecuted is only beginning to be perceived, and a rich harvest is reserved for those who will devote themselves to this field of labour."

Dr. Hammond was the subject of the attack; the form of the fever was tertian; it was arrested by two portions—ten grains in each—of disulphate of quinine taken on successive days after the first paroxysm. We must refer the pathologist to the work itself for the results, barely mentioning that in this case, during the paroxysm, the uric and phosphoric acids were much increased in amount, and the urea and chlorine greatly diminished; whilst, during the intermission, and after the use of the quinine, these constituents were re-established in their normal proportions.

His next memoir, the seventh, is "On the Injection of Urea and other Substances into the Blood." This research, he remarks, was undertaken chiefly with a view to test the accuracy of Frerichs' explanation of uræmic intoxication, according to whom the blood-poisoning in Bright's disease is not owing to accumulation of urea in this fluid, but to its conversion, through the agency of a ferment, into carbonate of ammonia.

In the trials he made, Dr. Hammond used both simple urea and urea combined with vesical mucus, also carbonate of ammonia, sulphate of potash and nitrate of potash. The following are his conclusions:

1st. That none of these substances injected into the bloodvessels of sound animals cause death, with the exception of nitre.

2nd. That the nitrate thus introduced is speedily fatal.

3rd. That death results from the other substances thrown into the circulation of animals, the kidneys of which had been before extirpated.

4th. That in neither case does the urea when introduced directly into the circulation become converted into carbonate of ammonia.

The eighth memoir is "On the Action of Certain Vegetable Diuretica." These were squill, juniper, digitalis, and colchicum. The trials were all made on healthy men, and were not extended beyond three days. The conclusions he arrived at were, that neither of them, with the exception of colchicum, had any depurating effect on the blood; that the three first, on the contrary, diminished the quantity of the solid matter eliminated by the kidneys, and consequently, are so far injurious.

The ninth memoir, entitled "Experimental Researches relative to Corroval and Vao: two New Varieties of Woorara, the South-American Arrow-poison." This, the joint labour of Dr. Weir Mitchell, of Philadelphia, and of the author, is deserving of all praise for completeness, and, as well as we can judge from the details, for care and accuracy in the experiments, and justness of reasoning and deduction. According to his method, he prefixes a brief history of the labours of other inquirers on the poison, previously and longest known, woorara. From the experiments on corroval, the conclusions arrived at are the following:

> "1st. That it differs essentially from any variety of woorara hitherto described, both in its chemical constitution and physiological effects.
>
> "2nd. That it acts primarily upon the heart, through the medium of the blood, producing an arrest of the action of this organ.
>
> "3rd. That the annihilation of voluntary and reflex movements is a secondary result of its action, depending primarily upon the discontinuance of the function of the heart.
>
> "4th. That it acts upon the nerves from the periphery to the centre, and abolishes both the sensory and motor functions.
>
> "5th. That it destroys muscular irritability.
>
> "6th. That it paralyses the sympathetic nerve, this being one of its primary effects.
>
> "7th. That it is absorbed both from the intestinal canal and the skin of frogs.
>
> "8th. That its poisonous qualities are due to an alkaloïd hitherto undescribed."

From the experiments on Vao or Bao, the deductions arrived at are given in fifteen propositions, admitting of the inference that vao is merely a weaker variety of corroval, and that the apparent difference in the effects produced by the original extracts is due merely to a difference in their strength.

The physical and chemical characteristics of these two substances are treated of in a distinct memoir, the tenth. From the experiments made, then limited by the smallness of the quantity operated on, the inference drawn was that the toxic principle of both is a peculiar alkaloïd, for which the name of corrovalia is proposed, and which, accounting for the difference of strength, was found to exist in a larger proportion in corroval than in vao. The power of the alkaloïd is very remarkable, "equalled by few, if any, substances hitherto known to man: one minim of a solution formed of one grain of the alkaloïd and one hundred minims of water, killed a small mouse in five minutes, when inserted under the skin; and five minims similarly placed under the skin of a medium-sized rabbit occasioned its death in five minutes. No antidote has yet been found for this poison; even artificial respiration, which seems to have some efficacy in poisoning by woorara, appears to have no power over the more deadly corrovalia. The plant or plants from which corroval and vao are obtained are yet unknown to the botanist. Both were brought from Rio Darien, in the province of New Grenada, South America; and, it would appear, had never before been noticed by those who have written and experimented upon the subject; nor is it yet known how either of them is prepared; that they are entirely vegetable seems to admit of no doubt.

The eleventh memoir is entitled, "Experimental Researches relative to a Supposed New Species of Upas." It had been obtained in Singapore, but without any information respecting its history. The quantity of this poison at the disposal of Dr. Hammond was too small to allow of a thorough investigation of its properties. From such experiments as he was enabled to make, the conclusion he finally came to was that this upas is not, as he first supposed, the upas tiente, the well-known arrow-poison of the Eastern Archipelago, but altogether a different species and hitherto undescribed. One of its remarkable and distinctive properties is that it takes effect when applied to the skin or mucous membranes. In consequence of the tetanic convulsions it produces, ending in the cessation of the heart's action, Dr. Hammond considers it more allied to strychnia than to corrovalia, and that it is probably a compound of two antagonistic principles, one acting on the heart, arresting its motion, the other on the voluntary muscles, occasioning tetanus. For the minutiæ of the research we must refer to the memoir. The results are exceedingly interesting compared with those described in the preceding memoir, and this not only on account of the intensity of action of the poisons on the living being, comparatively inert on dead animal matter, but also from the different morbid phenomena which they produce when taken into the circulation, and yet without materially affecting the blood which is their vehicle.

In the two memoirs with which the volume ends, Dr. Hammond resumes the inquiry respecting the "Diuretic action of Colchicum," and "Uræmic intoxication." The results of his additional experiments on colchicum are in accordance with his preceding, and confirmatory, he thinks, of his former conclusion that this medicine is a true depurator of the blood, and, in consequence, beneficial in those blood-diseases, gout, and rheumatism. Whilst he admits with Dr. Garrod that colchicum does not increase the excretion of uric acid, he does not adopt the doctrine of the latter, that this acid in excess in the blood is the cause of a paroxysm of gout.

His experiments on "uræmic intoxication" admit, he thinks, of the following conclusions, all of them, we may remark, important and especially interesting in a pathological point of view:

> "1st. That the injection of urea in limited quantity into the blood of animals produces a certain amount of disturbance in the nervous system, similar in its symptoms to the first stages of uræmia; but that this condition even disappears if the kidneys are capable of so depurating the blood as to eliminate the toxic substance.
>
> "2nd. That urea, when introduced into the circulation in larger quantity than can in a limited period be excreted by the kidneys, induces death by uræmia.
>
> "3rd. That by ligature of the renal arteries, or removal of the kidneys, the elements of the urine being retained in the blood, render this fluid unsuitable to the requirements of the organism, and, consequently, induce a condition of system not essentially distinguishable from the uræmic intoxication of Bright's disease, or that caused by the direct introduction of urea into the blood.

> As, however, was pointed out by Bernard and Barreswil, so long as the urea, or the products of its metamorphosis, are discharged by the stomach or intestines, uræmia does not take place, but that when these channels become closed, convulsions and coma are produced, and death soon follows.
>
> "4th. That the introduction of urea or urine into the circulation of animals, the kidneys of which have been ablated, shortens the life of such animals, as Frerichs and others have already shown.
>
> "5th. That there is reason to believe that the urine, as a whole, is more poisonous than a simple solution of urea; for in those cases in which urine was injected into the blood, the amount of urea thus introduced was much smaller than that previously thrown in, in a pure state, and yet symptoms of as great intensity followed.
>
> "6th. That urea, or the elements of the urine, as a whole, induce such a condition of the nervous system as strongly predisposes to congestion and inflammation of the viscera, especially of the lungs, pericardium, and spleen.
>
> "7th. That urea, when directly injected into the blood, or suffered to accumulate in this fluid by extirpation of the kidneys, deranges, in some manner, the process of sanguification, so as to disturb the normal relation of proportion existing between the white and red corpuscles, and either to hasten the decomposition of the latter, or to interfere with the due removal from the blood of such as are broken down and effete.
>
> "8th. That there is no reason to suppose that, under the circumstances specified, urea undergoes conversion into carbonate of ammonia, but that, on the contrary, there is sufficient evidence to warrant the conclusion that no such process ensues. The fact that in the foregoing experiments a larger amount of urea was generally found in the blood taken from the body after death than in that abstracted during life, is of itself conclusive against such an hypothesis."

We cannot, we think, do better in finishing our brief analysis of this able work than by offering another quotation, and by recommending the principle on which it was written, for the guidance of other inquirers in the noble and fruitful field of physiological research, with the addition of the excellent saying of the Abbé Fontana, that—"Those only who observe and experiment make mistakes; those only who do neither never err." The author's words in harmony with this are:

> "To say that I entered upon the inquiry without certain preconceived opinions would be far from correct. That such views as I had conceived have, however, blinded me to the truth, or warped my judgment of things as they actually were, I do not believe. Theories are true but for the time being, and physiological hypotheses are even more ephemeral than any others. We should therefore be prepared to yield our convictions without regret, when they do not accord with the results of experiments better devised and more accurate than our own, for only by so doing can we entitle ourselves to be considered useful labourers in the fields of science."

REVIEW IV.

1. *Seventeenth Report of the Commissioners in Lunacy to the Lord Chancellor.* Ordered by the House of Commons to be printed, 9th June, 1863.—pp. 176.
2. *Fifth Annual Report of the General Board of Commissioners in Lunacy for Scotland.* Presented to both Houses of Parliament by command of her Majesty, 1863.—pp. 226.

THESE annual blue-books present the usual particulars respecting the doings of the Commissioners, the condition of the asylums for the insane (public and private), and the statistics of lunacy. As on former occasions, we have again to remark on the greater value and completeness of the Report issued by the Scottish Commissioners. The English board appear wedded to their old style, and fail in presenting nearly so full an account of the state of lunacy in this country as their fellow-Commissioners do for Scotland. For example, we have no complete statistics of the number of the insane in this country prepared by the Lunacy Board—a fact that has been overlooked by some writers who have not been at the pains of accurately comparing the official returns made in each country respectively. To this the Scotch Commissioners allude in their last Report:

> "An erroneous estimate," they say, "has been formed of the prevalence of insanity in Scotland, by instituting comparisons between the number of the insane in England and Scotland, founded on the official returns of the English and Scotch Boards of Lunacy. But, in making these comparisons, it has been overlooked that in the English returns no account is taken of private lunatics, resident with friends and boarded with strangers, who have not been reported to the Commissioners. In our former Reports, we estimated the number of this class in Scotland at 1887. We see no reason to doubt that if similar investigations were undertaken in England, a corresponding increase would take place in the known number of the insane of that country. In making such comparisons, it is likewise necessary to bear in mind that in Scotland the visits of the Commissioners are not limited to asylums and poorhouses, but extend to every house, wherever situated, in which a pauper lunatic is placed. Every parish in Scotland is thus visited, on an average, once a year, either by the Medical Commissioners or Deputy Commissioners; and there is, accordingly, good reason to think that the number of unreported pauper lunatics is much less in Scotland than in England, where no such parochial inspections are undertaken. We are of opinion, therefore, that any such seeming excess of lunacy in Scotland is not so much due to a larger proportion of insane in the population as to more copious and accurate returns." (p. 5.)

Comparisons are proverbially odious, and in the above quotation the comparison thus officially drawn of the manner in which the duties of a lunacy board, when rightly apprehended, are performed in England as compared with Scotland, is unfavourable to the Commissioners in the former division of the kingdom.

Indeed, it is a fact that cannot be ignored, that the interests and welfare of the insane are in England not watched over for nearly the whole number known to exist. The attention of the English Commissioners is pretty nearly absorbed in the supervision of those lunatics whose position is the most favourable. Their visitations of public and private asylums leave nothing perhaps to be desired, except in frequency; but the poor lunatics who are detained in workhouses are only visited by them at intervals of uncertain length, of from one to two or three years, whilst that large number referred to by the Scottish board in the quotation above, as distributed singly in cottages and private houses, are not (except when found lunatic by inquisition and inspected by the Chancery physicians) visited at all by any inspectors or commissioners. And yet every Report gives details of unfit lodging and improper treatment, and of illegal detention in workhouse wards and in private houses, which in the course of the year have been brought under their notice by others.

The official Reports themselves may therefore be adduced to prove the necessity for the visitation of all lunatics wherever and however situated, and of a like thorough investigation into their existence as that which brought to light so many as 1887 scattered cases in the comparatively small population of Scotland. The desirability of a general supervision will doubtless be conceded by the Lunacy Commissioners, but they are unable to undertake so extensive a charge. At the present time they are pressed by work at their central office, and, as we have seen, cannot make an annual visit to the various workhouses which contain insane inmates, notwithstanding the extended provision of the Lunacy Act, enabling any one of the honorary or of the paid Commissioners to visit alone. The obvious remedy is to increase the number of Commissioners, or, as a more economical proceeding, to appoint Assistant-Commissioners, as has been done in Scotland, to whom a very large portion of provincial inspection might be allotted, and great facilities be thereby afforded for the performance of the functions invested in the Commissioners themselves as members of a central board, occupied in licensing, in a heavy correspondence, in making investigations, and in some degree in acting as a judicial body. But, oddly as it sounds, the Commissioners, according to the statements of some of their members examined before the Committee of the House of Commons in 1859–1860, are averse to any additions to their number in any shape, whether of compeers or assistants. We were told they are such a happy family that they could not avoid quarrelling with any intruder, because as a novice unacquainted with their traditions and usages, he would probably interrupt their harmony, and, what was still more to be deplored, might prove an innovator. We commend this sagacious argument against rendering a board efficient to the consideration of our readers whose mental vision is not obfuscated by the cobwebs of office.

There is not much of interest to the general medical reader in the English Commissioners' Report. Several sad cases of violent death in asylums are fully reported, and were investigated with the greatest care. One case, happening in a private house, would furnish a valuable and impressive text for any one who may not go to the full extent of the non-restraint principle in treatment. From the account given, it appears that the patient was a tall, powerful man, labouring under acute mania, with symptoms of general paralysis. "Three, four, and even five men had great difficulty in controlling him. In the course of the four days of his residence in the asylum several desperate conflicts took place between him and his attendants, during which they frequently fell down together, and over the bedstead which was in his room. At four o'clock in the morning of the day upon which he died . . . . a most violent struggle took place between him and the attendants, by whom he was then fastened to the bed with a sheet and towels." Death took place between ten and eleven o'clock that same morning, and on examination of the body, various bruises and abrasions were found, and three ribs "fractured on the right side and six on the left. The heart was also extensively diseased." In the inquiry before a jury no undue violence could be proved to have been used, but "the Commissioners could come to no satisfactory conclusion with regard to" the conduct of the attendants, although the two senior of them were requested to resign. The Commissioners' conclusion was, that the treatment "had not been judicious, and that there had been a want of proper care and supervision;" for it appears that the superintendent did not see the case until making his ordinary round at half-past ten in the morning.

Death is attributed to the heart-disease and the fractured ribs consequent on the struggle in the early morning of the day on which the patient died; and the Commissioners evidently disapprove even the final expedient of confining him to his bed. He must be a thoroughgoing partisan of non-restraint, and willing to sacrifice everything to it as a universal principle of action, who will not regret that some means of mechanical coercion had not sooner been resorted to—supposing always that seclusion was found inapplicable or ineffectual, rather than that such a terrible struggle for mastery, and indeed for life, should have gone on from day to day in a room between the infuriated maniac and his attendants. It will be generally admitted as unfair to attendants to be so exposed to maniacal violence, and to have their passions so tried by prolonged resistance and desperate assaults; while it must be also granted that such conflicts are necessarily attended with danger to the life and limbs of the unfortunate patient. In hinting at mechanical restraint as expedient in the case, we necessarily assume that all other means of calming excitement had been tried in vain, and that seclusion, drugs, baths, and moral influence had failed.

This was just one of those cases which foreign physicians, in their criticisms on non-restraint as a universal expedient, represent as inevitable, and therefore a sufficient reason against such an extreme principle. To this objection it has generally been replied by our English advocates of non-restraint, that such cases were purely problematical, and that in the experience of the many asylums of this country in which that principle is fully acted upon, no maniacs not amenable to firmness and kindness—in short, to moral treatment as understood and practised in England—were met with, and that if such did occur in foreign countries, it was because coercion was not wholly laid aside, and because the true system of non-restraint was not comprehended. There is much truth in this reply, for none can deny the extraordinary extent to which the non-restraint principle can be carried out in well-ordered asylums; but that struggles between patients and attendants are comparatively frequent, the annual reports issued attest by the yearly catalogue of patients whose ribs have been found fractured before or after death, not to speak of slighter injuries. We must, however, not be understood to imply that such struggles are the consequence solely of restraint not being used; on the contrary, we are perfectly persuaded that they will unavoidably happen, for insane patients are prone to violence, and attendants have not always moral control, and that in almost all cases where maniacal violence does occur, mechanical coercion should not be employed to control it. We argue therefore on the same side with foreign objectors to non-restraint as a universal principle, only so far as the recognition of very exceptional cases of maniacal violence is concerned, believing that humanity will be best served in such supposed instances by a departure from so inexorable a rule of management.

The State Criminal Lunatic Asylum is ready to receive patients, but the Commissioners find that a removal of all the registered criminal lunatics to it is unnecessary and undesirable. The number to be removed bears a small proportion to the aggregate, on account of the trivial nature of the offences of the majority, and other circumstances with regard to them mentioned. The Commissioners indeed

> "Have been forcibly impressed with the impropriety and absurdity of treating a large number of the patients confined under Secretary of State's warrants as of the criminal class, or otherwise than as ordinary lunatics, from whom they cannot, on any sound principle, be distinguished. We think it most desirable that the Secretary of State be empowered to transfer such patients from the criminal to the ordinary pauper class."

Eight hundred and seventy-seven criminal lunatics are enumerated as detained in asylums, hospitals, and licensed houses in England; but the number of those confined in convict prisons and gaols, the Commissioners are unable "to state, even approximatively." This last is a remarkable admission on the part of a Board which, if properly constituted and empowered, should, from its single special function in the State, be charged with the supervision of all lunatics in the kingdom.

In Scotland the only criminal lunatics alluded to by the Commissioners are those confined in the general prison at Perth, and only thirty in number. We cannot, in face of the large number enumerated in England, suppose those thirty to be the whole of the criminal lunatics in Scotland, yet no notice is given of any such inmates in the very complete lists of patients in asylums of all sorts, and we presume therefore that their existence is either not taken into account, or else that the Scotch criminal law operates in a different manner on the destiny of those guilty of crime who are at the same time of unsound mind. The English Commissioners have arrived at the conviction, which counsel decides to be legal, that registered hospitals may receive voluntary boarders, upon some sort of bond or agreement, such boarders not being actually insane, but "conscious of a want of power of self-control, or of the addiction to intemperate habits, or fearing an attack or recurrence of mental malady, and being in all respects free agents." This privilege will be a great boon to many such much-to-be-pitied individuals, who could or would not enter an asylum as registered lunatics subject to the restrictions necessary for the insane.

The Scottish Commissioners remark on the greater proportion of female pauper lunatics than of male, but instead of inferring therefrom, as is commonly done, the greater proclivity of the female sex to insanity, they go into the question, and show that the excess of female pauper lunatics is due simply to the larger source from which the supply of such lunatics is derived. "When the inquiry is restricted to private patients, this opinion derives considerable support, as it is then found that the proportion of male to female lunatics placed in asylums is as 100 to 106: the proportion of males to females in the general population being 100 to 111."

Both the English and Scotch Commissioners still concur in condemning workhouses and workhouse lunatic wards as places of detention for the insane, and in exposing the fallacies of Boards of Guardians respecting the great economy of such accommodation; and we would especially recommend for perusal the remarks of the latter Board on the whole question of its expediency and supposed pecuniary advantages, at p. 47 *et seq.* of their Report.

We shall conclude our notice of these Reports by briefly analyzing the general statistics of lunacy in the two divisions of the kingdom, during the past year.

The summary of the English Commissioners shows that there were:

	On January 1st, 1862.			On January 1st, 1863.		
	Private.	Pauper.	Total.	Private.	Pauper.	Total.
In County and Borough Asylums	267	19,387	19,654	259	20,314	20,573
In Hospitals for the Insane	1890	262	2152	1928	306	2,234
In Metropolitan Licensed Houses	1437	695	2132	1448	826	2,274
In Provincial Licensed Houses	1656	605	2261	1706	552	2,258
Total	5,250	20,949	26,199	5841	21,998	27,339

Comparing the totals of the two years, we find an increase of 1140 among those placed in asylums, of which by far the largest proportion, viz. 927, has taken place during the year 1862 among the pauper inmates of county and borough asylums. The number of pauper lunatics has also increased greatly in metropolitan licensed houses, viz. from 695 to 826, or a total of 131; and in a less degree in hospitals, viz. from 262 to 306, or 44; but on the other hand, there is a diminution of 53 pauper lunatics in provincial licensed houses. The total increase of pauper lunatics during 1862 was, therefore, 1049 of the whole increase of 1140 that had taken place in all the establishments, public and private, for the insane in England, or within 91 of that number.

The mistake is often made of quoting these totals of the summary in the Commissioners' Reports, as representing the actual number of the insane in England, whereas they represent only about one-half of those coming under the cognizance of public officials. Thus, the Commissioners tell us, at p. 22, that on the 1st of January, 1862, there were 8803 pauper lunatics in workhouses; and from a foot-note to that page, we gather that there were also 4463 others living as single patients with relatives, or boarded out with strangers. By adding, therefore, these numbers to the 20,949 enumerated as present in recognised establishments for the insane, we obtain a total of 34,215 pauper lunatics known to the Lunacy Commissioners and the Poor-Law Board on the 1st of January, 1862. To compare the total numbers of the insane in England with those reported by the Scottish Commissioners, we have to add to the 34,215 pauper lunatics found in England the number of private cases at the same date, viz. 5250, which gives a total of 39,465 insane inhabitants in England and Wales on the 1st of January, 1862; and to make an approximative estimate, we may assume this total to have reached 42,000 on the 1st of January, 1863.

Turning to the Scotch Report, we have the statement of the whole number of lunatics in Scotland, put before us at a glance in the table appended below; and this, we may observe, is compiled from more careful and accurate returns than we possess in England; for on comparing the returns of the English Commissioners with those of the Poor-Law Board, we find many discrepancies between them, and there is an evident want of concurrence and harmony between those two public offices, or otherwise we should not be able to point to the discreditable fact, that in a country like England we cannot state with accuracy even the actual number of chargeable pauper lunatics. To return, however, to the table setting forth the state of lunacy in Scotland on the 1st of January, 1862, there were—

	Private.	Pauper.	Total.
In Public and District Asylums	800	2020	2820
„ Private Asylums	231	690	921
„ Poorhouses	...	838	838
„ Private Dwellings	21	1741	1762
Total	1052	5289	6341

The Scottish Lunacy Commissioners make no return of the number on January 1st, 1863, and we cannot therefore ascertain the increase of lunacy in Scotland during 1862, as we were able to do for England. However, the increase is given during 1861, the Commissioners showing it to have been only 39 in the case of private patients, and of 32 in that of paupers—a remarkable contrast to what we have found to take place in England. "To what cause," say the Commissioners, "this comparatively slow growth of pauper lunacy is to be ascribed, we are not prepared to offer an opinion; but we scarcely venture to hope that the result of future years will be found as favourable."

The totals arrived at enable us to compare the relative prevalence of lunacy in England and Scotland. The population of the latter country was 3,002,294, and that of England 20,061,725, according to the Census of 1861. The relative proportion of the population in the two countries is therefore as 3 to 20; and the number of lunatics in England should, supposing lunacy equally prevalent in it as in Scotland, have been on January 1st, 1862, at least 6½ times greater, or 6341 × 6·5 = 41,216·5; whereas we found it = 39,465. After allowing for the more accurate enumeration in Scotland, lunatics would still appear to bear a somewhat larger proportion to the whole population in that country than in England.

In the returns of the Poor-Law Board there is a separate column for the enumeration of idiots, as distinct from lunatics. It would be very desirable for the Lunacy Commissioners, both of England and Scotland, to prepare tables indicating the number of idiots, whether placed in asylums or elsewhere, as well as to exhibit the number of idiots added to the population annually. The much-needed statistics of idiocy might then be determined, and the relative prevalence of idiocy and lunacy in England, and in England as contrasted with Scotland, be ascertained. Some conclusions, moreover, could eventually be arrived at on the question of the mental degenerescence of the people. Similar statistics are collected in various continental States; and it would be only creditable to collect them in this country, where social science pretends to so much cultivation and patronage.

REVIEW V.

On the Nature and Treatment of Gout and Rheumatic Gout. By ALFRED BARING GARROD, M.D., F.R.S., &c. Second Edition, enlarged and carefully revised.—*London*, 1863. pp. 618.

WE were glad to see a second edition of Dr. Garrod's work on Gout announced, affording proof that the great ability and original research displayed in the first have been well appreciated by the profession for whom it was written. Doubtless there are other circumstances quite apart from the merits of the publication which have promoted the demand for the book, as we know, and as is so well known, that mere originality of research and ability in an author will rarely insure success to his writings unless there be some elements in them distinct from science to render them attractive.

Now, of all the diseases to which man is liable, gout has peculiarities which have always made it one of more than ordinary interest. It is the special disease of man, and of civilized man, and is unknown, as far as we are aware, to the brute-animal, and to races of our own kind leading a rude life, but little different from that of the brute. Moreover, as far back as authentic history extends, now more than two thousand years, we have a certain knowledge of the occurrence of this disease, of its attacking persons in the same condition of life as at present, whether Greek or Roman, fixing on the rich, the highly-gifted intellectually and bodily, and sparing the poor and hard-working class of society: and then, as now, having so much the same symptoms, the same hereditary tendency, that there is no room to question the identity of the malady; and so far tending, collaterally, to prove that the constitution of man within the limits of the time specified has undergone no change. And when we consult the literature of the subject, which is vast, beginning with the writings of Hippocrates, we have further a proof, if proof were required, in the great similarity of reasoning on the morbid phenomena of gout in the several authors who have treated of it, that the mind, like the constitution of man, is much the same in quality and in power of thought in all ages; for it is curious to observe how nearly allied is the pathology of the disease as discussed of old in the infant stage of medicine, and at the present time in its advanced state, such as we find it in this work of Dr. Garrod.

These are a few reflections which have occurred to us whilst reading our author's introductory chapter, in which he gives a brief historical sketch of the labours of his predecessors.

Dr. Garrod informs us in his preface that this second edition is enlarged as well as carefully revised, and that the additions are chiefly of a practical character derived from a wider experience. After a careful perusal, we are satisfied of the correctness of the statements, and that, compared with the first, the work now before us is in every way of enhanced value.

The peculiar feature of Dr. Garrod's treatise, that which imparts to it so much interest and value, is the theory which it develops of the nature of gout, using the term theory in its best acceptation—that is, as an induction from, or a generalization of facts. Before he entered on his researches, the idea, the hypothetical idea, was common, handed down, as we have before observed, from a remote period, that gout has its special *materies morbi*, and that its morbid matter and the matter of urinary calculi are similar. Thus Sydenham threw out the conjecture that "calculus itself may be a part and parcel of the morbific matter" of gout; and later—viz., in 1793—Mr. Murray Forbes, as noticed by Dr. Garrod, made the happy conjecture that uric acid exists in the blood. He says, "Its frequent deposition in different parts of the body affords indisputable testimony of its being contained in the general fluids," and grounded on this, he asks, seeing that its deposition occurs in gout, whether gout may not be a consequence of its redundance. The notion of the analogy of the two diseases, gout and gravel, was not a barren one; it had a practical issue, and support was given to it by the results of treatment, some of the favourite remedies in their day for the one ailment being in repute for the other, especially the alkalies and magnesia. A further step in advance was made when Dr. Wollaston, in 1797, ascertained by the analysis of gouty concretions that they, in common with that kind of urinary calculus which is most frequently met with, are formed of uric acid, differing only from the matter of the calculus in being united with soda, one of the ingredients of the blood. After this discovery, the opinion more and more prevailed, that uric acid is essentially connected with gout. Dr. Garrod refers to Sir Everard Home, to Dr. Parkinson, and Dr. Wollaston, as holding this view, and to Cruveilhier and Dr. C. Petit among foreigners as maintaining it. Yet, however ingenious and plausible was the doctrine, it did not compel assent; it lay open to objection, and by some pathologists was considered erroneous. Thus, Dr. Durand-Fardel, writing in 1854, when he appears not to have been acquainted with Dr. Garrod's researches, denounces Cruveilhier's hypothesis, that the urate of soda is the material cause of gout, as a grave error in pathogeny, adding:

> "Admettons que la goutte soit une maladie *cum materia;* la matière y doit être considérée, ainsi que dans tant d'autres affections, comme l'effet et non point comme la cause de la maladie. L'urate de soude n'est pas plus la cause de la goutte que les mucosités ne sont la cause du catarrhe, l'exsudation plastique la cause du croup, les pustules la cause de la variole."*

It was in 1847 that Dr. Garrod ascertained, by chemical examination, that uric acid, in the form of urate of soda, is contained in the blood, normally in a minute quantity in healthy blood—abnormally, in excess in the blood of persons labouring under gout.† And all his after inquiries have proved that the excess of this salt in the blood is an essential circumstance of the disease, a *sine quâ non;* and so much so, that in doubtful cases, its presence or absence has enabled him to make a true diagnosis, one he holds to be infallible.

We owe to Dr. Garrod's chemical skill an easy method of detecting uric acid when in any excess in the blood. This method having been already described in our notice of the first edition, need not be repeated here. Fortunately for diagnosis, it is in the serum that it is to be looked for, and the serum produced by a blister will answer the purpose.‡

It is on this central fact of the presence of uric acid, and in excess, in the blood in gout, that Dr. Garrod's theory hinges. Its discovery is a striking example of the connexion of the sciences. Were we to make choice of an instance to show how pathology may be benefited by chemistry, we should have difficulty in finding a better. This theory, like every sound theory, is, we think, consistent in itself, and adequate for the most part, so far as the subject of morbid action is concerned, to account for the phenomena, and more than that, to afford a principle of rational medical treatment. It is interesting to see how science, how exact knowledge, of which theory is a representative, sharpens sense, and conduces to minute and accurate observation. This is well displayed in the account which Dr. Garrod gives of the symptoms of the disease, and in his description of its morbid anatomy. He has been the first to point out the occurrence of minute concretions of urate of soda in the external ear of persons troubled with gout;—an incident this so far from rare, that he has met with it, he states, in a majority of cases—even a single one, he assures us, has often enabled him to diagnose the disease when other symptoms had left room for doubt. Pain in the ear had by former authors been noticed as an occasional accompaniment of gout; but the cause had been overlooked, and it had attracted little attention, as from the situation of the deposit in the fibro-cartilage, and the yielding nature of the tissues of the helix, it seldom gave rise to acute pain, rarely to more than to a feeling of slight uneasiness.

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* Traité des Maladies des Vieillards, p. 838.

† We may here observe that, like Dr. Garrod, we have been unsuccessful in detecting uric acid in the blood of the common fowl, the urine of which consists mainly of urate of ammonia; affording thus, as he points out, proof of the great eliminating power possessed by the kidneys of the bird; and, we may add, we have sought equally in vain for it in the blood of the viper, the urinary excretion of which is of the same kind as that of the fowl.

‡ In the fluid effused from the action of a blister applied to a part with gouty inflammation, Dr. Garrod states he could detect no lithate of soda, and he attributes its disappearance to its destruction by the inflammatory action. May it not have been owing to another cause—the deposition of the urate in the part affected?

In the description of the morbid anatomy of gout, the information afforded is as satisfactory as it is exact, and remarkably in harmony with the theory. Here, again, the ancillary use of chemical analysis is well shown in the distinction which it enables us to make between true gouty concretions of urate of soda and the false, consisting chiefly of phosphate of lime, which by some authors have been erroneously associated with gout. The results Dr. Garrod arrives at are, that the gouty urate is never found deposited in any of the viscera, with the exception of the kidneys; never, so far as is yet accurately ascertained, in the liver, or spleen, or brain, or intestines, or heart, or in the course of the great bloodvessels, or in bone; only in cartilage and tendon and fibro-cartilage, and in these interstitially—all of them parts supplied with little blood, and consequently of low temperature—circumstances in themselves favourable to the separation of the lithate from its solution, and conducive also from the resistance offered by these tissues to pain from distension. The details of the morbid anatomy are well illustrated by numerous woodcuts and coloured drawings, some of the most valuable of which are microscopical. We would suggest for the author's consideration, whether it might not be for the advantage of the reader to have the latter, now interspersed in the text in an irregular manner without regard to reference, brought together, and appended to the end of the volume.

As regards the etiology of gout, that too, we think, harmonizes well with the theory. Its causes appear to be those chiefly which promote the generation of the urate, such as high living, in which animal food forms a large portion of the dietary, and strong wines and strong malt liquors a good part of the drink—the one yielding the nitrogenous element, the other, it would seem, promoting its accumulation in the form of the urate; such, moreover, as cold, or a cool, damp climate, in which, without active exercise, those parts of the body in which the deposition is found to take place—the feet and hands, the wrists and ankles, and especially that favourite seat, the great toe—are specially liable to reduction of temperature, with a sluggish circulation of blood in their vessels. On the contrary, the conditions of an opposite kind, such as a vegetable diet, abstinence from strong drinks, active exercise, a warm or tropical climate—these all seem to be preventitive of the disease.

The effect of a warm climate is very remarkable and instructive, and we can speak of it with some confidence from our own experience. During some years that we passed in the tropics, both the East and West, we do not recollect to have met with more than one instance of gout, and that in a gentleman well advanced in years, who had earned it in the convivial society of our metropolis when temperance was not the fashion of the day—we refer to nearly a half a century ago. In such a climate as that of the tropics, at a temperature varying from 70° to 80° and higher, the circulation in the extremities is always free, their temperature comparatively high; the skin is never chilled, and presenting a copiously exhaling surface, it rids the system of acid, with a more abundant desquamation than is experienced in a cold climate. These are conditions which seem equally favourable to the prevention of calculous matter in the urine, and of gouty matter in the joints—the balance between the formative and excretory function being on the side of the latter. Whether a more free exposure to light in a warm climate may aid or not a high temperature in checking accumulation of uric acid in the blood, may be deserving of inquiry. This we know to be a fact, that when the urates are exposed to a bright light, to the sun's rays, they are pretty rapidly converted into oxalates—a conversion, there is reason to believe, which sometimes takes place in the blood, Dr. Garrod having found, he states, this acid in the blood in many cases of gout. An idea prevails that within the tropics vegetable matter enters more largely into the dietary of the people than in cold or in temperate regions, and that this circumstance of diet may account for the exemption in question. The remark may be just as regards the coloured natives, but, as far as our experience allows us to offer an opinion, it is not applicable to Europeans resident, or to white Creoles when in affluent circumstances. Persons of this latter class make much use of animal food—none of their meals are without it; and though they are not commonly addicted to gross intemperance, they indulge, often pretty freely, in drinking malt liquors and wines of the stronger kinds.

In relation to the pathogeny of gout, the quality of the urine is very significant. The author's researches show in a satisfactory manner that the gouty diathesis is connected with a diminished excretion of uric acid by the kidneys, and its relief and cure by an increase of its excretion. In accordance with this, in the morbid anatomy of the disease, he has found almost invariably the kidneys more or less shrunk, of a weight below their average, and their tubuli more or less obstructed by uric deposit. He points out the error that the appearance of a large quantity of uric acid in the urine is indicative of an excess of it in the blood, the opposite of which being equally in accord with reason and accurate observation. In one of the worst and most inveterate cases of gout we ever witnessed, the absence of uric acid sediments in the urine, before, during, and even after a paroxysm, was remarkable, as was also the copious deposit of it in the joints.

As is well known, neither the early period of life nor the female sex is predisposed to gout. From Dr. Garrod's inquiries as to age, it would appear that the disease, except when inherited, is of very rare occurrence whilst the body is making progress in growth, the susceptibility to its attacks increasing with advancing age to full maturity of power up to the age of thirty-five years, when man commonly is most energetic and prone to commit the greatest excesses; and from that age to very advanced old age the tendency to it rapidly declines. As to sex, all experience also proves that the female is little predisposed to gout, especially during the stage of life that man is most predisposed to it—the age already mentioned of greatest vigour, which in woman is synchronous with a continuance with the menstrual function. Dr. Garrod attributes this comparative exemption partly to the special sexual function, and partly to less indulgence and exposure to the predisposing and exciting causes of the malady. So rarely, he observes, are women the victims of gout, that he holds the peculiarity to be one of the diagnostic marks between gout and rheumatism, the latter probably being more frequent amongst women than amongst men. We are disposed to think that there is in the female constitution, in her more perspirable skin, more dilute blood, and more dilute urine, another reason for her greater exemption; and in corroboration is her proportionally greater freedom from calculous complaints.

In our review of the first edition,* which was restricted to the more important and original portion of the work, especially the pathology and treatment and the varieties of gout, whilst the general doctrine enunciated by Dr. Garrod was received as satisfactory, doubts were expressed and objections made as to some of the details. These, we think, yet remain in force; such, to mention one instance, as the author's stated opinion that the gouty inflammation of a part is caused by a deposition of urate, the deposition preceding the inflammation, and yet persisting after the inflammation has subsided. Is it not more congruous, we would ask, to infer that the deposition is a result of the inflammatory action, that action occurring in the parts most liable to it, owing to the blood there being more impregnated than elsewhere with the urate at the beginning of a paroxysm, followed by increasing congestion and swelling until the salt is deposited, and thus accounting for the presumed destruction of the urate as the result of inflammation?

That there should be scope for objection here and there in such large theoretical views, comprising the whole history of a disease hitherto so obscure, excepting its etiology, that not including its *vera causa*, is no more than might have been expected; indeed, we are agreeably surprised that they command so much our assent. Few works that we are acquainted with display more laborious inquiry, more sound reasoning, and less addiction to speculation beyond the warrant of facts. We hold it to be an excellent example of research, and as such specially deserving of the attention of the advanced medical student. There is another class of persons to whom we would recommend it—those out of the pale of the profession, who imprudently consult medical books, impelled either by an apprehension of the disease, or by being already afflicted with it; and for this reason, that in the work of Dr. Garrod they will find the malady so well described in all its afflicting issues and its causes, whether predisposing or exciting, so well pointed out, that they will be fully indoctrinated in the formidable nature of the malady, and disposed to receive, not the congratulations, as of old was the fashion, but rather the condolence of friends on experiencing a fit of it; and further, whilst they will be put on their guard as to the bad habits and excesses on which the gouty habit so much depends, they will be enabled to see the folly of having recourse either to empirical medicines or to the empiric, or to becoming, except in the safe way of prevention, their own physician.

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* April, 1860, p. 419.

Before concluding, we will give two extracts from the work, which may be useful to those of our readers who may not possess or have an opportunity of consulting it:

Summary of Treatment.—"The management required in an ordinary attack of acute gout may be thus shortly summed up:

"The diet should be very light, and chiefly amylaceous, diluents freely used, but no alcoholic stimulants allowed, unless in exceptional cases.

"The medical treatment should consist in the administration of some simple alkaline saline, combined with a moderate dose of colchicum. If necessary, purgatives may be given, selected according to the habit and condition of the patient. In the majority of cases, this will be found to be all that is necessary, but in some instances certain modifications may be essential; for example, if there be plethora, the question of the abstraction of a few ounces of blood may possibly arise; on the other hand, if the vital powers are at a low ebb, and great vascular and nervous depression exists, it may be desirable to give ammonia in addition to other salines; at the same time the colchicum should be either omitted or used with the greatest caution.

"The only local application required in the majority of cases is cotton-wool, covered lightly with oiled silk; but now and then an anodyne may be advantageously used, and a small blister is occasionally of service." (p. 364.)

Diagnosis of Gout and Rheumatism.—"Gout cannot easily be mistaken for acute rheumatism when each malady is exhibited in its typical form. The subjects in which the diseases usually occur differ considerably, the first being most common in men after middle age, and in those who have lived freely; the second is at least as frequent in females, usually in the young and those of enfeebled powers. The characters of the diseases are also dissimilar. Gout, in its early seizures, ordinarily affects but one or two joints, and commonly the ball of the great toe, and the inflammation is accompanied with comparatively little fever, but with much local pain, œdema, and subsequent desquamation of the cuticle; the attacks are for the most part periodic, and gradually implicate a larger number of the joints. Rheumatism, on the contrary, generally affects many and large joints, even at first, and the upper extremities equally with the lower; the fever is likewise out of proportion to the local inflammation; and although the disease is apt to return upon re-exposure to its exciting causes, still no periodicity can be traced in its visitations. Gout may be further separated from rheumatism by the study of hereditary influence of the predisposing and exciting causes of the two diseases, as likewise of the great tendency to acute cardiac affection in the latter. Notwithstanding the great apparent facility in the diagnosis of typical cases, there are others very difficult to separate;

some such we have given in our description of acute gout, and these require different methods of analysis, involving a knowledge of their more intimate pathology.

"In gout we have proved beyond question that the blood is invariably impregnated with uric acid, and we are in a position equally to prove that in genuine acute rheumatism this principle is absent.* In an examination of forty cases of the latter disease, exhibiting all its typical peculiarities, the salient points of which will be found in the Appendix, no trace was discovered by the process before detailed.

"Furthermore, we have demonstrated that true gouty inflammation is always accompanied with the deposition of urate of soda in the structures primarily inflamed; and from the examination of the joints of numerous subjects who had suffered during life from acute rheumatism, I can testify that no such alteration ever ensues.

"In the chronic stage of the diseases, the separation of gout and rheumatism by the ordinary symptoms which present themselves, becomes in some instances a difficult task even to those who have given special attention to the subject. The forms most apt to be mistaken are chronic gout affecting both the large and small joints, and unaccompanied with any external deposits or chalk-stones, and certain forms of acute rheumatism, where the same structures are implicated. The external phenomena are then almost the same, probably but little febrile disturbance is present, the joints are but slightly swollen, and there is no redness of surface.

"A careful investigation of the history of the cases will do much: if the disease originally commenced in the great toe, and gradually became more extended in after attacks; if it began about the middle age, or bordering upon this, and the patient had lived freely, indulging in wine or malt liquors, then it is in all probability a case of gout. In many subjects the examination of the external ear will throw considerable light, and I have often been able to arrive quickly at a correct diagnosis by finding the presence of a spot or two upon the helix. . . . Instances occasionally occur in which the history, from peculiar circumstances, is of little value or cannot be procured, and where there are no external signs on which the diagnosis can be founded; these are rare, but any one frequently consulted on such subjects must have now and then felt a difficulty. In some of these cases I have derived much assistance from the examination of the blood or blister fluid; and a careful attention of the effects of drugs has often afforded much additional aid, as the inflammation of gout is relieved by the administration of colchicum in an infinitely more decided manner than that dependent on rheumatism." (p. 524.)

Not the least valuable portions of Dr. Garrod's work are its concluding chapters—those on rheumatic gout and rheumatoïd arthritis, especially the last, in which, as the name implies, there is a similarity of symptoms and dissimilarity of nature—an essential difference, and requiring necessarily a different mode of treatment. As the skill of the navigator is best displayed amidst hidden rocks and shoals, so is that of the physician in diseases such as those just mentioned, the obvious phenomena of which are not to be trusted to: these can be successfully diagnosed and treated only by means of such aids as advanced medical science affords, and which are described with much clearness and precision in our author's pages.

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* In Dr. John Davy's 'Physiological Researches,' of which a review was given in our last number, an instance is mentioned (note, p. 372), of urate of ammonia having been found in the blood of a man labouring under acute rheumatism.

REVIEW VI.

Clinique Médicale de l'Hôtel-Dieu de Paris. Par M. TROUSSEAU, Professeur de Clinique Médicale de la Faculté de Médecine de Paris, &c. Tome II.—*Paris*, 1863.

Clinical Lectures on Medicine delivered at the Hôtel-Dieu, Paris. By A. TROUSSEAU. Vol. II.

AMONG the multitude of medical publications issued of late years there are but few which are devoted to the details of clinical medicine. The task is, indeed, one of the most difficult that a medical author can undertake—a long familiarity with disease, an extended acquaintance with remedies and their application, and a readiness to accept and put to the test new views and proposals, all these are essentials to a successful bedside teacher, and with them must be combined in a high degree the faculties of observation and close comparison.

M. Trousseau has repeatedly indicated his possession of these acquirements, and we would welcome as a valuable addition to professional literature this second volume of his 'Clinique Médicale.'

Like its predecessor, noted in the pages of this Review for April, 1862, this book is devoted to the close consideration of distinct diseases, each one in a separate chapter. Throughout there exists a special charm for the practitioner of medicine; the work is essentially practical; we meet with no elaborate straining to reduce diseases to one system or another, no deep inquiry into the dim arcana of an eclectic pathology, but a simple statement is afforded to the reader of morbid processes as they have appeared to a thoroughly competent observer, with a clear enunciation of the best mode of dealing with each separate affection.

The volume includes forty chapters. We propose to direct attention to some which have, either from their subject-matter or from the treatment suggested, an especial claim.

Chap. XXXIX., and the first in this treatise, is devoted to the consideration of epilepsy. Occurring in the first instance not infrequently only during sleep, the attacks may for years even pass unnoticed. A symptom of their presence not very commonly recognised is thus referred to:

> "If you examine attentively an epileptic after his attack—better still on the following day—you will find very frequently on his forehead, on his neck, on his chest, little red spots resembling flea-bites, not disappearing under the pressure of the finger, and presenting all the characters of ecchymotic stains. . . . . The existence of these ecchymotic stains is a certain sign of an attack of epilepsy." (pp. 9, 10.)

Fear, so frequently stated by the relatives or by patients themselves to have been the immediate cause of the seizures, is so employed, in the author's opinion, merely to give an apparent external cause to a morbid condition far more often dependent on hereditary or other taint.

Epileptic vertigo may show itself in endless forms. Usually preceding, it is found occasionally to succeed on the severe convulsive type. This condition has been said to be the most frequent precursor of mental alienation—it would be more correct to speak of the sequence as more rapid rather than as being absolutely more certain. As a curative agent it is best to employ belladonna, given for some time, and in doses steadily increased. Metallic remedies—the salts of silver, zinc, &c.—have their value, and may be used either conjoined with atropine or alone.

The proposal to perform tracheotomy, which originated with the late Dr. Marshall Hall, is named only to be strongly reprehended.

The next chapter takes cognizance of epileptiform neuralgia, seated, as a rule, in the branches of the trifacial nerve, and rising at times to the most intense agony. By our own writers this most painful affection has been usually referred to the nerve-disorders especially due to organic intra-cranial changes. Distinct analogies are stated by M. Trousseau to link it with positive epilepsy. It is thoroughly incurable. Section of the affected nerve has sometimes given relief, transient only too often, while long-continued and very large doses of opium have seemed to be most deserving of trial.

Chap. XLI. Apoplectiform cerebral congestion, if not indeed thoroughly a misnomer, is yet a term given to a series of cases truly epileptic in character. They exhibit at their onset nervous or irregular convulsive movements of the limbs, vertigo more or less marked, and very commonly manifest some tendency to delirium. The local epileptic ecchymotic patches will be found on a close inquiry.

The medico-legal relations of epilepsy are then considered. It is not to be assumed that an epileptic is to be held irresponsible for acts of violence, because at times he is from his disease blind to the result of his deeds. To quote the author's own words on this matter:

> "Yet if this same epileptic has committed a murder without purpose, without possible motive, without profit for himself or any other person, without premeditation, without passion, in the sight and knowledge of all, consequently under conditions differing from those under which murders are committed, I have the right to affirm before the magistrate that the impulse to the crime has been *almost certainly* the result of the epileptic seizure.
>
> "I say *almost certainly* if I have not seen the attack; but if I have seen it, if eye-witnesses have noticed the complete paroxysm or the epileptic vertigo immediately precede the criminal act, I affirm then, *in an absolute manner*, that the prisoner has been impelled to the crime by a force which he has not been able to resist." (p. 59.)

Chap. XLII. Convulsions occurring in childhood may be *primary*, showing themselves at the commencement of some exanthem or acute febrile affection, or *secondary*, appearing at the close. These latter are of much the most serious significance. It must not be supposed that the convulsions are dependent upon the organic changes in the nerve-centres revealed by post-mortem examination; they rather precede, or indeed may be instrumental in the production of, the local congestions, &c., so found to exist.

Hereditary predisposition may induce their appearance, so too, and this we would note with especial satisfaction, the causal influence is recognised of "all these conditions which tend to weaken the whole economy," insufficient food, loss of blood, diarrhœa, &c.

In the treatment of this class of affections antispasmodics have a real value, such as chloroform, musk, belladonna, &c.; the older and severe plans of treatment have no praise. "I believe very strongly that the less we do, the better in general will be our course of action, and that our treatment ought to be expectant." (p. 99.)

In the convulsions of pregnant and lying-in women, depletion is condemned, and the use of such agents as chloroform, &c., is strongly insisted upon.

XLV. Chorea and its allied affections form the subject-matter of this chapter.

The close connexion existing between this neurosis and rheumatism is strongly pointed out; they are mutually interdependent, sometimes even appear to be convertible, and manifest a common tendency to induce endocardial mischief and deposits in the heart-valves.

The administration of the potassio-tartrate of antimony after the manner recommended by M. Gillette, is referred to as of possible advantage in those severer cases of choreic movement hitherto almost intractable. In some instances the free administration of opium is of material service, bearing in mind always that "when one administers opium, it is less the dose of the drug than the effects which one has to consider." (p. 160.)

Hysterical cough, that *bête noire* of daily practice, is referred by the author to the anomalous forms of hysterical chorea.

Chap. XLVII. The strange nerve-disorder known by our neighbours as the "maladie de Duchenne," is thus defined: "progressive abolition of the co-ordination of movement and apparent paralysis, contrasting with the healthy condition (*integrité*) of the muscular force."* (p. 181.)

Cases of this description, until lately grouped *en masse* with paraplegics, commence by symptoms of disturbance of the nervous system, the first being usually *pain* of a peculiar, severe, flash-like character, frequently returning upon changes of temperature, and as a rule occupying only circumscribed spaces. Subsequently are noted various paralyses, which may be either transitory or persistent, often indeed disappearing with great rapidity, and so conferring a false reputation on the medical treatment which may have been instituted.

Men are most frequently affected, and about the middle period of life. On first starting, the invalid totters, especially if he have been seated for some little time before attempting to move; then, after steadying himself by some fixed body, he is able to move on. The progression, at first slow and uncertain, becomes more hurried, and the legs are thrown forward hastily and convulsively, yet with all these symptoms, much muscular exertion in walking is quite compatible.

As the disease advances the violence and irregularity of the movements increase and weary out the powers of the patient before he has proceeded very far. Later on still, not one step is possible without falling; the unfortunate subject constantly sits or lies, and sloughs over the ischia or trochanters hasten the inevitable termination.

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* Duchenne, M.: De l'Electrisation Localisée, &c. &c. Seconde édition. Paris, 1861.

An aid to the diagnosis may be found in directing the patient to stand or attempt to walk with his eyes closed. The small amount of co-ordinating control still possessed is at once lost, and a fall, unless additional support be at once given, is inevitable. The malady may remain stationary for some time; one case is reported where it existed for twenty years; but it is never cured.

The consideration of this disease is again taken up in Chapter LXXIX., the last in the volume. Subsequent inquiries by several other observers are referred to, specially on the question of the anatomical changes found on post-mortem examination. Softening of the posterior columns of the cord and coincident atrophy of the posterior roots of the spinal nerves have usually been met with.

The opposition thus marked out between the results of disease and the teachings of experiment with regard to the functions of the posterior columns is fairly stated. As yet no adequate explanation is available.

Very little can be said on the question of treatment; MM. Charcot and Vulpian have obtained good results from the administration of nitrate of silver, so following the plan of M. Wunderlich, a remedy available on *à priori* grounds, since it has been proved of material use in other disorders of the nervous system.

Chap. XLVIII. Alcoholism has been by common consent transferred to the domain of physicians who study mental disorders alone. Yet there are in this pathological condition many points of interest for those who practise general medicine.

An especial reference is made to these cases in which, from slow saturation of the system with alcohol, delirium is impending to break out furiously when the individual shall have become the subject of any acute disease. Decisive signs of this character of delirium are as yet wanting.

Chap. L. enters on the consideration of Nocturnal Incontinence of Urine, dependent on an excess of tonicity and irritability about the muscular structure in the walls of the bladder, on this idea most satisfactorily treated by the slow and long-continued administration of belladonna, given for a time in full doses and then slowly diminished.

In those cases of nocturnal and diurnal incontinence due to the opposite condition, atony of the bladder, the preparations of strychnine are of most value.

Progressive Muscular Atrophy occupies the next Chapter, LI., known more familiarly under the name of wasting palsy. It is characterized anatomically by a diminution in size of the muscular fasciculi from which, as the disease gains ground, the transverse striæ are found to disappear, while fatty granules take their place.

The earliest symptom would seem to be that of local muscular weakness, limited at first to some one single member, increased by muscular exertion and under the influence of cold, the difficulty of movement ending in complete failure of power. Cramps are frequently met with; the muscles affected often become the seats of slight fibrillar contractions, so that they seem to be in continual movement; a sensation of slight shuddering is often complained of in consequence.

According to M. Duchenne, the disease depends not on any defect in nerve-power, but on the physical changes in the muscular structures. Since, however, even in the muscles positively altered in composition, some parts are to be found still possessed of their normal contractility, M. Trousseau leans to the opinion that the peripheral extremities of the nerves undergo some modification by virtue of which they become unable to determine the muscular contraction. Thus, then, the fault in the transmission of nerve-power would precede, and so determine the muscular degeneration.

The muscles first affected are usually those of the upper extremity; and attendant on the affection is well-marked wasting, so that one side, and this usually the right, may be almost deprived of the ordinary muscular prominences, while the other shall have preserved intact its normal outlines. An excess of fat may hide these ravages, but the electro-magnetic current readily decides which muscles are failing in, and which still retain, their functional power.

The rapidity of the course may vary materially, but the disease entails a certainly fatal termination. Examination after death discloses a marked diminution in the size of the anterior roots of the spinal nerves. This must not, however, in the author's opinion, be looked upon as the source of the disease, but rather as an anatomical change, consequent upon the loss of functional power in the muscles—a secondary, therefore, not a primary condition.

Chap. LII. This chapter enters on a question of interest, in these days of scepticism about the propriety of bloodletting, and treats of its value as a remedial measure in cases of cerebral hæmorrhage and apoplexy; only, be it understood, to negative the existence of the supposed advantages. The injury is done; no loss of blood will repair it. A softened patch of brain-substance has given way, or some cerebral vessel of greater or less size has poured out blood, the physical and intellectual powers have suffered accordingly; but these are not conditions to be treated by loss of blood, strict confinement to bed, and diminished supply of food. Far greater benefit will be obtained by the administration of food and careful nursing.

"I have the conviction that since I have adopted this expectant plan of therapeutics, my invalids have found themselves much better than those whom formerly I bled, kept on low diet, and confined to bed." (p. 274.)

Chap. LIII. Alternate hemiplegia, the face being paralysed on one side, the limbs on the opposite, is considered by M. Gubler[*] to indicate lesion of the pons Varolii.

"The special form of the hemiplegia is explained in the following manner. The lesion affecting the facial nerve in a point where it has accomplished its decussation, the paralysis is direct for the face, while it is crossed for the parts supplied with nerve-influence by the columns of the cord, the decussation of which is only accomplished in the anterior pyramids—that is to say, below the annular protuberance." (p. 281)

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[*] De l'Hémiplegie Alterne: Gazette Hebdom. Paris, 1856 and 1859.

For several reasons, stated somewhat at length, M. Trousseau does not agree with the reasoning brought forward.

Cerebral Fever, or, as with ourselves, Acute Hydrocephalus, occupies Chap. LV.

Among the symptoms referred to, some stress is laid on the existence of meningitic staining—"la tache méningitique ou cérébrale"—to be explained on the supposition of a material alteration in the vascular supply of the skin. It is described as a well-marked redness, rapidly developing itself upon a slight rubbing of the skin with any hard body, and remaining evident from ten to fifteen minutes. The redness is deeper and of more prolonged duration than that which follows on the same or even on more severe mechanical friction in ordinary febrile affections. It is specially found to exist on the anterior part of the abdomen, the thighs, and the face.

The three stages of the disease are recognised much in the order and as possessing the characters ascribed to them by English writers, the precursory conditions of the first stage being rather indicative of a deeply-implanted tendency to brain mischief, usually tubercular in origin, than as positively shadowing out the necessary sequence of acute hydrocephalus. Vomiting more or less persistent, and constipation, attended with headache of varying intensity, are the symptoms specially to be noted in this early stage.

The accession of the non-feverish period, with its singularly slow pulse, falling from 90 or 100 to 50 beats in the minute, gives rise to hopes of recovery. The repose contrasting so favourably with the previous restlessness makes the attendants on the child believe in the certainty of improvement. On this, however, follows deep somnolence, with a total disregard of external impressions, broken only by the mournful "cri hydrencephalique."

Retraction of the abdominal walls is a condition of especial value when the diagnosis between the disease before us and typhoïd fever is in question, since it is so strongly contrasted with the tumid and prominent abdomen of the latter affection. Peculiar to this form of encephalic seizure is mentioned the irregularity of the respiration, frequent inspirations, with a subsequent arrest of movement of the chest walls, not connected with the circulation, for it is coincident equally with the extremely slow pulse of the second as with the hurried heart-action of the third stage.

This third period is marked by a return of the feverish symptoms; the stupor becomes more profound; paralytic phenomena declare themselves; strabismus, dilatation of the pupil, and loss of power over the levator palpebræ are mentioned. Hemiplegia, more or less complete, may be found to exist, appearing sometimes to change its seat; for the limb sensitive to irritation one day is on the next visit uninfluenced by the same means—the explanation, however, being that both limbs are paralysed, the healthy limb of yesterday being the most thoroughly palsied to-day.

Convulsions declare themselves, and death closes the scene after, it may be, an apparent relief of all the symptoms. The older medical works contain published cases of cure. Since, however, the pathology and diagnosis of the disease have been more thoroughly cleared up they are no longer met with. In these touching lines M. Trousseau enunciates his opinion of the value of treatment:

> "Nevertheless, convinced as I am of my own impotence, I cannot decide to remain absolutely inactive; although, taught by long experience, I know that my efforts will be useless, I still try to contend with the disease. My intervention will, at least, have as its result that I do not ruthlessly snatch away all hope from those who surround the sick child, but that I sustain their courage, and do not leave to them the regret that I have done nothing to save him whom they confide to my care. But convinced, also, that too energetic plans of treatment weary out more rapidly the source of life, I endeavour to do the least possible ill, since I am powerless to do actual good." (p. 323.)

Chap. LVI. Cases of vertigo from stomach derangement are apt to be mistaken for those dependent on cerebral disturbance. They have this characteristic, that, throughout, the intelligence is neither impaired nor lost. The treatment should be suited to the relief of the coincident dyspepsia; on no account must it include any resort to low diet and depressing agencies.

Physicians, in treating disorders of the digestive organs, far too often fall into the error of dictating for their whole circle of dyspeptic patients that very *régime* which they have by experience learned to be best for their own requirements. The only sure guide to a correct diet scale must be found in the liking and the general condition of each separate patient.

For chemists and chemical theories the author has small reverence; the experiments of the laboratory and the experience of the bedside have little in common: "I know not how too thoroughly to put you on your guard against chemistry, which, principally in its application to therapeutics, leads to deplorable errors." (p. 378.)

To affections of the stomach and intestinal canal, including chronic gastritis, simple ulcer of the stomach, diarrhœa, &c., several chapters are devoted. The various plans of treatment found most satisfactory in the author's experience are noted in turn.

In Chap. LXII. attention is directed to the nursing and the weaning of children, and the bearing of the first series of teeth on these two physiological processes. The first teeth appear in groups, with a distinct interval of rest between the separate manifestations, and three or four even of these groups should be present before the child is weaned: "My rule is this—I wait for the complete evolution of the canine teeth before the children are weaned; I wait, then, until the child has sixteen teeth, without taking account of the age at which it has arrived." (p. 469.)

Chap. LXVI. Occlusions of the intestine—comprehending under this title those cases to which the terms of ileus, volvulus, iliac passion, &c., have been given—are due to external or internal causes. The causes *external* to the intestine are tumours, displaced viscera, and especially organized bridles of lymph, owing their existence to past peritonitis. *Internal* conditions, giving rise to obstructions, are, narrowing of the calibre of the gut from local disease, invagination, the lodgment of foreign bodies, &c. &c.

Out of four cases in which the author has, after the failure of other means, suggested the operation for artificial anus, two are stated to have recovered. The operation described is that practised by M. Nelaton, opening the lower end of the small intestine by an incision in the right iliac fossa. No reference is made to the procedure of opening the colon either by Littre's or Amussat's operation.

Hydatid cysts of the liver, Chap. LXVIII., are only to be opened surgically when adhesions exist between the hepatic substance and the internal surface of the abdominal walls. To induce these adhesions, M. Trousseau has recourse to "multiple acupuncture;" thirty or forty needles arranged in a frame are pushed through the integuments and subjacent tissues, so as to give rise to a localized peritoneal inflammation. Subsequently the requisite opening may be practised.

Chap. LXIX. Hydatid cysts of the lung have their primary seat in the pulmonary tissue. Secondary implication of the pleura may occur as the disease advances. As yet, the weight of experience is against any surgical interference in such cases.

Chapters LXX. and LXXI. are devoted to the consideration of diabetes in its two forms. The late researches of M. Bernard and M. Schiff are specially referred to. Our limited space forbids any attempt at the lengthened notice which these exhaustive papers so well deserve.

Chap. LXXII. entertains the strange disease, exophthalmic goître. In this affection, when fully developed, three well-marked morbid conditions are present:

1. *Protrusion of the Eyeballs*, more or less slowly increasing until the eyelids fail to cover them. Some disturbance of vision may be present; it is not, however, a necessary coincidence. Ophthalmoscopic examination in the hands of M. Wilhuisen has shown hyperæmia of the retina and pigment deposits, consequent probably on some change in nutrition.

2. *Enlargement of the Thyroïd Body.*—Of the two lobes the right is usually most affected, but the whole body, isthmus included, may become the seat of enlargement. The gland elements undergo a distinct hypertrophy, and the bloodvessels are also much enlarged. Two cases, indeed, have been reported where the thyroïd tumour was, from its expansion, supposed to be an aneurismal sac.

3. *Disturbance of Heart Action.*—Palpitations are much complained of; they form, indeed, a prominent symptom of the disorder; the heart acts regularly, but with much violence, and the beating is transmitted along the line of the large vessels; bruits de soufflet are not uncommon; but there is no necessary coincidence either of positive hypertrophy or of valvular disease. In the existence of a temporary hypertrophy, analogous to that of the organ in pregnant women, some belief is entertained.

The disease may exist even though there be no protrusion of the eyeballs; and the absence, also, of the thyroïd enlargement for a time will not invalidate the diagnosis. Sooner or later, the three distinctive characters will be manifested.

"In our opinion, the disease is a neurosis, with local congestions, having its immediate cause in a modification of the vaso-motor set of nerves." (p. 645.)

With reference to the treatment it is stated that iodine and its preparations, at first universally tried, have as generally been rejected. Pallor and œdema are constant attendants on the course of the disease, yet ferruginous preparations are not desirable. M. Trousseau recommends general bloodletting, the local application of cold, and the administration of digitalis, pushed to the verge of its poisonous action. In females, especial care should be paid to the soliciting a due menstrual discharge.

In the most severe accessions of dyspnœa, tracheotomy will probably seem to be the only means of saving life. If it is attempted, the operator must bear in mind the certainty and the dangers of an excessive hæmorrhage. M. Chassaignac's écraseur might be found available.

The affection of the supra-renal capsules, attended by discoloration of the skin, Addison's disease, is referred to in Chap. LXXIV.

Only two cases have come under M. Trousseau's care. The employment of tonic and supporting treatment affords the only hope of benefit, rarely hitherto realized.

After a notice, Chap. LXXV., of the white-cell blood leucocythæmia, the next section is devoted to that interminable medical puzzle—gout, in its various forms.

Both in local manifestation and in general symptoms the peculiar and specific characters of the disease are well marked. Analogies exist between gouty and rheumatic arthritis; but the differences are found in every direction—the mode of access, the parts affected with their subsequent local changes, and in the duration of the whole disease.

Several pages are given to acute and chronic gout. The deposits of chalk-stones are formed not during the attack of gout, as frequently supposed, but during the intervals of comparative ease. No necessary health-disturbance attends on these local changes.

Masked gout (*goutte larvée*) may be the determining cause of angina pectoris, or may show itself under the form of severe asthma. Attacks of these affections will frequently alternate with the ordinary arthritic mischief.

Visceral or anomalous gout not unfrequently results on the treatment of the ordinary articular affection by too severe or too prolonged using of remedies. Pulmonary or nephritic disorders are among the most common forms. It is of the most serious moment, since the organs affected are those whose functions are essential to the very existence of the sufferer.

Is active medical intervention during the acute access of gout necessary, or even advisable?

In accordance with the opinions of our great authority, Sydenham, the author deprecates active interference, and in these words:

"At the commencement of my practice I have endeavoured, like many others, to contend against the malady; now I stand with my arms folded. I do nothing, absolutely nothing, against the attacks of acute gout, especially when they seize an individual in the prime of life. More than once I have had to repent a departure from this inaction, and have learnt how perilous active intervention might become." (p. 739.)

Acute gout, heroically treated, will, in many instances, merge into the atonic cold and persistent form. If remedies are imperatively called for, then they should be used carefully and methodically. Colchicum with quinine is a valuable form of remedy, given in small doses towards the close of an attack, so as to moderate, not to extinguish the pain.

Of local applications, as generally employed, M. Trousseau says nothing. He has derived benefit from the subjecting of the parts affected to the influence of tobacco fumigation—employing the process in the absence of severe pain. Most probably the remedy acts by diminishing the increased local susceptibility.

The remaining chapters take cognizance of intermittent fevers and of rickets.

It would seem that not only does scrofula not entail rickets, but that the two diseases tend mutually to exclude one another. MM. Rufz and Guérin have deduced a similar law from their researches.

The whole work, in reflecting the daily teaching of its distinguished author, bears ample testimony to his unwearied zeal and assiduity. It will be heartily welcomed by those who embrace advances in medical knowledge as means—

> "Quibus hunc lenire dolorem
> Possint, et magnam morbi deponere partem."

REVIEW VII.

Ricerche sull' Anatomia Normale e Patologica delle Cassule Soprarrenali e Considerazioni sull' Apoplessia di questi Organi e sulla' Malattia dell' Addison. Per RAFFAELLO MATTEI, Professore di Patologia Generale alla R. Università di Siena.

Researches, Anatomical and Pathological, on the Supra-renal Capsules, with Remarks on Apoplexy in these Organs, and also on Addison's Disease. By RAFFAELLO MATTEI, Professor of Pathology in the Royal University of Siena. (From the 'Sperimentale,' 1863, pp. 28.)

THE charm of novelty attaching to minute investigations into the anatomy and pathology of these organs is not yet so far dispelled as to lead us to regard with an ordinary degree of interest observations so original as those which lie before us. As they are seldom found to be affected by disease, the obscurity which surrounds the function of these secondary viscera, and, perhaps also a certain preconception as regards their importance and activity, long led to their unmerited neglect, and, in the minds of most observers, to a partial indifference with regard to them, to which character of feeling the master-mind of Rayer alone formed for some time a single and brilliant exception.

To a like mental inertia may we not have a tendency to revert, now the chain of occurrence is discovered not to be constant in uniting two capital morbid phenomena which, by a most unexpected demonstration, were brought into pathological relation or dependence by the industry of another investigator whose name has deserved an honour which, in foreign publications and from foreign lips, is reflected back upon his country. Addison's disease, indeed, is but feebly illustrated by the original facts brought forward in the present pages; still the ultimate bearing of his labours in furthering inquiries in this field, may perchance lead beyond what we can at present either conjecture or understand.

In the anatomical structure of the supra-renal capsules, Signor Mattei recognises but two divisions or elements—an internal medullary portion of pearl-white colour, and an external cortical of a yellowish cast. The brown substance which is often found between the two, sometimes as a fine coffee-coloured line, and sometimes involving the cortical substance in a state of ramollissement or liquefaction, he regards as a common post-mortem change, varying according to character of the malady, state of the fluids, or external circumstances. In the normal state there is no cavity, and the term capsule, he thinks, had better be abandoned for that of gland or glandule, introduced by Winslow.

One of the most interesting and original parts of Signor Mattei's memoirs regards the weight of these organs, and we shall place before our readers his tables, which give the results in milligrammes:

Intra-uterine Existence.

Months.	Weight of the capsules in milligrammes.
Three	0·000392
Five	0·000490
Five	0·000490
Six	0·001374
Six	0·001472
Eight	0·001767
Nine	0·002454

Extra-uterine Existence.

Years.	Number of capsules weighed.	Median weight in milligrammes.
One	9	0·002074
From 2 to 10	11	0·002194
From 11 to 20	10	0·005000
From 21 to 30	10	0·004550
From 31 to 40	9	0·004340
From 41 to 50	11	0·004697
From 51 to 60	7	0·004901
From 61 to 70	14	0·003596
From 71 to 80	9	0·003886
From 81 to 90	2	0·005251
From 91 to 100	1	0·006184

These estimates of weight differ from those of Brown-Séquard, who gave the weight of the supra-renal capsules in the new-born infant as between 2 and 4 grammes, and placed it in the adult at 7 to 12 grammes. His conclusions, it seems, were not drawn from a large number of cases.* Those of Signor Mattei afford as the highest observation from 100 cases, the following—viz., 2454 milligrammes from a new-born infant, 7166 from an adult. He is far from denying the accuracy of Dr. Brown-Séquard, but he doubts the normal condition of the supra-renal capsule which gave the highest figures quoted by him.†

As regards morbid appearances, we believe we shall do most justice to our author in quoting the text:

> "I have now examined very attentively the morbid alterations found after death in the supra-renal capsules of 310 subjects. This number includes 2 cases of apoplexy of those organs, 1 case of cancer, 1 of adipose tumour, four of tuberculosis, 1 case of fibroïd tissue, accompanied with a not easily definable material, 1 case of atrophy, 1 of arrested development, many instances of unusual form and local adhesions, 4 cases of sanguineous engorgement, 1 case of inflammation of the capsular investment.
>
> "Apoplexy of the supra-renal capsules was discovered by me in the autopsy of a man aged sixty years, and again in that of a fœtus at full term born dead. In either case apoplexy had occurred in both of the supra-renal organs. In the first case—viz., that of the adult, the left capsule was seen to be considerably enlarged, but preserved still its ordinary form. This capsule was 50 millimetres long, 16 high, and 25 wide; its weight amounted to 16,492 milligrammes. The right capsule was less voluminous, and had a somewhat square shape; it was 24 millimetres long, 30 in width, 15 high, and it weighed 11,780 milligrammes. The fine stratum of connective-tissue surrounding them, and the capsular fibrous investment proper to them, were both strongly tinged with sanguineous infiltration. Incised parallel to their greatest diameter, and in the direction of their width, so that each became divided into two halves, there was displayed a coagulum of blood which had occasioned their increase in volume. The bloody coagulum was everywhere surrounded by cortical substance, so that this part was forcibly separated from the medullary portion, and was also loosened out, where one half joined on to the other, in such wise that each capsule, as its name indicated, really formed sacs deserving the name they bear. In the middle of the coagulum in the left capsule there was a little blood which was uncoagulated. The blood in either capsule was of a dark-red colour, but assumed a lively red tint on exposure to the air.
>
> "The apoplectic capsules of the fœtus presented a volume much greater than is usual, but they preserved entirely their proper form. The weight of each was eight grammes. The capsular membrane preserved its normal colour, but beneath it there appeared various spots of extravasated blood. When incised, the two capsules appeared of a deep-red colour, uniformly diffused, and this was derived from the blood, which fluid flowed easily from the surface of the cut, under the least pressure. The morbid change in the two capsules evidently consisted in an interstitial apoplexy; different from that of the two previously mentioned, which displayed each a single clot. Whether the blood came from

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* Archives Générales de Médecine, 1856, vol. ii. p. 390.

† Dr. John Ogle found that, on examining a large number of healthy supra-renal capsules removed from adults, the weight varied from 65 to 90 grains. See Trans. of Path. Soc., vol. ix. p. 408.

the rupture of one or a few vessels of some size, or contrariwise, from a considerable number of small vessels, it is difficult to say. Perhaps this latter hypothesis, considering the perfect preservation of form in the capsules, may be the most reasonable one; seeing that if any large vessel had burst, it is difficult to understand how the blood could so readily distribute itself in every tissue of two capsules. The cancer observed by me existed solely in the left supra-renal capsule. This capsule was larger than the common size, and weighed 35,063 milligrammes; its shape was nearly normal, except that it was somewhat nodulated on the surface. When incised in the direction of its large diameter, this capsule, instead of its normal parenchyma, contained a product of new formation which was almost entirely substituted for it. To remind you of the supra-renal capsule, there existed only on the convex margin a portion of cortical substance, and here and there internally some few yellowish spots spread among the lobes into which the new formation was divided. This last formation, as regards its character, had all the physical appearance of encephaloïd cancer.

"In the right supra-renal capsule of the same subject, there existed a small encysted tumour of the size of a nut, of adipose structure.

"As regards tubercle, I have twice met with its occurrence in both supra-renal capsules. In one subject, the left capsule, in its central part, was occupied from one surface to the other by a tubercle measuring ten millimetres in its transverse diameter, and twenty-five millimetres in the direction of its length. In this same capsule were found four other tubercles, one of which was of the size of a small lentil (*lente*), the other of a large pea (*pisello*). The right supra-renal capsule inclosed two tubercles of the size of a pea (*cece*). In the other two cases, the tuberculosis existed in one capsule only; in one case this occurred on the right, in the other on the left side. In the first-named case, there was a tubercular mass of an oval shape, twenty-five millimetres long, eleven wide, and eight thick; in the second one a tubercle of the size of a large pea (*cece*), situated in the centre of the organ.

"In the left capsule of a lady much advanced in years, I found several deposits of a material which had the physical appearances of tubercle in a state of crudity. The principal mass had the volume of a large pea (*pisello*). The parenchyma of the capsule was in great measure gone, and it was replaced by a very compact fibroïd substance which surrounded the various deposits of morbid material. Examined by the microscope, there appeared cells of various size which, when treated with acetic acid, mostly dissolved; those which remained were cells of fat. Taken altogether, the capsule was thicker and more compact than normal.

"As regards atrophy of the capsule, I found it in a man of seventy-three years of age. The left supra-renal capsule, the only one atrophied, was much smaller than the right, and certainly below the normal size; it had become of a somewhat circular shape and presented on each face sulci in the direction of the periphery passing from the centre; this gave it the appearance of an organ retracted on itself. The weight of this capsule was 3020 milligrammes; that of the right capsule was 4200 milligrammes. In this case the spleen was very large and its fibrous investment hard and thickened.

"Arrest of development of the supra-renal capsules was found by me in a fœtus of seven months affected with derencephalus. The capsules of this fœtus preserved their ordinary form as well as their colour, and were of such small size that neither of them weighed five centigrammes. The kidneys weighed 4 grammes and 35 centigrammes on one side, and 4 grammes 15 centigrammes on the other.

"The fœtus, with the exception of the cranial cavity, was perfectly well developed. Its weight was 686 grammes.

"I have often, as before mentioned, witnessed great differences of form, and adhesions of the capsules both to the kidney and liver. Once I found the right capsule of the length of nine centimetres. This resulted from the prolongation of its anterior lip, which reached to the level of the hilus of the kidney, and really covered that viscus on its anterior surface in the greater part of its extent at least. The lip, thus lengthened, was constituted entirely of the fibrinous envelope of the capsule, except at its edge, where there was a fringe of its proper parenchyma. From its change of form, the convex margin of the capsule had become parallel to the convexity of the kidney. Such intimate adhesions had occurred between the fibrinous membranes of these two organs that neither could be separated from the other. In the same subject, the left capsule adhered with equal force to the kidney, and where the adhesion occurred, in a certain space, only presented the fibrous envelope, the laminæ of which were glued together.

"In four instances, I found engorgement of the supra-renal capsules—viz., in a fœtus of nine months; in an infant six days after birth; in the body of a man dead with choleraic symptoms; in that of one with symptoms of asphyxia.

"In a subject of phlogosis of the enveloping membranes of the capsules, which occurred in my experience, these membranes presented a lively and uniform redness; moreover, in some parts were perceivable spots of more intense redness. The whole fibrous envelope was covered by a false membrane, rather thick and resistant, and so adherent in some points to the proper envelope that it was impossible to separate them. The parenchyma of the organ was healthy."

Of these morbid complications apoplexy is that which stands in strongest relief in the pages of our author. Addison, we believe, quotes but a single case of apoplexy of the supra-renal capsules; it was a case in which mechanical pressure by a tubercle on the large capsular vein occasioned extravasation.*

Other systematic writers have not appreciated the disease. Only Rayer† puts it prominently forward in a memoir written by him on diseases of the supra-renal capsules.

He mentions six cases of apoplexy with one or more large clots or centres of extravasation; and moreover, he affirms that he has found in newly-born infants blood *infiltrated in the form of ecchymosis in the interior of these small organs.*‡ The observations of Signor Mattei and of Professor Giorgio Pellizzari do not accord with those of Rayer as regards the considerable frequency of interstitial apoplexy in the capsules of the newly-born. It would seem to occur in them with more facility than in adults, however subject these organs may be throughout life to engorgement, as appears to be much the case, especially when there is some mechanical impediment, or an alteration in the crasis of the blood. Half the number of cases known to our author occurred in newly-born, and were due apparently to pressure, there being no other assignable cause. This observation is very worthy of the attention of accoucheurs.

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* In vol. xv. of the Trans. of the Path. Soc. of London, two cases of extravasation of blood into the supra-renal capsules are related: one by Dr. Dickinson, at page 256, and another by Mr. [illegible] at page [illegible], one also by Dr. [illegible] [illegible], is related at page [illegible] of the [illegible] volume.

† Recherches Anatomo-Pathologiques sur les Capsules Surrénales. L'Expérience, [illegible].

‡ Loc. cit.

The following is a case of interstitial apoplexy in an infant, quoted by our author:

> "In the month of May, 1862, the body of a female infant, born at full term, was brought into the dead-house of the hospital at Siena; it was said to have died during birth from compression of the umbilical cord. This child was well-formed and well-developed, of normal appearance and colour. The meninges and encephalon were healthy. The thoracic organs were sound, but the lungs undilated. The liver was large, and somewhat gorged with blood. Nothing was observable in the abdominal organs excepting in the supra-renal capsules, which were infiltrated with blood, as Rayer has described."

In this case the symptoms of compression of the umbilical vein, sufficient to produce death, were wholly wanting. No one would, we think, attribute death to engorgement of the liver. Signor Mattei refers it to capsular apoplexy. Such is the disease as described by him. Our author has not omitted to place before us the obscure symptoms attributed to supra-renal apoplexy in the adult:

> "Domenico Ronciani di Terranuova, aged sixty years, was received into the hospital of Santa Maria Nuova, in Florence, for sore legs, under the care of Dr. Bosi. On the 11th of November, 1858, he was seized with strong pains in the lower belly, which ceased only with extinction of life. This fatal result occurred at six in the morning of the following day, after six hours of great agony. On dissection, on the 13th, nothing was found but apoplectic clots in the two supra-renal capsules; all the other organs were sound. To what shall we attribute the abdominal pain, and the death of the patient? I do not hesitate to refer both to the apoplexy of the capsules, for the following reasons: 1. Because this was the only visible alteration. 2. Because the hæmorrhage was recent, as proved by the physical characters of the blood, and its behaviour when exposed to the air. 3. Because the abdominal pain was sudden, and it was the most prominent and earliest symptom. 4. Because the cause of it must have been in the abdomen; and finally, there was no other source to which we could attribute it."

Signor Mattei takes much account of the pressure of the enlarged capsules on the semilunar ganglia and their appendages, especially on the right side, where the liver increases the weight of pressure. Resistance is afforded by the spine and pillars of the diaphragm during inspiration.

In one of the cases of apoplexy the semilunar ganglion was found imbedded or indented in the capsule. It will be remembered that Dr. Brown-Séquard has shown that mechanical injury of either semilunar ganglion, especially of the right side, will arrest, or at least diminish, the movements of the heart. Blows over the stomach—not to invoke more refined observations—serve to prove the strong influence of these organs over vitality. It seems not unlikely that violent impressions on the very numerous filaments of the supra-renal nerves might both cause harm and afford ready evidence of it. In connexion with this subject, Signor Mattei adduces the following experiment:

> "In a rabbit, I somewhat violently compressed both supra-renal capsules, at the same moment, using the forceps pretty roughly as I did so. The animal, at

the moment of compression, gave a sharp cry, and fell instantly into a sudden and marked prostration. The respiration which, from the section of the abdominal wall, had increased in frequency and in noise, became all at once small, frequent, and silent; the state of general and violent contraction of the muscles ceased, and the animal, when loosened, scarcely stirred. In a few minutes it was dead."

It thus seems that innervation and enfeeblement of the heart's action follow injury of those parts.

The cause of apoplexy in the supra-renal capsules has been referred by Rayer to want of firmness in their so-called medullary structure, and in the fragility of the veins which it contains. Moreover, the large size of the chief vein of the capsule, according to Mattei, exposes it to pressure, which the numerous small arteries escape, so that any afflux of blood while pressure exists on the veins may tend to accumulation and rupture. In four out of six of Rayer's cases, we may add, the extravasation was on the right side. On this side the capsule is generally larger and thinner, in accommodation to the liver, whose proximity appears to modify its shape.

It will be seen on reading the work of Mattei that he has not related a single case of bronze-skin in connexion with the morbid anatomy of these organs. Yet he quotes with high respect the statistics of Dr. Chavanne,* who found that in 44 cases of bronze-skin 34 were cases of diseased capsules. So high a proportion deserves weight, from the comparison that in establishing the connexion of fatty liver with phthisis the proportion of its occurrence, according to Louis, is only 1 in 3 cases. Signor Mattei seeks conscientiously for a solution of the difficulty. When we reflect that the bronze-skin is found far more constant in its alliance with diseased capsules, while these, on the other hand, seem evidently more independent of the complication called bronze-skin, a just and natural reasoning would rather and more naturally attribute to the skin that originating element which should include the other in its consequences; but as we are not likely to rest satisfied with an argument based on such feeble calculations of frequency, it will no doubt be necessary to search in the field of physiology for some common cause which may yet not offend by leaving any of the pathological phenomena unaccounted for.

Considering that the *lesion of the functions* should determine the seat of the malady, our author believes it would be difficult to assign any common cause so proximate as a morbid condition of that ganglionic system which presides over and connects itself with the multiform operations of organic life. The lumbar pain, gastralgia, frequent nausea, and occasional vomiting which occur in so-called Addison's disease, are thus accounted for; so, also, the feebleness of the heart's action, the anæmia, the mental and bodily prostration, with subsequent muscular wasting and more rare symptoms, epileptic and other forms of convulsion.

It is, then, as a neurosis of the ganglionic system that our author has brought himself to consider this disease; and he seems to arrive at the conclusion by a process of reasoning which, if from its nature not wholly satisfactory, has the merit of being logical, consonant with modern physiology, and, we will add, in accordance with the earliest and most recently advanced views of pathology on the subject. It receives some degree of corroboration from those experiments of Brown-Séquard,[*] who in various animals has noticed the formation of pigment, and even of pigment-cells, after removal of the supra-renal capsules. As regards convulsions,[†] the same physiologist observed convulsive phenomena in animals to follow on lesion of the capsular nerves; and, finally, the question may be put thus—In what other direction than in the ganglionic nerves are we to look for an explanation of the whole of the symptoms of Addison's disease? As regards analogy, the phenomena of chorea, hypochondriasis, and hysteria are invoked by our author as examples to demonstrate how the first of these neuroses, in the existence of worms or other irritation, the other two, in irritation of special organs, serve to explain the part performed by the supra-renal capsules in co-operating with the nerves so as to develop the complementary phenomena which constitute the entire disease.

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* Gazette Hebdomadaire, 1860, tom. vii. p. 99.

The peculiar dead and earthy hue observed in the skin of starved and underfed races of mankind, as well as its well-known rosy or transparent tint in the florid and well-nurtured, still further, according to us, favour Dr. Mattei's opinion of the modified dependence of this organ, for its integrity and normal beauty, on the nerves of organic life. We may also notice the occurrence of ulcers of the skin from an endemic cause, or bad diet, as in ships, barracks, and jails, and we may be allowed to advert to the observations of Dr. M'Cormac on a character of skin occurring in the milder cases of epidemic fever in Ireland, 1843, which might be *termed bronze for want of a better term*, and to the observations of others regarding the deposit of colouring matter in the skin in the course of various morbid conditions. We must state, however, that Signor Mattei does not subscribe to the opinion that the so-termed Addison's disease, in its greater or less development, results from a general diathesis or dyscrasia, or that the supra-renal organs are often the subject of tuberculosis. In fact, according to his observations, disease of the supra-renal capsules seldom accompanies extensively developed cancer or tuberculosis in other viscera and organs. Most commonly they escape disease. When affected, the cause seems, according to him, rather referrible to obstruction from mechanical impediment or peculiar blood crasis. Except in the cases of apoplexy, the disease in the capsules never constituted the sole morbid alteration.

We feel we have been unable to do full justice to Signor Mattei in that part of his work which sets forth, at greater length than we can afford to follow him, the ground and substance of his opinions. The extensive extract which, as of the highest importance to place on record, we have taken the liberty to place before our readers, will fully display his merit as a laborious investigator; and those who will make his pamphlet the subject of careful study, will, we are persuaded, entertain like ourselves a high opinion of the author's merit.

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[*] Loc. cit. p. 573.

[†] Loc. cit. p. 400.

Review VIII.

1. *Gazeta Medica de Lisboa.* Redactor Principal Dr. Pedro Francisco da Costa Alvarengo. Publica se nos dias 13 e 28 de cada mez. Imprensa Nacional.—*Lisboa,* 1862–3.

Medical Gazette of Lisbon. Principal Editor Dr. Peter Francis da Costa Alvarengo. Published the 13th and 28th of every Month. Printed at the National Press.—*Lisbon,* 1862–3.

2. *Jornal de Pharmacia e Sciencias Accessorias.—Lisboa.*

Journal of Pharmacy and the Accessory Sciences.—Lisbon.

3. *O Escholiaste Medico.—Lisboa.*

The Medical Scholiast.—Lisbon.

4. *Compendio de Materia Medica e de Therapeutica.* Por Caetano Maria da Silva Beirão, Lente de Materia Medica e de Therapeutica na escóla Medico-Cirurgica de Lisboa, &c. Tomo 1^{mo}.—*Lisboa,* 1862.

Compendium of Materia Medica and Therapeutics. By C. M. da Silva Beirão, Professor of Materia Medica and Therapeutics in the Medico-Chirurgical School of Lisbon, &c. Vol. I.—*Lisbon,* 1862.

5. *Estudos sobre a Hemeralopia a Proposito dos Casos observados no Quarnicão de Lisboa, offerecidos á Academia Real das Sciencias de Lisboa.* Por João Clemente Mendes, Cirurgão de Brigada, &c. —*Lisboa,* 1862. 8vo, paginas 80.

Studies on Hemeralopia in reference to Cases observed in the Garrison of Lisbon, and offered to the Royal Academy of Sciences of Lisbon. By John Clement Mendes, Brigade-Surgeon, &c.—*Lisbon,* 1862. 8vo, pp. 80.

6. *Anatomia Pathologica e Symptomatologia da Febre Amarella em Lisboa no Anno de* 1857. Memoria apresentada á Academia Real das Sciencias de Lisboa em Julho de 1860. Pelo suo Socio Effectivo Dr. Pedro Francisco da Costa Alvarengo, Medico da Camara de suá Magestade, do Hospital de S. José, &c. Com 6 Mapas, 15 Taboas.—*Lisboa,* 1861. 8vo, paginas 338.

Pathological Anatomy and Symptomatology of Yellow Fever in Lisbon during 1857. Memoir presented to the Royal Academy of Sciences of Lisbon in June, 1860, by its Fellow, Dr. Peter Francis da Costa Alvarengo, Physician in Ordinary to the King, and also of St. Joseph's Hospital, &c. With 6 Maps and 15 Statistical Tables.—*Lisbon,* 1861. 8vo, pp. 338.

7. *Estudos sobre o Garrotilho ou Crup.* Memoria apresentada á Academia Real das Sciencias de Lisboa. Por Antonio Maria Barbosa, Socio Effectivo do mesma Academia, Cirurgião Honorario da Camara de suá Magestade Fidelissima, Lente da Escóla Medico-Cirurgica de Lisboa, &c.—*Lisboa,* 1861. 4to, paginas 189.

Studies on Garrotilho or Croup. Memoir presented to the Royal Academy of Sciences of Lisbon. By ANTHONY MARIA BARBOSA, Fellow of the same Academy, Honorary Surgeon to his Most Faithful Majesty, Professor in the Lisbon Medico-Chirurgical School, &c.—*Lisbon*, 1861. 4to, pp. 189.

8. *Memoria sobre a Tracheiotomia no Garrotilho.* Apresentada á Academia Real dãs Sciencias de Lisboa. Por ANTONIO MARIA BARBOSA, &c.—*Lisboa*, 1863. 4to, paginas 231.

Memoir on Tracheotomy in Croup. Presented to the Royal Academy of Sciences of Lisbon. By A. M. BARBOSA, &c.—*Lisbon*, 1863. 4to, pp. 231.

9. *Notes of Holiday Tours in the Peninsula.* By a PHYSICIAN. Anonymous.—*London.*

OUR April number contained an interesting article of some length regarding the hygienic condition of the Portuguese army, principally based on a valuable work recently published by one of its distinguished military surgeons, Senhor Marques, who is besides, a professor attached to the Medico-Chirurgical School of Lisbon. In our subsequent observations we propose extending the inquiry to questions chiefly of a civil character, more especially since, analogous to Spain, the medical literature of Portugal seems generally but imperfectly known among the great mass of British practitioners. However, like the adjacent country above named, decided progress has lately been made by the latter, both in reference to material as likewise scientific attainments, so that critics may now justly say, the Portuguese nation appears at last really awakened from its former apathetic condition and isolation, which many foreigners considered characteristic of native Lusitanians.

Politically speaking, this peninsular kingdom has recently effected quite as great social reforms as her Iberian sister. It also now possesses a free representative Legislature, and having long remained on friendly terms with England, without being so jealous of foreign interference or inimical to strange customs as their more eastern neighbours, the people of Portugal and its Government seem anxious to promote improvements in matters relating to commerce, education, and the learned professions. However, this not being an appropriate place for discussing such questions, we shall confine our remarks contained in the following pages chiefly to some points bearing upon medicine, and the modern advances which the nation has made towards attaining knowledge; and lastly, the present state of the profession throughout that portion of Europe, which was heretofore, and is still classically designated Lusitania. For that purpose, we have selected the medical periodicals and other works whose titles head this article, but especially the publication first named, it being now much esteemed by the profession in Portugal, and further, knowing that the 'Gazeta' virtually constitutes a good exponent of professional proceedings, both in the Portuguese capital and elsewhere.

Besides the journal just named, the two others also specified habitually discuss questions connected with medicine and its collateral sciences; both works being published in Lisbon. But as neither of these periodicals are generally considered by native professional readers of equal authority with the 'Gazeta,' our subsequent remarks respecting Portuguese medical literature, will chiefly comprise cursory notices of its contents, followed by brief allusions to several books lately published by medical writers in Portugal; in addition to which, we propose adding some observations made personally during a recent excursion in that country, undertaken as well for recreation, as for the inspection of various charitable establishments and other public institutions of that heretofore but little known, yet really interesting district of Southern Europe.

Contrasted with most medical journals of different European countries, the one here specially selected for consideration may probably appear to British critics as of an inferior description, both in reference to its limited size and also the little original matter usually supplied by native writers. Reviews of foreign books, with short extracts from the medical and scientific journals of other European nations, but especially French, frequently occupy a large portion of the columns in the periodical above designated. Quotations of that description nevertheless are doubtless instructive and equally interesting to most Portuguese practitioners. However, as critiques of new works written by various eminent Portuguese physicians and surgeons occasionally occupy the 'Gazeta's' pages, on that account alone, if not in other respects, it well merits perusal even by extra-peninsular *confrères*. Subsequently, original and critical remarks, with detailed reports of cases treated at St. Joseph's Hospital, the chief medical charity in Lisbon, or at other public establishments, are also occasionally communicated for the information of practitioners, whether national or foreign.

Having said this much of the journal in question, and further, that it might often be perused with advantage and profit by professional writers in other countries, we would now remark for the information of readers, that among various medical publications recently emanating from the Lisbon press, 'The Compendium of Materia Medica and Therapeutics,' by Dr. Silva Beirão, deserves the praise it has generally received. The author now named is an able professor in the Lisbon medical school; and, according to some native critical authorities, his work is deemed to have filled up a great blank which they admit previously characterized Portuguese professional literature. This production may virtually be designated a complete treatise on the subjects discussed, rather than a mere compendium, seeing that the first volume, which is large, enters often fully into the questions investigated. The author classifies the whole Materia Medica into four separate divisions—viz.: 1. Imponderable agents. 2. Products of the inorganic kingdom. 3. Vegetable productions; and lastly, animal substances. Upon the remedial and other properties of numerous articles comprised within these respective departments, Dr. Beirão speaks at some length, and most efficiently. Hence the treatise thus cursorily mentioned, is reported by his *confrères* as worthy of perusal.

Another recent publication, which has also appeared in Portugal—viz., a Memoir presented to the Royal Academy of Sciences of Lisbon by Dr. Da Costa Alvarengo—equally deserves special mention. The work now alluded to is entitled, 'Pathological Anatomy and Symptomatology of Yellow Fever,' which prevailed epidemically during 1857 in the Portuguese capital. This memoir forms a valuable contribution to medical science, and is highly creditable to the learned writer, who is editor of the journal which has been prominently quoted in previous paragraphs, as an instructive exponent of professional discussions at present mooted throughout the author's native country.

Of course, we cannot here enter into any minute description of the pathological appearances met with in the 63 dissections, as reported by the writer, of patients who died from yellow fever during the prevalence of that malady in Lisbon. It will suffice to observe, by way of giving an outline of the chief morbid alterations of structure then noticed, that in 53 cases the skin was more or less yellow-coloured; in 54 the spinal cord appeared normal; the brain was healthy in only 18; congested in 30; softened in 17; 16 had serous effusion in the ventricles; and 9 cases exhibited the brain as abnormally hardened. The lungs seemed of healthy aspect in not more than 10 instances; 49 being more or less congested, while the remaining 8 showed either ecchymoses, or that blood was effused in their structure. The mucous coat of the stomach appeared red-coloured in 20 examples, black in 33, and yellow in only 10 of all the bodies examined.

Besides these morbid phenomena, it may be added, the intestines did not look much altered from their natural condition, the mucous membrane being red in only 9 dissections, and yellow in not more than 3; whereas the intestinal contents seemed black in 43, yellow in 8, and red in 6, with 4 cases otherwise tinged, thus leaving but two bodies in which the matters contained were normal. The liver had a yellow colour in 58 cases, its size being augmented in 25, while the remaining 38 were normal in volume. The kidneys had become yellow in 13 cases, red in 6, and 44 continued of the natural colour. The urine was yellow in 13 of the bodies examined, bloody in 6, and black in only 1 instance; from which appearances it hence follows that the urinary secretion was deemed unchanged in the remaining 43 cases. According to the pathological facts we have now narrated, observers may very fairly infer that, generally speaking, there existed nothing remarkably peculiar in the morbid alterations of structure characterizing the recent outbreak of yellow fever at Lisbon, and that the consequent pathological phenomena did not essentially differ from those commonly noticed in fatal cases of malignant epidemic fevers, which are often met with throughout hot climates.

Dr. Alvarengo next details the symptoms he commonly observed in patients attacked by the disease under discussion. Among these are specially noticed hæmorrhagies, both on account of their extent and severity, as also from being often followed, and rapidly, by fatal results. Indeed, many cases of yellow fever even succumbed from the great loss of blood the patients experienced within only a few hours after being attacked, and when they had even walked on foot to the hospital, on purpose there to receive medical treatment. Vomiting of blackish matters constituted almost always one of the most serious symptoms characterizing the yellow fever recently prevalent in Lisbon, and this occurrence was so marked that "vomito preto"—black vomit, became the popular designation of that malady; while the substances dejected were chiefly blood more or less pure, and the ordinary contents of the stomach mixed with bile. Yellowness of the skin was likewise generally present, while great prostration of strength or impaired physical force usually supervened, especially when the disease first commenced; at which period, the sufferers often felt so much coldness of their frame that, this special feature was then denominated "forma algida." Suppression of urine also frequently followed; and wherever this symptom occurred, it usually indicated much danger. During the latter stages of attacks terminating fatally, delirium was not uncommon, being then generally low or muttering, but seldom becoming furious, particularly when repeated hæmorrhagies occurred. Coma and stupor were ultimately often marked symptoms, although however but rarely accompanied by any diminution of intelligence on the individual being roused. Of this feature some very interesting and affecting illustrations are recorded in the treatise at present under review. For instance, we may cite the case of Surgeon-Major Calvet, who, although suffering from a severe attack of yellow fever, actually superintended the medical treatment of his friend Lieut. Damblard, then occupying an adjacent bed in the same dwelling, to which both patients had been transported, on the first appearance of their respective maladies. Examples of a similar clearness in the patient's intellectual faculties, notwithstanding that various parties so situated were otherwise severely affected, might be likewise quoted; but that seems superfluous, since to describe more fully every peculiarity characterizing the late epidemic visitation at Lisbon would extend our remarks far beyond all reasonable limits. Consequently, we refrain from further extending our critique of Dr. Alvarengo's able production.

The next work to which we would direct notice, when enumerating the novelties in medical literature that indicate progress now apparently made in Portuguese medicine and surgery, is Dr. Mendes's opuscule, designated 'Estudos sobre a Hemeralopia.' The author here named is an army surgeon and director of the military hospital of Lisbon, from the wards of which establishment, and through the experience therein obtained, most of the writer's observations appear chiefly based. His publication is divided into six separate chapters—viz., 1. Definition, synonyme, and history of the malady; 2. Etiology and nature; 3. Congenial or concomitant diseases and complications; 4. Symptoms, diagnosis, and pathology; 5. Duration, prognosis, and termination; 6. Prophylaxis and treatment. Although some controversy has arisen among the author's professional brethren at Lisbon in consequence of the opinions enunciated by Dr. Mendes with reference to the affections described, we deem it unnecessary now to enter thereon, our present object being principally to show that the army medical officers of Portugal are not idle in their vocation, but seem, on the contrary, zealously disposed to advance sound military surgery through clinical observation and practical experience.

To the publications of Senhor Barbosa, who is one of the most eminent civil surgeons now practising in Lisbon, and also an able professor in the medico-chirurgical school, besides surgeon to St. Joseph's Hospital, we would now direct the attention of English practitioners. His 'Estudos sobre o Garrotilho ou Crup' is really an excellent performance, and well deserves being translated into English, for the information even of practitioners in Great Britain. This author states that the work above-named is chiefly intended as preliminary to a subsequent publication which he proposes giving to the profession, more especially with reference to performing tracheotomy for curing the disease discussed in his present volume. After entering largely into the history of croup, which he successively traces from Hippocrates, through numerous foreign authors, up to recent times, and after especially noting its prevalence epidemically throughout Spain, as also in Portugal, during former centuries, the writer next reviews its pathology, causes, symptoms, prognosis, and finally the treatment.

Respecting the several points which Senhor Barbosa has thus specified, our limited space prevents alluding to them seriatim. Nevertheless, we may succinctly remark that, out of 44 cases of croup which came under observation, 30 died, while only 14 recovered, or less than one in three of all those attacked, the period of life most fatal being in young persons ranging from their third to sixth year, when, among 21 cases of the disease at that age, 15 deaths were reported. With reference to remedies employed, according to the results just stated, it may be fairly concluded few of those used appear to have been of much efficacy. However, as the author divides this part of his treatise into medical and surgical treatment, but postpones discussing the latter portion of his subject, we can only now notice the former, and that even but briefly. Sudorifics were found often useful, especially in the early stage of an attack; while emetics always acted far more efficaciously, tartar emetic and ipecacuanha being the best of that class, exhibited either singly or when combined. Chlorate of potassa seemed likewise often beneficial; and although calomel internally, with mercurial frictions, were frequently tried, Senhor Barbosa considers that mode of treatment generally proved prejudicial rather than useful. Antiphlogistic medication he further found was very often exceedingly injurious. In fact, to lower the system of young patients suffering from croup was always deemed dangerous by the author. Hence, blood-letting in any form, blisters, and even scarifications, were absolutely interdicted. Purgatives he thought also improper; but where the bowels became loaded enemata should be used, and if these proved inefficient, citrate of magnesia, or small doses of castor-oil might then be exhibited as mild aperients. Among topical applications, nitrate of silver held the first rank in Senhor Barbosa's estimation; after which sulphate of copper and the perchlorate of iron were sometimes observed to produce good effects; but on the whole he only considered these agents as adjuvants in the treatment.

When concluding this memoir, he observes finally that "garrotilho" ought always to be treated with tonic remedies, quinine and preparations of iron being most efficacious. Besides the remedies here named, generous wine and animal food, particularly flesh whose fibre is red-coloured, should be administered. Moreover, cold baths, either of plain water, or to which aromatics or salt had been added, were likewise found useful in several cases, while electricity, by exciting the nervous system, sometimes proved advantageous. In short, the author's mode of management, which is very strongly recommended in his work, and now brought under the notice of British medical practitioners, was invariably the reverse of antiphlogistic.

After an interval of two years, Senhor Barbosa's promised 'Memoir on Tracheotomy in Garrotilho' made its appearance in Lisbon, under the title classified No. 8 among the headings of the present article. This valuable contribution to surgical science the author divides into two principal portions, the first being chiefly occupied with the applicability of tracheotomy in cases of croup, according to the views now recognised by European practitioners; while the second section of his publication refers especially to 38 examples of that operation which have been recently performed in Portugal, 12 of whom he states recovered, 6 being in-patients of his own; while the other 6 cases occurred in the practice of three different surgeons, colleagues of the reporter. Besides discussing the propriety of having recourse to tracheotomy for curing croup, Senhor Barbosa enters at some length into the history of that operation, which he believes was known even so early as the time of Asclepiades of Bithynia; the surgeon who first actually peformed it being Antyllus about the third century. The author, however, observes that Dr. Home, of Edinburgh, recommended the operation in 1765 for croup; although Bretonneau and Trousseau, in France, have more recently brought it into greater repute among modern surgeons. In Portugal, the earliest examples actually known to have proved successful occurred during October, 1852, when the life of an infant labouring under croup was fortunately saved through tracheotomy. This favourable termination produced considerable impression upon the minds of various Portuguese practitioners, chiefly in consequence of three previous operations of the same description having ended fatally, whereby it fell into so much discredit that many surgical authorities in Lisbon expressed decided opinions adverse to its adoption, at least until the above satisfactory result again materially reinstated the operation of tracheotomy in professional estimation.

Respecting the ages of the 12 patients on whom tracheotomy was successfully performed by Senhor Barbosa and different Portuguese surgeons, it appears that 2 were four years old, and 4 aged six; while the greatest number of deaths met with occurred among parties in their fifth year, when 6 were reported. Speaking generally, from the varied tables now published by Senhor Barbosa, it may be fairly inferred, that the most favourable period of life for performing tracheotomy in garrotilho, or croup, was in patients varying from three to seven years; seeing that out of 26 operations at such periods, 11 proved successful, whereas not one of the eight young persons operated upon, who had attained seven years, and up to their seventeenth, recovered; while further, only 1 in 4 operations performed on infants aged from nine to thirty months, terminated satisfactorily, the other 3 having died subsequently. Therefore, reasoning upon the above data, it may be concluded that the most auspicious time of life, for advising the operation under discussion, would be between the fourth and sixth year; the consequent danger being much greater if the age falls below or exceeds that period of life. Such results seem analogous to the experience recorded at the children's hospital in Paris, where among 29 successful cases of tracheotomy, 19 were met with in patients varying from their fourth to sixth year inclusive; indeed, nearly half of the above cures occurred when the patient was five years old at the time such an operation was performed.

Subsequently, Senhor Barbosa adds, in reference to sex, that among 20 female children operated upon by Portuguese surgeons, 7 recovered; whereas only 5 males out of 18 similar operations were so restored to health; the deaths in both sexes being, however, equal in number, or 13 of each; but then it should be also remembered, that 20 females underwent the operation, contra-distinguished to 18 males, analogously treated. Consequently, if any discrepancy can be assumed to prevail, the scale vibrates somewhat in favour of females. But further to illustrate the conclusions now enumerated, the author gives an interesting statement respecting the total cases of tracheotomy performed recently, whereof he had obtained reliable evidence, and in which the ultimate results were published. The aggregate number of such cases amounted to 1164 altogether, of whom 627 were male, and 537 female patients; among these, 143 males were cured, and 133 females; while 484 of the former sex, and 404 of the latter, died subsequent to tracheotomy; thereby shewing that, as 276 cures followed in 1164 operations, the ratio of recovery is about 1 out of every 4 cases on whom tracheotomy was resorted to when treating garrotilho. This result will be considered by some as not over-encouraging; nevertheless, believing that most of these individuals would most likely have died from the very dangerous malady with which they were attacked, if only a single life can be saved in every four operations, surgeons are, we think, fully justified in making the attempt even under most unfavourable anticipations, based upon such statistics, and especially when the patient operated upon ranges from four to six years old, that age being more favourable in results than if either older or younger.

Afterwards, the author makes some interesting observations regarding the season of the year in which tracheotomy will most likely prove beneficial. According to his remarks it appears that autumn and spring both appear in a greater degree conducive towards recovery than summer, while cold winter months always become more prejudicial in cases where this operation was performed. To which opinion the writer, however, adds, that should the disease prevail epidemically, such contingent feature must likewise be taken into consideration prior to forming any prognosis respecting the individual's likelihood of recovery.

Although numerous equally important points, both in reference to the operation and the disease for which it is authoritatively recommended, are amply investigated in subsequent chapters of Senhor Barbosa's elaborate treatise, the remaining space now at our disposal prevents us even briefly alluding thereto at present, however much farther criticisms might prove instructive. Still we may here observe, that the anatomy of the tracheal region, and the instruments employed by himself, and also the mode of operating by Trousseau, Chassaignac, and Maisonneuve, are minutely described in consecutive portions of the author's publication.

The accidents which sometimes supervene during the performance of tracheotomy, and various subsequent contingents, are likewise fully discussed by the author. Among these he specially alludes to hæmorrhagy, the entry of blood into the trachea, asphyxia, wounding the windpipe at its posterior wall, cutting into the œsophagus, convulsions, and finally, emphysema: all which serious consequences, alike embarrassing during the operation, occasionally supervene, and often materially influence the ultimate convalescence, whether it terminates successfully or otherwise. Besides immediate injurious effects, the writer speaks of others more remote, which may affect a patient during different future stages, and so endanger recovery. The secondary maladies enumerated are, erysipelas and gangrene attacking the wound, ulceration taking place in the trachea, hæmorrhagy, accumulation of mucus in the air-passages, bronchitis and pneumonia. Upon each of these subjects useful practical remarks are made. We now pass on to consider the treatment advised after the operation, which is of equal, if not greater, moment, in reference to convalescence.

To the judicious feeding of his young patients on whom tracheotomy had been performed, Senhor Barbosa attaches the utmost importance, and says, abstinence or low diet, with any kind of debilitating remedies, always become exceedingly injurious—"*sobre modo funesta.*" Avoiding every form of antiphlogistic treatment during the first twenty-four or forty-eight hours after the operation, he strenuously advocates, at the same time, that liquid aliments should only be administered;

the best food, in the writer's estimation, being at that period good and pure milk, of which from two to four ounces should be given every two or three hours, according to the patient's age and capabilities of digestion; but warm beef-tea can also be alternated in certain cases with the above aliment. About the third day, flour, arrow-root, and similar farinaceous substances may be combined with the beef-tea or milk previously prescribed; besides which, cream or calf's-foot jelly are deemed judicious additions, if relished by the juvenile patient. On the fourth day, nutritious soups, soft boiled eggs, warm milk and bread, or even chocolate, are allowable, if not contra-indicated by special circumstances. Towards the fifth and sixth day, solid animal food, such as beef or mutton, if properly cooked, often prove useful; at the same time, port wine—*vinho do Porto*—should be prescribed, particularly if great prostration of strength supervenes, or the symptoms assume a malignant character, in which case quinine, taken in tea or coffee, will likewise act beneficially—indeed, are essential. We have been thus minute in reference to the mode of alimentation Senhor Barbosa pursued in the cases on which he operated, seeing that the rules now described for English readers' information were chiefly considered by that surgeon as being often highly conducive to the patient's recovery.

Respecting the internal medical treatment which the Portuguese surgical authority, whose work has now passed under review, recommends for adoption in cases of tracheotomy, it seems to have been always exceedingly simple, and he by no means sanctions over-medication. Senhor Barbosa restricts his remedies almost exclusively to administering chlorate of potassa in doses of from one to two drachms dissolved in water, according to the patient's age, which he advises being given during the twenty-four hours at frequent intervals. If, however, typhoïd symptoms set in with much physical debility, then sulphate of quinine and perchlorate of iron must be substituted. Should dyspnœa afflict the patient, or difficult expectoration supervene, especially if mucus be also abundant, small doses of ipecacuanha or tartarized antimony, constitute the best remedial agents under such circumstances. Finally, to accomplish convalescence, improved nourishment, tonic remedies, steel, country air, and sea-bathing are strongly insisted upon by the eminent surgeon, whose recent literary contributions towards advancing medical science have occupied our attention in previous paragraphs. To illustrate the various points discussed by Senhor Barbosa, detailed reports of the thirty-eight cases alluded to are appended in the concluding part of his dissertation. Many of these interesting examples, minutely describing the malady under investigation, will repay perusal; and all extra-Iberian medical practitioners who feel desirous of becoming better informed on the present state of Portuguese surgery, especially in reference to croup, may consult the memoir now brought under their notice, with considerable advantage, if not valuable practical instruction.

Reverting to the 'Gazeta Medica,' we would here again observe that, various original communications are interspersed in its recent numbers, which deserve notice, both by foreign and native practitioners, as well from communicating useful knowledge on important questions respecting the practice of medicine and surgery, as from their illustrating the present condition of medical science in Portugal. Among several papers of interest, one on the advantages of Lithotrity over Lithotomy, by the author just previously named—viz., Senhor Barbosa—should not be overlooked. In this communication it is stated, besides other facts, that lithotrity has only very lately been generally practised by Portuguese surgeons; the first operation being about twenty-five years ago, when an eminent practitioner named Pereira, attempted its performance. However, as this patient died from acute cystitis, after only two sittings, so much discredit was thrown upon such novel proceedings, that no second operation of the kind was deemed advisable until June, 1857, when Senhor Continho had recourse to lithotrity on a boy aged nine years. Unfortunately, a fragment of the stone having been left in this young patient's bladder, another surgeon was obliged some short time afterwards to perform lithotomy to ensure ultimate recovery. Undismayed by the very discouraging results following these early operations, lithotrity was nevertheless undertaken by Senhor Barbosa in October, 1857, on an adult aged fifty years, who, unlike the two patients previously mentioned, completely recovered in about six weeks. This propitious termination induced the Senhor more readily to perform an analogous operation a second time—viz., in October, 1861, on a man seventy-two years of age. This patient having also got well speedily, lithotrity hence came to be considered by many Portuguese surgeons as a judicious operation, and one preferable in many cases to that of lithotomy generally adopted.

Another original communication in the 'Gazeta's' pages may next be named as worthy of notice—viz., the remarks regarding rice cultivation in Portugal, and the insalubrious effects thereby caused upon residents of districts where that cereal is planted. As this agricultural occupation and its produce every year assume more importance throughout Portugal, the number of victims consequent thereon has lately numerically increased. Some villages which were formerly healthy have thereby of late so much changed their sanitary character, that literally these localities have been almost depopulated, since the adjacent fields were converted into rice-grounds. Among the places thus deteriorated in reference to health, the town of "Caldas da Rainha" may be specified: more especially as it has been long a celebrated resort for fashionable visitors during certain seasons, on account of its baths and medicinal springs, which have obtained great reputation in relieving, if not curing, various maladies. The Caldas waters are at present often employed by persons having scrofulous constitutions, as likewise in cases of indigestion, and its warm sulphurous baths, being chiefly esteemed as efficacious in rheumatism, have hence become much frequented by both rich and poor. In districts where the cultivation of rice has recently much superseded other cereal productions, the diseases which prevail are fevers, exhibiting often malignant types. Residents of such localities pass their lives under miserable circumstances, while they also suffer great privations. They become wasted and cadaveric in person, get large bellies, and ulcers often attack their extremities, being sometimes followed by erysipelas. The deadly fevers which devastate this wretched population are usually intermittents of a most pernicious kind, and so fatal as often to rival those common in many unhealthy localities throughout the African sea-coast; while dropsy, with enlargement of the liver and spleen, also appear frequent accompaniments. Speaking of the deleterious influences which afflict the health and physical condition of persons who dwell or even briefly reside in these marshy rice-producing districts, the writer already quoted states that their constitutions are lymphatic, cachectic, and likewise soon become much impaired in physical strength. Numerous marriages among such residents prove unfruitful, while in other cases a considerable proportion of the females impregnated suffer abortion. Even where living infants are born, a large number die during the first months of their precarious existence; and when by chance they even are able to struggle through ordinary infantile perils, it may almost be confidently foretold that the offspring born of debilitated parents so constituted rarely attain manhood—most likely they will all fall victims to premature senility.

During a recent holiday visit made by ourselves to Caldas da Rainha, purposely to see this reputed fashionable Portuguese "banhos," it was then reported on good authority that upwards of 300 deaths from fever had taken place throughout the autumn among the rural inhabitants of several adjacent villages, whose vicinity, being rice-fields, was hence rendered insalubrious. No wonder can therefore be felt, if the public generally have lately got alarmed at the injurious consequences which often follow the extension of rice-rearing into districts of Portugal, where it was previously unknown. The attention of Government has very properly been called to this matter; and various remarks made in the 'Gazeta' respecting this important subject conclusively show that the evil has become now so great as to require some efficient remedy for preventing further depopulation.

The report of an interesting case in the 'Gazeta,' in which the left subclavian artery was tied for the cure of axillary aneurysm, and terminated favourably, likewise deserves being brought under the notice of English readers, so as thereby to illustrate the progress recently made in Portuguese surgery. It seems, however, instructive here to mention, in reference to such an important operation, that up to March, 1862, only four examples of such-like operations had been previously met with in Portugal, the first being a case, in 1826, treated by the same able surgeon who introduced lithotrity, namely Senhor Pereira, that gentleman having then tied the carotid artery of a male patient aged thirty-seven, in St. Joseph's Hospital, which proved successful. The second operation was performed likewise in St. Joseph's Hospital, twenty years subsequently, by Senhor Vianna, but unfortunately without a favourable result, as the patient died on the sixteenth day, from hæmorrhage. Senhor Almeida was the third surgeon who, during 1846, adopted the same proceeding at St. Antonio Hospital, Oporto, in a case which ended satisfactorily. This was followed, in 1847, by the fourth example, which occurred in St. Joseph's Hospital at Lisbon, when Senhor Zeixeira was the operator, but, like the second instance above recorded, it also proved unsuccessful. These data, giving two recoveries against two deaths, not being very encouraging, were not deemed much in favour of the practice adopted, and hence it was not till twenty-five years afterwards that any Portuguese surgeon again undertook the operation in similar cases—i. e., not previous to April, 1862, when Senhor Barbosa tied the left subclavian artery of a male patient in St. Joseph's Hospital at Lisbon, the particulars of which are detailed in his communication to the 'Gazeta,' where he states that on the 4th of May, just one month after the operation, the wound became cicatrized, and by the 20th the patient returned home cured of his axillary aneurysm.

In different numbers of the already often-named journal detailed accounts are given of drugs and other medicinal objects which Portugal or her colonies contributed to the late International Exhibition, at London. The explanatory catalogue there supplied is extensive, and shows further that, many important articles, frequently employed by medical practitioners, may be, and indeed are, actually obtained from various districts or colonies of the Portuguese dominions. To national readers of the 'Gazeta' these statements must have proved both interesting and instructive, while to foreigners they conclusively indicate how much several European nations might benefit through extended commercial relations with Portugal and its dependencies.

Another feature characterizing the periodical at present quoted should likewise be mentioned—viz., the copious extracts it usually contains, taken from French, German, and English medical publications. The frequent references so made are highly important, not only as being an excellent mode of conveying practical information to its readers, but also as enabling native practitioners to know what is now passing in adjacent countries. Indeed, it may be asserted with confidence, that the profession in Portugal is by no means ignorant of what their *confrères* are doing elsewhere; for, besides giving the above fact in illustration, we can add, from personal observation, that many professional men of that metropolis are necessarily familiar with British medical literature, since on the tables of reading-rooms various periodical publications which are well known throughout Great Britain, were by ourselves more than once recognised. Further, as educated Portuguese gentlemen frequently understand English, they are better able to avail themselves of the knowledge contained in books of that description. In fact, numerous residents at Lisbon and Oporto seem even more versed in the language and literature of England than Englishmen frequently appear respecting these subjects in Portugal.

Nay, the former often remain ignorant, if not regarding matters of science, very much at least in reference to professional questions now discussed throughout this eastern portion of the Iberian peninsula. However, for those who are desirous of procuring such information, the 'Gazeta de Lisboa,' and other medical journals named, will often be found instructive vehicles, and amply deserving perusal.

After these varied critical allusions to several modern Portuguese medical publications, and the frequent references to the periodical which has heretofore often occupied our special attention, we shall now proceed to give some general outline illustrating the recent history, progress, and present state of medicine in Portugal, believing that such endeavour, even if imperfectly accomplished, may prove interesting to readers, and also may not be deemed incompatible with our previous remarks and observations. We therefore shall now advert to several questions having an intimate relation to the medical profession in the country to which this article has repeatedly referred, or to matters appertaining to its charitable institutions, since these inquiries seem especially worth investigation, and hitherto have not at any time attracted sufficient attention, even when noticed incidentally in foreign publications.

Medicine in Portugal, considered historically, may be justly designated as having always shown more or less progress; and, notwithstanding that this feature has indubitably manifested itself in various paths, as also by even different means, it was chiefly cultivated, until very recently, through the University of Coimbra, which still remains the only establishment of that description for education, whether civil, religious, or professional, throughout the kingdom. Previous, however, to its foundation in that city, about 1537, we should also here mention, there had existed in Lisbon a similar institution since 1290, until it was translated to Coimbra, in the year first named, according to a Popish bull issued for the purpose. Heretofore, or during early ages, the practice of medicine, and the duty of instructing students in the healing art was generally deemed a clerical privilege, which the clergy were considered by common consent to have inherited from ancient popular usage, and according to the prevalent custom in most European nations. When first organized at Coimbra, the system there pursued was the one previously followed at Lisbon—namely, that comprehending the Galeno-Arabic doctrines, which then predominated in medical teaching and discussions, these being, of course, more speculative than practical. Nevertheless, even at this very early period the University frequently produced both learned and excellent physicians, some of whom subsequently attained considerable eminence. Afterwards, when the study of anatomy, physics, and chemistry, created entirely new views in physiology, whereby medicine became more a science of practical observation and experience, instead of being based on theoretical speculations, the medical faculty at Coimbra began to decline in reputation.

Sundry causes further contributed to produce that result. Among these may be named, as of prominent importance, its distant site from the metropolis, which in every country always constitutes the focus of scientific and literary advancement, besides forming the great centre of attraction to men of superior intellect and acquirements. Possessing only a small hospital, where no large number of patients could be obtained from so limited a population as that of Coimbra, and in consequence of which no great variety of diseases would be likely ever to come under observation for the practical instruction of medical pupils, it consequently followed that these most essential requisites towards advantageously pursuing pathological studies, as also for superintending the treatment of disease, became eventually wanting.

Irrespective of this serious drawback, which retarded the proper prosecution of professional education, the restricted attention generally given to the study of anatomy, as likewise to physiology and pathological investigations, had an equally detrimental influence. Indeed, it has been asserted by native authorities, when speaking of the educational system formerly pursued at Coimbra, that notwithstanding that anatomical knowledge constitutes the very basis of sound medical education, dissection of the human body was usually so little cultivated by teachers in this celebrated university that, during not very ancient times, instead of operating on human corpses as elsewhere, *sheep's carcasses* were even substituted. The depressed condition of surgery, owing to the indolent habits of all parties, too generally prevalent, often had a benumbing influence both upon students and professors.

Nevertheless, being the first and principal teaching institution in Portugal, also having great traditional reputation, and furthermore, possessing exclusive privileges in conferring the degrees of bachelor and doctor upon candidates for such titles, Coimbra did not suffer from competition with any other national establishment.

An implied literary reputation, and the supposed, if not real mental development which attendance at this "Alma Mater" could impart to its disciples, also materially tended towards keeping up a fresh supply of juvenile aspirants for university honours. The quality of students who mostly frequented this seat of learning must likewise not be overlooked; since a large proportion had sprung from parents in easy, if not wealthy, circumstances: while numbers even appertained to families occupying often high social positions. The numerous graduates in philosophy, mathematics, law, and theology, who annually issue from its academic precincts, and whereof many rose ultimately to fill important offices in the State, unmistakably indicate the reputation this national institution has acquired among Portuguese youths for their education, and who there obtain an essential qualification to enter the public service in various capacities. To illustrate the privilege, or rather monopoly, possessed by the University of Coimbra, it may be mentioned that, prior to being admitted a candidate, for example, in the diplomatic department, the applicant must have first spent a certain number of years at this institution, and also become a graduate. Otherwise, he could not be appointed to various employments, both civil or professional, as it was imperative that the nominee should have previously passed the requisite examination, and obtained a diploma from Coimbra. Even peers cannot take their seats in the Upper House of Parliament without producing the above essential document; of which we know an example—viz., that of the eldest son of a deceased nobleman who could not be admitted as the hereditary successor to his own father's peerage, in consequence of his having been "plucked" at Coimbra; such an impediment having first to be removed ere this new "Condé" became legally qualified to enter the Chamber of Peers, as a legislator.

Another feature of pre-eminence likewise at one time distinguished this University—namely, that it had long remained as the seat of the superior council of public instruction for Portugal. The above circumstance imparted great power to that body, besides giving a predominant influence, and even an independence of any external control, which was sometimes exhibited by their opposition to various reforms considered advisable to improve the mode of teaching and so forth at the University. However, that *prestige* no longer continues, since the National Council of Education has been transferred to Lisbon; owing to this and to the fact that the former splendour attached to the medical faculty at Coimbra has become considerably obscured by decided progress made elsewhere, matters now seem materially altered. Besides, the present educational board being less influenced by its previous provincial position and local prejudices than heretofore, no partiality is shown for ancient systems, nor an unwillingness exhibited to foster modern ameliorations. Such altered circumstances appear further to have influenced medical legislation, especially since an eminent metropolitan surgeon was lately appointed president of the educational council. Consequent upon these and other changes, the medical faculty of Coimbra does not now retain all its pristine reputation among unprejudiced Portuguese practitioners. Being deemed retrograde, and strongly adhering to ancient usages while relying on privileges it always possessed, critics confidently assert that this provincial school does not keep pace with the decidedly active literary and scientific movement manifested in the capital, where much more ample means and opportunities are found for pursuing practical studies, and where medical alumni can more readily obtain useful professional knowledge. Nevertheless, it would be here unjust were we to avoid stating that various medical professors attached to the University of Coimbra are learned men, and enjoy considerable reputation as teachers. To have been educated at Coimbra indubitably gives to its pupils a certain degree of importance; and as many belong both to the middle and upper classes of society, the connexion with the place becomes in after-life often advantageous; and to have been a graduate of the celebrated Portuguese "Alma Mater" of Coimbra, always gave bachelors and doctors of medicine a higher social position, than if they were simply surgical practitioners. Unfortunately in Portugal, as often is the case elsewhere, jealousy soon sprang up betwixt the Coimbra University and analogous medical institutions, especially the medico-chirurgical school established at Lisbon. But such antagonism, however regrettable, is not peculiar to Portuguese educational establishments, since similar feelings exist in other countries—as, for example, in Sweden, among others, where the Carolinisch Medical Institute at Stockholm contends with the University of Upsala for independence regarding the power of granting licences to practise medicine.

The Lisbon school although now of great repute, during some period after its inauguration, early in the last century, had only one professor—viz., of minor surgery—whose duty was merely to teach pupils attending at St. Joseph's Hospital, the art of bleeding, dressing wounds, opening abscesses, and similar trifling operations. Subsequently, an anatomical chair was added, and afterwards a lecturer for the higher operations in surgery. When pupils attending this school had complied with the rules prescribed, at first not very stringent, they were examined before the chief state surgeon, who, until within a few years past, possessed the privilege appertaining to his office, of licensing persons to practise surgery throughout the Portuguese dominions. Besides these surgical examinations, it is further interesting to mention, that the council of public health also examined and could license practitioners as dentists, or make "Sangradors," who might then bleed legally. Matters continued much in this state until 1825, when the Royal School of Surgery at St. Joseph's Hospital was instituted. The course of education at this place comprised five years, while the preparatory studies it required were Latin, Logic, French, and English. In 1836 a royal charter made some modifications at the above institution, and named it the "medico-chirurgical;" at the same time that a similar school for surgery was also established at Oporto by Government; both being now in full activity, and enjoying considerable reputation.

At present, the course of education prescribed by law for students attending either of these surgical academies much resembles the Medical Faculty of Coimbra's curriculum, the preliminary studies being likewise analogous. The only difference between these bodies consists in the monopoly which the University enjoys of making physicians, and granting the degrees of bachelor or doctor of medicine. Notwithstanding, however, the great similarity now existing at these different medical institutions, with reference to the system of education required from alumni, if a surgeon licensed by the Lisbon school afterwards wishes to become either bachelor or doctor of medicine, he must regularly attend, like any other pupil, the varied courses of lectures ordered by the Coimbra Faculty, besides possessing every preliminary requisite before he can be examined for any of these honours. In fact, attendances elsewhere to procure professional knowledge are ignored at Coimbra, since all parties coming up for examination must have previously studied five years at this University; otherwise, they cannot appear as candidates for academic titles. In consequence of this restriction, when medical pupils of the Lisbon or Oporto school wish to obtain degrees in medicine, they usually resort to Paris, Belgium, or Germany, where their previous professional studies are recognised as part of the necessary curriculum. It hence follows that, notwithstanding the two Portuguese institutions above designated are deemed worthy of recognition by foreign Universities, neither have heretofore been acknowledged by their own national alma mater at Coimbra, or considered to be efficient teaching establishments.

Formerly, and until a very recent period, public opinion ascribed considerable importance to practitioners who had obtained a University medical degree. Municipal corporations then gave to candidates having that qualification a preference when conferring official appointments. In hospitals, and for employment under Government, such parties were not only often preferred, but even better remunerated, while they also occupied a much higher social position, than persons not academically decorated. Besides these privileges attached to the doctorate, not only were medical professors invariably selected from among graduates of Coimbra, but various lectureships in the medico-chirurgical schools became similarly occupied. At present, however, as these restrictions are being done away with in many respects, the number of surgeons who now visit foreign countries to procure medical degrees has much diminished, and people believe that, should further proposed reforms respecting medical education be accomplished, emigration to foreign countries solely to obtain university diplomas will no longer continue, since the inducements to take that step will have lost much of their former value through recent judicious legislation.

At the three Portuguese institutions already named—viz., Coimbra, Oporto, and Lisbon, as also Madeira and Goa, which colonies have each analogous establishments for educating medical students—the system pursued is identical, both as respects preliminary acquirements, and nearly so in their subsequent professional curricula. The preparatory studies required, when young men first matriculate as pupils at any of the above-named medical academies, besides a knowledge in the languages already mentioned, comprise philosophy, principles of natural right, geography, chronology, history, mathematics, physics, chemistry, botany, and zoology. When admitted, the university medical student spends his first year in attending lectures on anatomy; the second is dedicated to physiology, hygiene, and operations; the third, to materia medica, pathology—medical and surgical—with clinical lectures both on medicine and surgery; the fourth is employed in studying midwifery, the diseases of women and children, as also internal pathology along with clinical medicine; while the fifth year embraces medical jurisprudence, public hygiene, and further attendance on clinical medicine. Having completed the lengthened educational course just specified, a pupil may then be examined for the degree of bachelor in medicine. But to obtain the higher grade of doctor, another year's residence at the University is required. After complying with these regulations, besides giving theses on the various subjects previously studied, candidates must likewise publish an inaugural dissertation. As already stated, the curriculum required at the Portuguese medico-chirurgical schools is nearly identical with that of Coimbra; but in addition to publishing an inaugural dissertation, all aspirants for surgical diplomas must likewise propound six theses, three being on medical and three on surgical questions, wherewith the period of pupilage terminates.

Attached to the institutions named, there are students in pharmacy, and also female pupils who propose becoming midwives. The term of attendance for either class is two years, the preliminary qualification for candidates in the first category being knowledge of mathematics, philosophy, Latin, and their own language. If they are afterwards found duly qualified, a licence to act as pharmaceutists or midwives is then granted to such candidates respectively.

Analogous to several European countries, which need not be here specified, medical reform has of late years much occupied the profession throughout Portugal, especially with reference to the recently-established medico-chirurgical schools in the metropolis. Among other questions which were lately discussed in the Portuguese Parliament, one was that of augmenting the number of medical professors at these establishments. Most of the mooted propositions were, however, so strongly opposed by conservative Coimbra University authorities, who have two members in the Chamber of Deputies, that various attempts made during several years proved unsuccessful. Still, lectures on legal medicine were instituted in a late session, and reformers confidently anticipate that further improvements will be enacted by the Legislature and carried out by Government.

(To be concluded.)

REVIEW IX.

1. *The Geological Evidences of the Antiquity of Man; with Remarks on Theories of the Origin of Species by Variation.* By SIR CHARLES LYELL, F.R.S. Second Edition, revised. Illustrated by Woodcuts.—*London*, 1863. pp. 528.
2. *The Antiquity of Man.* ('Edinburgh Review,' July, 1863.)
3. *Evidence as to Man's Place in Nature.* By THOMAS HENRY HUXLEY, F.R.S.—*London*, 1863. pp. 159.

WHATEVER drawbacks there may be to some persons in the practice of the "healing art," there can exist but very few, we should imagine, to the study of medicine as a general science. The only stumbling-blocks that we can think of are its necessitating a greater or less familiarity with the dead, and with decomposing bodies, with *foci* of infection, and with the excreta of the human machine. With some, no circumstance nor time can overcome the repulsiveness of these necessities. But most studies and professions have their unpleasant conditions, occasionally even duties of no little danger. And which of them can lay claim to the manifold attractions with which medicine, after all, allures the votaries in her train? Does not every study appear by the side of it imperfect, unsatisfying? The study of medicine alone gives a key to the mysteries surrounding us, and imparts to all a life we could not otherwise perceive. Of course, we use the term "medicine" in its wider sense—not in the simple application of its principles and experience, as the "*ars medendi*," but as that wide and all-embracing study which, beginning with physics, chemistry, and natural history, passes on to human and comparative biology, and appears finally adorned with such jewels of knowledge as the graver lore taught at the bedside of disease and death can alone impart. Herein lies the power of medicine over her disciples—viz., in her dealing with *so many*, as well as with the more recondite of nature's secrets. A man may be the profoundest lawyer, or the deepest philologist, the divinest artist, the most learned theologian; he may be the great warrior, navigator, engineer, and yet as either such simply he may walk abroad through creation and be deaf to more than half she utters. But let him have studied medicine as medicine *may* be studied, and he at once becomes free to the *arcana arcanissima* at his feet. He possesses more surely and extensively than any other man such a range and peculiarity of information as can vivify the world in a way to be vivified by no other one. So far as the pure botanist, pure chemist, pure anatomist, &c., are concerned, he cannot, of course, read such deep lessons in individual books of nature as can they. But he has this power, he can read something, often a great deal, in all of them, as well as in that, the most wondrous of all, and the most hidden to others—viz., the sybilline leaves of the body and mind in disease. Thus the man who comprehensively studies medicine becomes master of such a *passe partout*, that no other study can bestow. We have sometimes tried to think how we should have translated, or what kind of notion we should have formed of the strange acts and processes going on around us, had we not sat at the feet of the old man of Cos. Of the existence of a great number we should not have been conscious, it is true; but of those of whose presence we were aware, what should we have indeed thought? But we cannot now compass the idea of such an ignorance, having, thanks be to God, the key of knowledge in our hands.

All embracing as our department of knowledge is—various as are the formative sciences upon which it is based—there is undoubtedly a great difference as regards the nature and amount of help which the latter afford us in arriving at our culminating or practical effort—the alleviation of sickness and of disease. Some of these collateral branches can offer us but little, others are vital in the extreme. The former must be resigned in the propylœum; the latter accompany us into the adytum of the Æsculapian fane. But having left the latter, our novitiate passed, and having stepped out into the world with a little time to look about us ere we take our settled place, how many of us are there not who, remembering the charms of some of those fair handmaids of knowledge we left upon the temple steps ere we passed beneath its dome, return to them, single one out, and fly with her, and dwell with her for ever! He who was to have become the physician, the practitioner of the healing art, becomes instead botanist, chemist, or naturalist, &c., as the case may be. To such as remain true to their vows, becoming members of the profession of medicine in its strictest sense, the progress of these collateral branches of knowledge they were once grounded in, generally continues to be matter of considerable interest. The merest practitioner cannot hear of their novelties without some recognition of them; whilst to the more intellectual of the medical circle, a chief delight is to give such attention to them as the urgencies of active practice may permit. Little, in the majority of cases, no doubt, this is, and it would be often less were it not for such literary and scientific jackals like ourselves, whose duty it is to hunt out the lion's provender, and lay it before him, so that as little as possible of the monarch's time be uselessly spent. And this office we are now about to perform, believing that the intellectual banquet we shall provide will be worthy of attention. We cannot say it will be food for babes, but rather meat for strong men; yet, with all, there will be found a piquancy about it. And, so far, it is in accordance with the fashion of the time, for *sensation* is the order of the day. "Spiritualism" has to struggle for its own; it is pushed almost from its seat by the Aurora Floyds and Lady Audleys of feminine literature. In theology, there are 'Essays and Reviews,' Colenso's 'Enquiry,' and 'La Vie de Jesus' by Renan. Chemistry dazzles us with spectrum analysis. Astronomy startles us about the sun.* Engineers present us with a main-drainage scheme; Social Science with "woman's work;" Zoology with the gorilla; the theatres with ghosts; medicine with the renewal of life;

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* "I have still to advert to Mr. Naysmith's remarkable discovery that the bright surface of the sun is composed of an aggregation of apparently solid forms shaped like willow-leaves or some well-known forms of Diatomaceæ, and interlacing one another in every direction. The forms are so regular in size and shape as to have led to a suggestion from one of our profoundest philosophers of their being organisms possibly even partaking of the nature of life, but at all events closely connected with the heating and vivifying influences of the sun. These mysterious objects, which since Mr. Naysmith discovered them have been seen by other observers as well, are computed to be each not less than 1000 miles in length and about 100 miles in breadth. The enormous caverns in the sun's photosphere, to which we apply the diminutive term 'spots,' exhibit the extremities of these leaf-like bodies pointing inwards and fringing the sides of the cavern far down into the abyss. Sometimes they form a sort of rope or bridge across the cavern, and appear to adhere to one another by lateral attraction. I can imagine nothing more deserving of the scrutiny of observers than these extraordinary forms."—(The President's Address to the British Association for the Advancement of Science, September, 1863.) This apparent conversion of the sun into a cluster of glowworms or fireflies will no doubt be received with a degree of hesitation, considering that astronomers have at the same time announced that they have likewise just found that they have hitherto been wrong as regards the solar parallax. This they propose to increase so as to bring the earth closer to the sun by four million of miles, and to diminish the distances and dimensions of all the planets! (Hind, Stone, Hanson.)

and last, though not least, comes the geologist, with the 'Antiquity of Man.'

From the comprehensive character of the study of medicine, we become privileged to say that certain of the branches of knowledge by which the latter question is unravelled and tested fairly belong to the list of the collaterals of our own department. Since this question is a highly-interesting one, we intend availing ourselves of this privilege, and of devoting a few pages to a succinct account of it. The argument now *sub judice* may be expressed thus in a few words.

Does the first appearance of man upon the earth date back from incalculable ages, or not longer than a few (six) thousand years? But little more than half a century back, it was the general belief that the globe itself was not older than six thousand years, and that it, along with all living things upon its surface, was formed and fashioned in that period of time which we now reckon as a week. To doubt this was held at any rate to be equivalent to a disbelief in the fundamental doctrines of revealed religion. But men increased in knowledge, and it was found that such a literal chronology of the Mosaic writings as they had hitherto been endowed with was not God's gift, but man's, and that the space of time, both when the earth was originated and during which it was in process of formation, could be carried back and magnified to uncountable ages, without any disrespect to the intentionally vague information which the Infinite had vouchsafed concerning them. To satisfy, however, the "weaker brethren," it became necessary to distinctly indicate a method by which such a reconciliation could take place; and consequently several theories were propounded to bring about the harmony of geology and Genesis. These it is not our purpose, of course, to discuss; but it is not irrelevant to our present position to state that, whilst we hold it impossible that we shall ever be able to establish such a parallelism between the great characteristics of the Mosaic days and the palæontologic remains of geologic epochs as shall satisfy acuter intellects, yet that for all the purposes of the Christian apologist, we agree with those* who think the hypothesis usually associated with the name of Chalmers, and afterwards illustrated in the earlier writings of Hugh Miller, amply sufficient.

Dr. Chalmers, so early as 1804, had arrived at the conviction that

> "The writings of Moses do not fix the antiquity of the globe. If they fix anything, it is only the antiquity of the species. In the article on Christianity, this general assertion appears in a more distinct and intelligible form. When it is asked, 'Does Moses ever say that there was not an interval of many ages betwixt the first act of creation described in the first verse of the book of Genesis, and said to have been performed at the beginning, and those more detailed operations the account of which commences at the second verse? . . . Or does he ever make us to understand that the genealogies of man went any farther than to fix the antiquity of the species, and of consequence that they left the antiquity of the globe a free subject for the speculations of philosophers? . . . . It is not said when the *beginning* was. We know the general

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* See an article entitled "Genesis and Science," in North British Review for November, 1857.

impression to be that it was on the earlier part of the first day, and that the first act of creation formed part of the same day's work with the formation of light. We ask our readers to turn to that chapter, and to read the first five verses of it. Is there any forcing in the supposition that the first verse describes the primary act of creation, and leaves it at liberty to place it as far back as we may? that the first half of the second verse describes the state of the earth (which may already have existed for ages, and been the theatre of geological revolutions) at the point of time anterior to the detailed operations of this chapter, and that the motion of the Spirit of God described in the second clause of the second verse was the commencement of these operations?"[*]

In the 'First Impressions of England and its People,' by Mr. Hugh Miller, the above theory was further illustrated, though it was afterwards abandoned by the writer, in his 'Testimony of the Rocks.' From the former work we make the following extracts:

"Between the creation of the matter of which the earth is composed, as enunciated in the first verse, and the earth's void and chaotic state as described in the second, a *thousand* creations might have intervened. As may be demonstrated from even the writings of Moses himself, the continuity of a narrative furnishes no evidence whatever that the facts which it records were continuous. Take, for instance, the following passage: 'There went out a man of the house of Levi, and took to his wife a daughter of Levi. And the woman conceived and bare a son, and when she saw that he was a goodly child, she hid him three months. And when she could no longer hide him, she took for him an ark of bulrushes, and daubed it with slime and with pitch, and put the child therein, and she laid it in the flags by the river's brink.' The narrative here is quite as continuous as in the first three verses of Genesis. In the order of the relation, the marriage of the parents is as directly followed in the one case by the birth of a son as the creation of matter is followed in the other by the first beginnings of the existing state of things. . . . . We know, however, from succeeding portions of Scripture, that the father and mother of this child *had* several other children born to them in the period that intervened between their marriage and his birth. . . . . Had it been as necessary for the purpose of revelation that reference should have been made to the intervening creations in the one case as to the intervening births in the other, we doubtless would have heard of them too. . . . . it was not necessary at all. . . . The ferns and lepidodendra of the coal measures are as little connected with the truths which influence our spiritual state as the vegetable productions of Mercury or of Pallas; the birds and reptiles of the oolite, as the unknown animals that inhabit the plains or disport in the rivers of Saturn or Uranus. And so revelation is as silent on the periods and orders of systems and formations as on the relative positions of the earth and sun, or the places and magnitudes of the planets."[†]

Though acquiescence has now for some time been accorded to the belief that geology demonstrates the earth was not created only six thousand years ago, and that Revelation does not apodictically affirm it was so, any alteration of opinion with regard to the origin of man has not been generally admitted. He, it has been strenuously maintained, is a recent visitor. A few writers, it is true, have asserted the contrary, affirming that geology proved a more ancient origin of man, and that Revelation did not gainsay it. Even the orthodox and cautious author of the 'Researches into the Physical History of Mankind,'

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[*] Memoirs of the Life and Writings of Thomas Chalmers, D.D., &c., vi. pp. 386, 387.

[†] Op. cit., third edition, pp. 321, 323.

Dr. Prichard, agreed that there is not sufficient evidence of continuity in the genealogies of Genesis to afford either a computation of the age of the world or the assignment of a date in the creation of man. While the date of the arrival of Abraham in Palestine may be computed with a near approximation, "beyond that event," said Dr. Prichard, "we can never know how many centuries, *nor even how many chiliads of years*, may have elapsed since the first man of clay received the image of God and the breath of life." Such views were, however, generally regarded as unsupported by science and as opposed to Revelation. But at length they were urged by some with such sufficient force as to draw forth serious discussion. If, it was said in reply to them, man existed—as it is now asserted—in ages past along with at present extinct animals, myriads of whose remains have been discovered, how is it that we do not find traces of coëval man, or man in the fossil condition? We do find such, answered the recusants, and Donati, Germer, Rasoumouski, and Guetard asserted that human bones had been found intermixed with those of lost species of mammiferæ in several places. Some enthusiasts went almost to the extent of seeing human remains in every newly-discovered ossiferous treasure, whilst the majority still seemed determined that no such palæontologic curiosity should ever be found. The latter were like Diderot, when he said: "Si l'on venait de toutes partes me raconter qu'un mort se promène a Passy, je ne me derangerais pas pour l'aller voir," instead of going to see whether such a supposed miracle was not to be explained by somnambulism, galvanic excitation, or some other appreciable cause. A few, however, like Cuvier, made a scientific acquaintance with some of these strange fossils. Cuvier at once showed the *homo diluvii testis* of Scheuchzer to be a species of salamander; whilst some bones dug up near Lucerne, and described as those of a giant eighteen feet high, were demonstrated as elephantine. Even Spallanzani was proved to be wrong in supposing the osseous breccia of Arigo to contain human relics. But though time went on, the innovators refused to yield; they admitted such mistakes as the above had been made, but maintained that *all* examples were not to be explained away like them, nor, as in the well-known case of the Guadaloupe skeletons, with the supposition that though the bones were human they belonged to the recent or present period. Human remains, it was maintained, had been discovered in the cave at Kirkdale even so far back as 1786, and they were enumerated as found there by Dr. Buckland. They had been found at Meissen, at Durfort in the Jura, by M. Firmas, at Kostritz by Schlotheim, and elsewhere.[*] It was insinuated, also, that one-tenth part of the evidence produceable in substantiation of such facts would have sufficed to admit at once almost any other statement in general science. But although the humanity of many of these bones were admitted, as also their admixture with the fossil remains of extinct mammalia, Cuvier and Buckland shook their heads, and even Sir Charles Lyell, when visiting the collection of Dr. Schmerling, at

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[*] See The Natural History of the Human Species, by Lieut.-Col. Charles Hamilton Smith. Edinburgh, 1848.

Liege, in 1833, was incredulous about that which, in 1863, he admits as positive evidence in favour of the antiquity of man. The truth was, no one was willing to commit themselves to an opinion which was as yet scientifically as well as theologically heretical. The new school maintained its point, however, and brought forward further evidence, as it supposed, in the shape of the discovery of the remains of human art (such as flint implements or kelts) in deposits anterior to the recent period. Such things, it was averred, must have been fashioned by human agency, and yet they were found in close juxtaposition with the fossil bones of extinct pachydermata, and buried deep in strata or shut up in caves which did not present the least signs of having been disturbed since their first deposit or formation. Still, no general impression of there being truth in all this was made either upon the scientific or popular ear until 1858, when a new and intact bone cave being discovered at Brixham, about four miles south of Torquay, it was thought proper to have a thorough and systematic examination made of it. The Royal Society, incited by a memoir of Mr. Godwin-Austin,* made two grants towards defraying the expenses, and a committee of geologists was charged with the investigations, among whom Mr. Prestwich and Dr. Falconer took an active part, visiting Torquay while the excavations were in progress under the superintendence of Mr. Pengelly. The result of the Brixham examination was so striking as to induce Dr. Falconer to go to Sicily to examine certain ossiferous caverns there. On his way, in the autumn of 1858, he stopped at Abbeville to see the collection of M. Boucher de Perthes. An examination of the latter at once urged Dr. Falconer to beg Mr. Prestwich to thoroughly explore the geology of the valley of the Somme. Mr. Prestwich's report induced Mr. Flower and others to follow in his steps. Sir Charles Lyell was one of the number. Their conclusion has been, that not only do certain recent discoveries lead to the idea of the antiquity of man, but that many of the older and scouted ones bore truthful testimony of the same kind. Such, then, has been the force of evidence and weight of authority, that the facts relating to the co-existence of human remains and remains of human art with the bones of mostly extinct mammals, and the deposit of the remains of human art in ancient strata, could no longer be set aside. It has at length come to be admitted that there is such an amount and kind of evidence now before the scientific world as to demand that they be scrupulously but unprejudicedly examined, and the question of man's past duration on the globe no longer be repudiated. Sir Charles Lyell, in his well-known and very interesting work, places the most of this evidence before us. This, along with some other stores of information, we purpose analysing and laying our results before the reader.

The first and most direct testimony to the truth of the idea that man was an inhabitant of the earth in very far bygone ages, must be looked for in certain examples of the admixture of fossilized human bones with those of now extinct animals. Trustworthy examples of this kind are comparatively very rare. In the first place, the instances of mere admixture of such remains are very uncommon; and in the second place, when it exists, the conjunction may be explained generally, it has been thought, in a more satisfactory way than by supposing it to have existed from the time of the ancient *fauna*.

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* Transactions of the Geological Society, 2nd series, vol. vi. p. 444.

It is certainly somewhat remarkable, not only that we should be so deprived of the fossil remains of man, if he lived in long past ages, and that we should scarcely find a trace of his bones, associated with the comparatively numerous stores of what have been regarded as his handiwork—the flint instruments—but that we should miss them also under circumstances where we might naturally, geologically speaking, expect them. But the absence of them under the latter conditions, must naturally render us less surprised at the want of them under the former circumstances.

> "It is not many years since the Government of Holland resolved to lay dry that great sheet of water formerly called the Lake of Haarlem, extending over forty-five thousand acres. They succeeded, in 1853, in turning it into dry land. . . . . There had been many a shipwreck and many a naval fight in those waters, and hundreds of Dutch and Spanish soldiers and sailors had there met a watery grave. The population which lived on the borders of this ancient sheet of water numbered between thirty and forty thousand souls. In digging the great canal, a fine section had been laid open about thirty miles long of the deposits which formed the ancient bottom of the lake. . . . . Mr. Stering, who had been for some years employed by the Dutch Government in constructing a geological map of Holland, was my companion and guide. He informed me that he and his associates had searched in vain for human bones in the deposits which had constituted for three centuries the bed of the great lake. . . . . If history had been silent, and if there had been a controversy whether man was already a denizen of this planet at the time when the area of the Haarlem lake was under water, the archæologist, in order to answer this question, must have appealed, as in the case of the valley of the Somme, not to fossil bones, but to works of art imbedded in the imperfect strata." (Lyell, p. 147.)

Mr. MacAndrew and the late Edward Forbes followed in the experience of other dredgers, for they not only utterly failed in drawing up from the deep a single human bone, but scarcely ever met with a work of art, even after counting tens of thousands of shells and zoophytes collected on coast lines of several hundred miles in extent, and approaching within less than half a mile of land peopled by millions of human beings. For an interesting account of this portion of our subject, we must refer for further information to Sir Charles Lyell's work, and also to Colonel G. Greenwood's 'Rain and Rivers,' chapter xiii.

Many think, however, that more examples have been met with than have been brought forward, from fear of opposition or ridicule, and that the hitherto favourite mode of explaining the union is more far-fetched than the interpretation which the few would place upon it. To deny bones to be human, and to suppose some mistake, or to admit them to be so, but to maintain that they became accidentally mixed in recent times with the remains of older animals, is to some as satisfactory a solution of the matter as it is repugnant to others.

There cannot be a doubt, however, that such has really been the key to some of the palæontologic puzzles, but the question remains: Are there not other instances of the union of such reliquiæ which do not permit of such explanation? High authorities are now inclined to answer in the affirmative. Of such instances we may refer to the following. Thirty years back, Dr. Schmerling, of Liège, a skilful anatomist and palæontologist, in the course of a very careful exploration of the ossiferous caverns bordering the valleys of the Meuse, more than once disinterred human bones in association with the bones of extinct species of bears, elephants, and rhinoceroses, and of certain present-existing creatures. All the bones were of the same colour and condition as to their amount of animal matter, and the human bones were so rolled and scattered as to preclude all idea of their having been intentionally buried on the spot. As no gnawed bones nor coprolites were found by the discoverer, he inferred that the caverns of Liège had not been the dens of wild beasts. The conclusion was that their organic and inorganic contents had been swept into them in ages past by streams communicating with the surface of the country, and that the periods of the life of the former could not have been separated by any long interval.

> "Some rude flint implements of the kind commonly called knives or flakes, of a triangular form in the cross section were found by Schmerling dispersed generally through the cave mud, but he was too much engrossed with his osteological inquiries to collect them diligently. . . . . He also discovered in the cave of Chokier, two and a half miles south-west from Liège, a polished and jointed and needle-shaped bone, with a hole pierced obliquely through it at the base, such a cavity, he observed, as had never given passage to an artery. This instrument was imbedded in the same matrix with the remains of a rhinoceros. . . . . Although in some forty fossiliferous caves explored by him human bones were the exception, yet these flint instruments were universal, and he added, that 'none of them could have been subsequently introduced, being precisely in the same position as the remains of the accompanying animals. I therefore,' he continues, 'attach great importance to their presence, for even if I had not found the human bones under conditions entirely favourable to their being considered as belonging to the antediluvian epoch, proofs of man's existence would still have been supplied by the cut bones and worked flints.'" (Lyell, p. 66.)

These discoveries of the Belgian naturalist, so far at least as the import of the human bones and instruments went, were disregarded by the scientific world, and continued so until the memoir of Mr. Prestwich appeared relative to certain researches at Amiens and Abbeville, to be presently referred to. Sir Charles Lyell himself, who saw Schmerling's collection in 1833, expressed, he tells us, "some incredulity respecting the alleged antiquity of the fossil human bones." (p. 68.)

> "One positive fact, it will be said, attested by so competent a witness ought to have outweighed any amount of negative testimony previously accumulated respecting the non-occurrence elsewhere of human remains in formations of the like antiquity. In reply I can only plead that a discovery which seems to contradict the general tenor of previous investigations is naturally received with much hesitation." (p. 68.)

Soon after the publication of the memoir alluded to, Sir Charles again visited Liège, and examined along with Professor Malaise some of the caverns which still remained, and the latter, continuing the investigation after the departure of Sir Charles, found at the depth of two feet below a crust of stalagmite, three fragments of a human skull and two perfect lower jaws with teeth, all associated in such a manner with the bones of bears, large pachyderms, and ruminants, and so precisely resembling these in colour and state of preservation as to leave no doubt in his mind that man was contemporary with the extinct animals. In these caverns the decay and decomposition of the fossil bones appear to have been arrested by a constant supply of water charged with carbonate of lime, which dripped from the roofs while the caves were becoming gradually filled up. By similar agency the mud, sand, and pebbles were usually consolidated. Amongst the portions originally discovered by Dr. Schmerling was a particular skull, now known as the Engis skull, and in the Museum of Liège, existing in such a state of integrity as to enable the anatomist to speculate on its race. It was buried five feet deep in a breccia along with the tooth of a rhinoceros, several bones of a horse, of a reindeer, and of certain ruminants. This skull has since become famous, and either directly or by means of casts, photographs, and engravings, has been studied by the *savans* of Europe. The discoverer himself thus wrote concerning it: "For my own part, I hold it to be demonstrated that this cranium has belonged to a person of limited intellectual faculties, and we conclude thence that it belonged to a man of a low degree of civilization; a deduction which is borne out by contrasting the capacity of the frontal with that of the occipital region." This quotation is made by Professor Huxley from Dr. Schmerling's 'Recherches sur les Ossements Fossiles,' &c., and he tells us likewise that upon the high authority of Sir Charles Lyell, he takes it for granted

> "That the Engis skull belonged to a contemporary of the mammoth (*Elephas primigenius*), and of the woolly rhinoceros (*Rhinoceros tichorhinus*), with the bones of which it was found associated;" and that it "takes us to at least the farther side of the vague biological limit which separates the present geological epoch from that which immediately preceded it; and there can be no doubt that the physical geography of Europe has changed wonderfully since the bones of men and mammoths, hyænas, and rhinoceroses were washed pell-mell into the cave of Engis." (p. 121.)

In the early part of the year 1857 a human skeleton was discovered in a limestone cave in the Neanderthal, near Hochdal, between Düsseldorf and Elberfeld. It is supposed that, when first discovered, the skeleton was complete, but that the workmen, ignorant of its value, scattered and lost most of the bones, preserving the larger ones only. No other animal remains were found with it, but three years afterwards the tusk of a bear was disinterred from a lateral embranchment of the cave, though whether it was referrible to a recent or extinct species of bear, Sir Charles Lyell could not determine. The skull, and indeed, all that remains of the skeleton, have on account of the peculiar formation of the bones, lately excited very much discussion. The former was covered both on its outer and inner surface, and especially on the latter, with a profusion of dendritical crystallizations, and some of the other bones were marked in a similar way. The bones had lost so much of their animal matter as to adhere strongly to the tongue, agreeing in this respect, according to Sir Charles Lyell, "with the ordinary condition of the fossil remains of the post-Pliocene period." (p. 78.) The great point of interest, however, in connexion with this skeleton is, that it is thought by many to exceed all other human forms in those peculiarities of cranial development and osseous protuberances, &c., which mark a barbarous savage or degraded animal-like race. From this "degraded character" of the skull and its approximation to the cranial development of the chimpanzee, together with the great thickness of the bones, the great development of all the elevations and depressions for the attachment of muscles, and the unusually-rounded shape and abrupt curvature of some of the ribs, a few have assumed this skeleton to be one of an antique race more human than any anthropoïd ape we know of now, and more pithecoïd than any human race existing at present. Professor King, at the last meeting of the British Association for the Advancement of Science, read a paper on "The Neanderthal Skull," in which

> "He gave reasons for believing it to belong to the Clydian period, and to be specifically distinct from man. He contended that the Neanderthal man was living in the concluding division of the Glacial or Clydian period. . . . . Why may there not," said Professor King, "have been a Pliocene or Clydian species possessed of no higher faculties than such as would enable it to erect a protecting shed, fashion a stone for special purposes, or store up food for winter, but like the gorilla or chimpanzee, be devoid of speech, and equally unconscious of the existence of a Godhead? Man's psychical endowments are visibly expressed in the prominent frontal and elevated vertex of his cranium.
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> "But considering that the Neanderthal skull is eminently simial in its great characters, I feel myself constrained to believe that the thoughts and desires which once dwelt within it never soared beyond that of the brute. . . . . Psychical gifts of a lower grade than those characterizing the Andamans cannot be conceived to exist—they stand next to brute benightedness. Applying the above argument to the Neanderthal skull, and considering its close resemblance to that of the chimpanzee, and moreover, knowing that the simial peculiarities are unimprovable—incapable of moral and theistic conceptions—I see no reason to believe otherwise than that similar darkness characterized the being whom I do not hesitate to call *homo Neanderthalensis.*"

Sir Charles Lyell observes:

> "There is doubtless, as shown in the diagram (fig. 4), a nearer resemblance in the outline of the Neanderthal skull to that of a chimpanzee than had ever been observed before in any human cranium, and Professor Huxley's description of the occipital region shows that the resemblance is not confined to the mere excessive prominency of the superciliary ridges. The direct bearing of the ape-like character of the Neanderthal skull on Lamarck's doctrine of progressive development and transmutation, or on that modification of it which has of late been so ably advocated by Mr. Darwin, consists in this, that the newly-observed deviation from a normal standard of human structure is not in a casual or random direction, but just what might have been anticipated, if the laws of variation were such as the transmutationists desire. For if we conceive the cranium to be very ancient, it exemplifies a less advanced stage of

progressive development and improvement. If it be a comparatively modern race, owing its peculiarities of conformation to degeneracy, it is an illustration of what the botanists have called 'atavism,'. or the tendency of varieties to revert to an ancestral type, which type, in proportion to its antiquity, would be of a lower grade." (p. 92.)

In the year 1852, a labourer employed in mending the roads near Aurignac, of the Haute Garonne, not far from the Pyrenees, observed that rabbits, when hotly pursued, ran into a certain hole on the side of the hill. He put his arm into this hole, and pulled out one of the long bones of a human skeleton. Further inquiry showed a cave to exist there containing bones that must have formed parts of not less than seventeen skeletons, of different sexes and various ages; some so young, that the ossification of particular bones was incomplete. The mayor ordered all of the remains to be re-interred in the parish cemetery. No further heed was given to the matter until M. Lartet visited Aurignac, in 1860, and determined to investigate systematically what remained of the deposits both outside and inside the cave. The village sexton was unable, unfortunately, to inform M. Lartet in what exact spot the trench was dug into which the skeletons removed in 1852 had been thrown. Outside the great slab of stone forming the door of the cave not one human bone occurred. In the substratum of the inside which remained, after the skeletons had been taken away, were found about ten detached human bones, including a molar tooth; and M. Delesse ascertained, by careful analysis of one of these, as well as of the bones of a rhinoceros, bear, and some other extinct animals, that they all contained precisely the same proportion of azote, or had lost an equal quantity of their animal matter. In this substratum was also found the tusk of a young *Ursus spelæus*, the crown of which had been stripped of its enamel, and had been carved into apparently the shape of a bird's head. It was perforated lengthwise, as if for suspension as an ornament or amulet. A flint knife also was found in the interior, which had evidently never been used. There was no stalagmite in the grotto; and M. Lartet came to the conclusion that all the bones and soil found in the interior had been artificially introduced. Outside the grotto he found a

"Layer of ashes and charcoal about seven inches thick, extending over an area of six or seven square yards, and going as far as the entrance of the grotto, and no farther, there being no cinders or charcoal in the interior. Among the cinders outside the vault were fragments of fissile sandstone, reddened by heat, which were observed to rest on a levelled surface of nummulitic limestone, and to have formed a hearth. The nearest place from whence such slabs of sandstone could have been brought was the opposite side of the valley. Among the ashes, and in some overlying earthy layers separating the ashes from the talus, were a great variety of bones and implements. . . . Among other articles outside the entrance was found a stone of a circular form, and flattened on two sides, with a central depression composed of a tough rock, which does not belong to that region of the Pyrenees. This instrument is supposed, by the Danish antiquaries, to have been used for removing, by skilful blows, the edges of flint knives, the fingers and thumb being placed in the two opposite depressions during the operation. Among the bone instruments were arrows without barbs, and other tools made of reindeer horn, and a bodkin formed out of the more compact horn of the roedeer. This instrument was well shaped and sharply pointed, and in so good a state of preservation, that it might still be used for piercing the tough skins of animals." (p. 184.)

Scattered through the same ashes and earth at the entrance were the osseous remains of the mammoth, Siberian rhinoceros, cave-bear, cave-hyæna, gigantic Irish deer, aurochs, &c. &c.:

"The bones of the herbivora were the most numerous, and all those on the outside of the grotto which had contained marrow were invariably split open, as if for its extraction, many of them being also burnt. The spongy parts, moreover, were wanting, having been eaten off and gnawed after they were broken—the work, according to M. Lartet, of hyænas, the bones and coprolites of which were plentifully mixed with the cinders, and dispersed through the overlying soil." (Lyell, p. 186.)

"We can scarcely doubt that we have here an example of an ancient place of sepulture, closed at the opening so effectually against the hyænas or other carnivora, that no marks of their teeth appear on any of the bones, whether human or brute. (p. 188.) . . . . These beasts of prey are supposed to have prowled about the spot, and fed on such relics of the funeral feast as remained after the retreat of the human visitors, or during the intervals between successive funeral ceremonies which accompanied the interment of the corpses within the sepulchre. (p. 186.) . . . . If the fossil memorials have been correctly interpreted—if we have here before us, at the northern base of the Pyrenees, a sepulchral vault, with skeletons of human beings consigned by friends and relatives to their last resting-place—if we have also at the portal of the tomb the relics of funeral feasts, and within it indications of viands destined for the use of the departed, on their way to a land of spirits, while among the funeral gifts are weapons wherewith, in other fields, to chase the gigantic deer, the cave-lion, the cave-bear, and woolly rhinoceros—we have at last succeeded in tracing back the sacred rites of burial, and, more interesting still, a belief in a future state to times long anterior to those of history and tradition." (p. 192.)

Among the fossil remains of man which have been put forward with claims of very high antiquity may be mentioned "the fossil-man of Denise," said to have been found in a volcanic breccia near the town of Le Puy-en-Velay, in Central France, and the fossil human bone of Natchez, on the Mississippi, supposed to have been derived from a deposit containing the remains of the mastodon and of the megalonyx. But no fossil bone has become more famous than the "jaw-bone of Moulin Quignon." On the 28th of March last, whilst some workmen were engaged in the gravel-pits of Moulin Quignon, near Abbeville, they discovered first a human tooth and then a jaw-bone. These bones were found in a deposit containing flint instruments of antique type and the bones of elephants. Controversy had long been going on—as we shall presently see—respecting the flint implements; and one reason why certain persons considered the latter to have been formed by nature, and not by art, was, that no human bones had been met with near them. Sir Charles Lyell remarked:

"It is naturally a matter of no small surprise that after we have collected many hundred flint instruments (including knives, many thousands) not a single human bone has yet been met with in the alluvial sand and gravel of the Somme . . . . That ere long, now that curiosity has been so much excited on this subject, some human remains will be detected in the older alluvium of European valleys I confidently expect." (pp. 144, 145.)

Scarcely had this been written when a jaw-bone was found in the "black-band flinty gravel" of the Somme valley beds! To it we shall return presently. The last examples of this kind of testimony to the antiquity of man to which we shall refer, are to a few human remains found in the peculiar loamy deposits commonly called "loess," of the basins of the Rhine, Danube, and some other large rivers, draining the Alps, and which extend down the Rhine into the low countries. In 1823, M. Ami Bonè,

> "Well known by his numerous works on geology, and a well-practised observer in every branch of the science, disinterred with his own hands many bones of a human skeleton from ancient, undisturbed 'loess,' at Lahrr, nearly opposite Strasbourg, on the right side of the great valley of the Rhine. No skull was detected, but the tibia, fibula, and several other bones, were obtained in a good state of preservation, and shown at the time to Cuvier, who pronounced them to be human." (p. 338.)

In some period between 1815 and 1823, a human lower jaw, with teeth, was found along with the molars, tusks, and bones of elephants, in a terrace of gravel, covered with "loess," on the right bank of the Meuse, at Maestricht. The jaw was deposited at nineteen feet from the surface. The stratum is said to have been intact and undisturbed; and Sir Charles Lyell, who visited the site in 1860, where these fossils were found, states that he "could see no reason for suspecting the human jaw to belong to a different geological period from that of the extinct elephant." With other illustrations, such as the "cavern of Bize,"* the "Mickleton tunnel skeleton;"† and the "Muskham and Hanbury Burn cave remains,"‡ we need not detain the reader, as they are evidently out of court. What objections are to be urged against those we have referred to we shall state after having gone over the other forms of evidence in favour of the antiquity of man. The next to be noticed is that afforded by the remains of human art—such as "flint implements," imbedded in alluvial gravels or fluviatile drifts, and in caves of limestone, along with the fossil bones of the mammoth hippopotamus, rhinoceros, and other extinct animals. The existence of these rude implements on the floors of limestone caverns along with the fossil bones of long since lost species, has been occasionally referred to for more than half a century. But since it appeared that no precise geological time could be affixed to the incrustating deposit forming the floors of these caverns, the mere admixture itself of the remains in it did not necessarily determine that the things so mixed had been contemporaneous. Hence here, as under other circumstances, the testimony afforded by caverns was read with very great suspicion. But the results of the examination of the Brixham cave, in 1858, were such as to lead several authorities to attach not only a very high probable value to its own apparent teachings, but to some of the cave-evidence which had been so unscrupulously rejected. At Brixham,

> "No human bones were obtained anywhere during these excavations, but many flint knives, chiefly from the lowest part of the bone-earth, and one of

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* Lyell, p. 59. † Edin. New Philosoph. Journal, April, 1856.
‡ Trans. Ethnological Society, 2nd series, 1863.

> the most perfect, lay at the depth of thirteen feet from the surface, and was covered with bone-earth of that thickness. . . . . About fifteen knives, recognised as artificially formed by the most experienced antiquaries, were taken from the bone-earth, and usually near the bottom. . . . . The anteriority of those at Brixham to the extinct animals is demonstrated not only by the occurrence at one point in overlying stalagmite of the bone of a cave-bear, but also by the discovery at the same level in the bone-earth, and in close proximity to a very perfect flint tool, of the entire left hind leg of a cave-bear. . . . . Every bone was in its natural place, the femur, tibia, fibula, ankle-bone, or astragalus, all in juxtaposition. Even the patella, or detached bone of the knee-pan, was searched for and not in vain. Here, therefore, we have evidence of an entire limb not having been washed in a fossil state out of an older alluvium, and then swept afterwards into a cave so as to be mingled with flint implements, but having been introduced when clothed with its flesh, or at least when it had the separate bones bound together by their natural ligaments, and in that state buried in mud. If they were not all of contemporary date, it is clear from this case and from the humerus of the *Ursus spelæus* before cited, as found in a floor of stalagmite, that the bear lived after the flint tools were manufactured, or, in other words, that man in this district preceded the cave-bear." (p. 101.)

But such cave evidence even as this has sunk into some neglect in comparison with the importance which is attached to the occurrence of the same remains of human art in beds of gravel. So long back as 1797, Mr. J. Frere drew attention to the discovery of some flint instruments in a bed of gravel twelve feet from the surface at Hoxne, in Suffolk. Above the gravel lay, in a sandy bed with shells, the jaw-bone and teeth of an enormous unknown animal. Not much regard was paid to this discovery, however. But in 1847, M. Boucher de Perthes announced that he had found, since 1841, flint implements in the lowest beds of a series of ancient alluvial strata bordering the Valley of the Somme, which geologists had termed "diluvium." Still, as Sir Charles Lyell expresses it, the scientific world had no faith in the statement that works of art, however rude, had been met with in undisturbed beds of such antiquity. A few years later, MM. Rigollot and Buteux corroborated M. Boucher's statement. Little attention was accorded, nevertheless, to these discoveries in the Valley of the Somme, until the investigation of the Brixham Cave before mentioned, at which Dr. Falconer assisted. Struck by the discoveries here, this gentleman left for Sicily to pursue further inquiries in certain ossiferous caverns, and stopped at Abbeville on his road, examining the collection of M. Boucher. Being satisfied that the flints called "hatchets," in possession of the latter, had really been fashioned by the hand of man, Dr. Falconer wrote to Mr. Prestwich, urging him to explore the geology of the Valley of the Somme. This Mr. Prestwich did, in company with Mr. John Evans, of the Society of Antiquaries. He found the gravel beds of St. Acheul, capping a low chalk hill a mile S.E. of Amiens, more than one hundred feet above the level of the Somme, and not commanded by any higher ground. The upper beds consisted of about from ten to fifteen feet of brown brick-earth, containing many old tombs and some coins, but without organic remains; under this was a whitish marl and sand, with recent shells, mammalian bones and teeth whose thickness varied from two to eight feet; while, lastly, there was found from six to twelve feet of coarse, subangular flint gravel (identical with the gravel of East Croydon, of Wandsworth Common, and other places), with remains of shells in sand, and the teeth and bones of the elephant, horse, ox, and deer. With these were found the worked flints in considerable numbers. The whole deposit rested on chalk. At Menchecourt, near Abbeville, along with the remains of two extinct deer, an extinct species of horse, of the mammoth and tichorine rhinoceros, flint implements were discovered at depths varying from sixteen to twenty-two feet. Before Mr. Prestwich's return, he succeeded, says Sir Charles Lyell—

> "In dissipating all doubts from the minds of his geological friends, by extracting with his own hands from a bed of undisturbed gravel at St. Acheul, a well-shaped flint hatchet. . . . . There were no signs of vertical rents in the enveloping matrix, nor in the overlying beds of sand and loam, so that it was impossible to imagine that the tool had gradually worked its way downwards, as some had suggested, through the incumbent soil into an older formation. . . . . Mr. Flower, who accompanied Mr. Prestwich on his second excursion to St. Acheul in June, 1859, succeeded, by digging into the bank of gravel, in disinterring at the depth of twenty-two feet from the surface, a fine symmetrically-shaped weapon of an oval form lying in and beneath strata, which were observed by many witnesses to be perfectly undisturbed." (p. 103.)

Sir Charles himself shortly afterwards visited the same pits and obtained seventy flint tools, one of which was taken out while he was present, though he confesses that he did not see it before it had fallen from the matrix. He afterwards, in the same year, expressed his opinion to the British Association in favour of the antiquity of the flint tools. M. Pouchet, who was subsequently commissioned by the municipality of Rouen, saw a hatchet extracted from the gravel in its natural position. MM. Gundry, Garnier, and two others, afterwards went over the ground previously traversed by the above investigators. The former, in his Report to the French Academy, stated:

> "The great point was not to leave the workmen for a single instant, and to satisfy one's-self by actual inspection, whether the hatchets were found *in situ*. I caused a deep excavation to be made, and found nine hatchets most distinctly *in situ*, in the diluvium associated with teeth of *Equus fossilis*, and a species of *Bos*, different from any now living, and similar to that of the diluvium and of caverns."

We may add to this, that still more recently M. Desnoyers has followed M. Gundry, and with analogous results. The alluvium of the Valley of Somme exhibits, according to most geologists, nothing extraordinary or exceptional in its position or its external appearance. It is of the post-Pliocene period, similar in arrangement and composition of materials, and in its organic remains with the drift in numerous other valleys of France and England. The claim of these gravels to particular attention is due purely to their containing the "flint instruments." Many of the latter are said to be not at all unlike some stone implements used to this day, as hatchets and tomahawks, by natives of Australia. Of them Professor Ramsay remarks: "For more than twenty years, like others of my craft, I have daily handled stones, whether fashioned by nature or art, and the flint hatchets of Amiens and Abbeville seem to me as clearly works of art as any Sheffield whittle."*

Professor Ansted thus expresses himself in a contemporary journal:

> "Concerning the various weapons or tools, or whatever else the human remains buried with hyænas' and bears' bones may have been, one fact is very significant—namely, that in all parts of the world—in England and France, Germany and Italy, Russia and Scandinavia, everywhere, in a word, throughout Europe—these remains, wherever found, are in all essentials the same, and are not unfrequently of foreign material. This of itself is interesting; but when we find that from the interior of India and China, the banks of the Mississippi, and the vast plains of South America, specimens of sculptured stone are obtained always precisely similar; that the jade of the East is mixed up in caverns and gravel with flints from Western Europe, and with greenstones from America, and that even the northern parts of Australia and Madagascar appear to contain examples of manufacture differing from these nothing in style and little in material—we are reminded pointedly of the original unity of the human race, and we see that an undetermined question of time forms the only serious difficulty interfering with the reception of one of the most startling innovations resulting from modern geological investigation."

When found, some of the Somme implements are of an ochreous-yellow colour, others are white or brown, according to the colour of their immediate matrix. The surface of many is encrusted with a film of carbonate of lime, while others are marked by those ramifying crystallizations called dendrites. But for fuller information on these points, and on the shapes, sizes, purposes, &c., of the "flint implements," we must be content to refer to the pages of Sir Charles Lyell. Some of these curious tools have also been discovered in this country in the ancient fluviatile gravel of the valley of the Ouse, near Bedford, and in a bed of gravel in the valley of the Lark, below Bury St. Edmunds. In connexion with these Sir Charles observes:

> "One step, at least, we gain by the Bedford sections which those of Amiens and Abbeville had not enabled us to make. They teach us that the fabricators of the antique tools, and the extinct mammalia coeval with them, were all post-Glacial, or, in other words, posterior to the grand submergence of central England beneath the waters of the glacial sea." (p. 166.)

Nor has the valley of the Thames been absolutely barren of them, even in association with the skeleton of the elephant. The valley of the Wey (with so ancient a drift that one part of it had been disturbed and tilted before another part was thrown down), has also yielded at least one specimen. Before we pass to the next kind of testimony to the antiquity of man, this may be the appropriate place to notice, that M. Desnoyers has recently communicated to the French Academy the description of a series of markings upon fossil bones discovered near Chartres, and which he attributes in great part to the action of flint implements. Many of these "striæ gashes and incisions" on the ancient mammalian bones have the same appearance, and must have the same origin as those met with on more modern bones—viz., the action of human weapons—whilst other striæ of a finer and more rectilinear character, and which intersect each other, seem to be analogous to those seen on blocks and pebbles which have been scratched and polished by the action of glaciers. From these and associated facts alluded to by M. Desnoyers in his Report, he concludes that man lived upon the French soil before the great and first glacial period (at the same time as *Elephas meridionalis* and the other Pliocene species characteristic of the Val d'Arno, in Tuscany), and that he was contemporary with these huge animals anterior to *Elephas primigenius* and the other mammals whose *débris* have been associated with the remains or indications of man in the gravels or quarternary beds of great valleys and in caverns.

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* Athenæum, 1859.

The last description of evidence we have to refer to is that obtained from the old mud deposits and alluvial plains of large river deltas, such as those of the Mississippi, Nile, &c. In these, the remains of human art, and even human bones, have been stated to have been found at very great depths below the surface. The deposits being assumed to be thrown down with a certain degree of regularity or measurable rate in time and amount, the depths from which such remains have been withdrawn, afford a clue to the length of time which has been passed in thus covering them over. Mr. Horner's researches in the delta of the Nile afford a prominent illustration. At this spot historical momuments of great antiquity exist, originally built upon the mud of the river, and which have been since covered over, or partially hidden by such a thickness of deposit as belongs to the time which has progressively elapsed. The obelisk at Heliopolis, e.g., is thought to have been erected two thousand three hundred years B.C. It is now buried nearly twelve and a half feet, and of which but sixteen and a half inches are supposed to indicate the portion originally sunk. Thirty miles from the apex of the delta is Memphis, a city believed to have been constructed four thousand years B.C. M. Girard, of the French expedition, calculated the average rate of increase of Nile mud on the plain between Assouan and Cairo, to be five English inches in one hundred years. By Mr. Horner this rate is reduced to about three and a half inches; whilst M. Rosiere estimates the mean rate of deposit at two inches and three lines in a century. It is generally admitted, however, that whatever the actual rate may be, the vast accumulation of mud forming the delta of the Nile has been deposited at the rate of only a few inches in one hundred years. In this mud numerous borings were made at the suggestion of Mr. Horner, partly at the expense of the Royal Society, and partly at that of the late Viceroy Abbas Pacha. For the first sixteen or twenty-four feet, jars, vases, pots, a small human figure in burnt clay, a copper knife, and other entire articles were brought up; but when water soaking through from the Nile was reached, the boring instrument employed was too small to allow of more than fragments of works of art being brought to the surface. But pieces of burnt brick and pottery were extracted almost at every spot, and from all depths, even where the sinking was sixty feet below the surface, towards the central parts of the valley, and in none of these cases did the borers get to the bottom of the alluvial soil.

> "Were we to assume six inches in a century, the burnt brick met with at a depth of sixty feet would be 12,000 years old. Another fragment of red brick was found by Linaut Bey in a boring seventy-two feet deep. . . . . Were we to take two-and-a-half inches, a work of art seventy-two feet deep must have been buried more than 30,000 years ago." (Lyell, p. 38.)

According to Sir Charles Lyell, the lowest estimate of time required to form the existing delta of the Mississippi would be probably more than 100,000 years. In a portion of it, near New Orleans, at the depth of sixteen feet from the surface, and beneath four buried forests superimposed one upon the other, some charcoal and a human skeleton (the cranium of which is said to belong to the aboriginal type of the Red Indian race) are asserted to have been found. From particular chronological calculations, Dr. Dowler has ascribed to these remains an antiquity of 50,000 years. In a calcareous conglomerate, forming part of the coral reefs of Florida, supposed by Agassiz (in accordance with his mode of estimating the rate of growths of these reefs) to be about 10,000 years old, some fossil human remains are said to have been discovered by Count Portalis. But we are now getting quite into the cloudland of palæontologic history, and it is time to inquire what criticism has to offer on the evidence upon which most stress is likely to be placed.

If a geologist were asked what would be the least trustworthy stratigraphical conditions under which an admixture of human bones and remains of art with the bones of extinct animals being found, inferences could be drawn as to their contemporaneous existence, he would point out exactly such as have been pressed into the modern service. True it is we have no other, and that in itself is suspicious. Such geologist would say to an enthusiastic novice, "Be very cautious how you reason upon what you may find mixed up together in limestone caves and in beds of gravel." And why so? Because in the bone mud of caverns are to be found local and accidental accretions, disconnected with ordinary geologic causes and devoid of definite position in the recognised strata of the globe. Because the gravel or alluvial drift, upon which so much stress is laid, may have been formed out of some older gravel, and from which the remains of the ancient animals may have been procured. The caves, too, may have served as the channels through which the waters of occasional land-floods, or engulfed rivers have flowed, so that the remains of living beings which have peopled the district at more than one era may have subsequently been mingled in such caverns, and confounded together in one and the same deposit. *Mere juxtaposition* of fossils, as proof of contemporaneity, must, under any circumstances, be reasoned upon with the utmost caution, but juxtaposition in the stalagmitic crusts and breccia of ossiferous caves and in beds of gravel requires more than ordinary circumspection in its use.

> "The convulsions and revolutions of the geological world," says Hugh Miller, in his 'Old Red Sandstone,' "like those of the political, are sad confounders of place and station, and bring into close fellowship the high and the low; nor is it safe in either world—such have been the effects of the disturbing agencies—to judge of ancient relations by existing neighbourhoods, or of original situations, by present places of occupancy."

The ossiferous caverns have in many instances remained accessible ages after the mud deposits of their floors were formed, and some may have served for occasional concealment or shelter down even to comparatively modern times; at all events, as the writer in the 'Edinburgh Review' remarks, they

> "Were tenanted for long periods by successive races, whether of animals or men, and the record of their antiquity was not, as in the case of strata, geologically superimposed, sealed up, and verified by a succession of later deposits." (Op. cit.)

In the interesting papers on "Caverns and their Contents," by Professor Ansted, recently appearing in the 'Popular Science Review,' the author writes:

> "In countries little cultivated, and where wild animals are common in the adjacent forests, the bear, the hyæna, and some other beasts of prey, occupy caverns as dens, or use them either as larders or as burial-places. Sometimes they bring in and deposit there the carcases of their victims; sometimes they would seem to retire there to die. The skeletons and bones accumulated from either of these habits are not unfrequently heaped in quantities almost incredible, and they are sometimes mixed with and sometimes coated with recently-formed stalagmite. Elsewhere bones, shells, and various remains of animals, have been washed into caverns on the occasion of some unusual flood, or have fallen in from above with stones, boulders, or angular fragments of rocks." (Op. cit., July, 1862.)

What is true to-day was true yesterday. In other instances such caves have been found to communicate with the surface by narrow vertical or oblique fissures, the upper extremities of which have become choked up with sand and gravel. There is thus little difficulty in understanding how much of the materials, organic and inorganic, now filling the cave may have been washed into it through such fissures, and subsequently consolidated by a constant supply of water, charged with carbonate of lime, dripping from the roof during their gradual accumulation. In the ninth edition of his 'Principles of Geology,' Sir Charles Lyell writes as follows:

> "After giving no small weight to the arguments of M. Desnoyers and the writings of Dr. Buckland on the same subject, and visiting several caves in Germany, I came to the opinion that the human bones mixed with those of extinct animals in osseous breccia and cavern-mud in different parts of Europe were probably not coëval. The caverns having been at one period the dens of wild beasts, and having served at other times as places of human habitation, worship, sepulture, concealment or defence, one might easily conceive that the bones of man and those of animals which were strewed over the floors of subterranean cavities, or which had fallen into tortuous rents, connecting them with the surface, might, when swept away by floods, be mingled in one promiscuous heap in the same ossiferous mud or breccia." (Op. cit., p. 740.)

That such intermixtures have really taken place, and that geologists have been deceived by them, and assigned to one and the same period fossils which had been introduced at successive times, all unprejudiced observers are now willing to allow. But the question remains, whether there are not certain examples—such as those we have referred to—in which surrounding conditions and circumstances tend to prove that the caves have been undisturbed from the first, and that what has been discovered in them has been buried in undisturbed loam or clay, beneath a crust of stalagmite that must have been formed subsequently to their introduction, the hypothesis of sepulture being quite inapplicable. Sir Charles Lyell and other eminent geologists now answer in the affirmative, and believe that there is *cave* as well as other evidence to show that man and the mammoth coëxisted. The Brixham cave was examined with such precautions as might free it from the objections urged against its predecessors. It is thought to have come out of the ordeal successfully, and to show also that it is probable that if the depositories explored by Schmerling, McEnery, and others, had now to be opened, and with the same care, they would offer valid testimony. The time has passed for this, however, and hence we are justified in accepting them only as very qualified witnesses. As regards the interesting cavern of Aurignac, it has been objected that M. Lartet was personal witness to only a portion of the facts which he relates, eight years having passed since it was first opened to when he visited the locality. In the second place, it would seem to prove *too* much, for by it we are carried back either to the sacred rites of burial, to the relics of the funeral feasts, and to the very portal of the tomb of a people living innumerable cycles before Œdipus or Priam, before Tyre, before Memphis, before the Flood, or are landed simply on the shore of a comparatively very recent historic time. Who shall determine which? Cautious and sceptical in our mood, we shall believe the latter; but when speculative and poetical, we shall exclaim, in De Quincey's apostrophe to opium as to geology:

> "O just and righteous opium—*geology*—that to the chancery of dreams summonest, for the triumphs of despairing innocence, false witnesses, and confoundest perjury, and dost reverse the sentences of unrighteous judges—thou buildest on the bosom of darkness, out of the fantastic imagery of the brain, cities and temples beyond the art of Phidias and Praxiteles, beyond the splendours of Babylon and Hekatompylos, and from 'the anarchy of dreaming sleep' callest into sunny light the faces of long-buried beauties and the blessed household countenances cleansed from the 'dishonours of the grave.' Thou only givest these gifts to man, and thou hast the keys of Paradise, O just, subtle, and mighty opium—*geology*."

Assuming for the present that every remain asserted to have been found in the gravel-beds of the Valley of the Somme has really been so discovered, the question has to be asked, What is the geological age of these diluvia? If general opinion be correct, the gravel of the Somme is true diluvium, formed chiefly from the denudation of the tertiary strata by an excavating river at a time when the great extinct pachyderms were inhabitants of the earth. The upper gravels in which the remains have been found are assumed to be the oldest, being "deposited at a time when the chalk had only been scooped out to the level on which they are now found resting, and that the lowering of the bottom of the valley from that level to thirty or forty feet below the present surface of the peat—that is to say, the removal of from one hundred to one hundred and forty feet of chalk by the denuding action of water—must have taken place since the deposit of the upper gravels." Now, it is said, "he must be a bold man who would fix even a *minimum* limit to the period required to effect such a denudation by any body of water which can be reasonably conceived to have flowed down the valley." But the fact stares us in the face, that one of the most eminent geologists of France, M. Elie de Beaumont, denies the accuracy of the current opinion regarding the age and formation of the gravel of the Somme. Twenty years back this high authority expressed certain views regarding the nature of the ground of Moulin Quignon, where the famous jaw-bone was found last spring. Since the present controversy, M. de Beaumont has declared that he still holds the same views—viz., that certain gravel deposits like that of Moulon Quignon must be distinguished from the Alpine drift, or *diluvium*, properly so called, and the origin of which latter is owing to causes which have ceased to operate. The Somme gravels are owing to other causes—that is, to such as we still see in operation. These gravels have been attributed either to the action of the polar ice which may have floated on the Bay of Somme, or to various successive changes of level in the general mass of the adjacent land. Whether it be justifiable or not to ascribe so small an effect to such gigantic causes is to M. de Beaumont less than doubtful. But even if not so, the production of the latter would after all lie within the range of *actual* causes. If, also insists M. de Beaumont, the gravelbank of Moulin Quignon be the result of a later mixture of grey and red drift, it certainly does not belong to that grey which is the real Alpine drift, considered by Cuvier as well as by himself as representing the end of the period of fossil elephants, and as anterior to the presence of man.

In support of this opinion that the gravel deposit in question is owing to the most common among the actual causes—viz., storms, frost, snow, &c.—M. de Beaumont draws attention to the fact that the bank of Moulin Quignon is situate at an altitude of 30 metres above the Somme at Abbeville, and consequently at 39 metres above the level of the sea. It is overlooked at a distance of less than 2 kilometres by points the altitudes of which are respectively 61, 65, and 67 metres, at less than 3 kilometres by another point 80 metres above the level of the sea, and at less than 5 kilometres by points marking 100 metres. The gradients of the lines going from Moulin Quignon to these points all exceed the proportion of 1 to 100, or more than tenfold the *maximum* inclination of the beds of navigable rivers, and greater than those of the Arve, Isère, &c., near their sources, where their waters, even when but slightly swollen, flow with immense rapidity, and will occasionally commit the greatest ravages. Now to produce similar ravages on the undulated plains of Picardy, a single heavy snow-storm would be quite sufficient; and who would venture to guess, asks M. de Beaumont, the *maximum* effect of this kind which may have taken place in the environs of Abbeville since the age of stone? The deposit of Moulin

Quignon may therefore be very well owing to such a cause, though anterior to the turf deposits of the North of France, many of which are posterior to the Roman roads. Such deposits, which M. de Beaumont calls "moveable deposits on declivities," are particularly abundant in the North of France, owing to want of coherence of the Eocene, Miocene, and Pliocene deposits which cover the chalk formation, and are essentially contemporaneous with the alluvial beds of valleys—those along the coasts and turf-deposits. Thus, then, these Somme gravels upon which so much stress has been laid are, according to one of the highest authorities, not even alluvia deposited by the encroachments of rivers on their banks, but are composed simply of washed soil, deposited on the flanks of a valley by excessive and frequent falls of rain and of snow of the age of the "stone period," of peat-moss, and of the Swiss lacustrine habitations! The views of M. Elie de Beaumont have been confirmed, we may remark, very recently by the Abbé Chevalier, who, in a report to the French Academy upon the superficial strata of Touraine, stated, that the "moveable deposits on declivities" described by the former were there very frequently met with, and that flint hatchets had been discovered in such moveable deposits, whilst none had been met with in the real drift. We would here draw attention likewise to the statement of Sir Roderick Murchison concerning the drift of the South-east of England*—a formation regarded by some good authorities to be geographically and geologically the counterpart of that of the valley of the Somme. This writer finds evidence for believing that the "flint drift" was not the lingering deposit of long ages of comparative repose, but was the result of short turbulent agencies, performing in a few years the work thought by one school of geology to require hundreds or even thousands of centuries. At the last meeting of the British Association, we find Mr. Philips† coming to the conclusion, in his paper on the flint instruments and gravel-beds of St. Acheul, "that the changes were local, and that they afforded no evidence of the great antiquity of man." Mr. Austin also, in speaking at the same meeting of the Bedford section, "endeavoured to show that the remains of the extinct animals taken out of the gravel, which was thirty feet above the level of the sea, were derived from an older gravel, and that this view would equally apply to the accumulations of the Somme and Ouse." Now may we not with great emphasis repeat the words of the writer in the 'Edinburgh Review'—viz., that the existing fundamental opposition which has arisen between such eminent geologists as M. Elie de Beaumont and Sir Charles Lyell, Mr. Prestwich, and others, as to the age and formation of the Abbeville gravels, is sufficient evidence to show that the very grammar of this part of geological science requires, if not to be written, at least to receive an adequate sanction. Let us pass from the caves and gravel-beds to what has been disinterred from them, or asserted to have been so.

First, in respect to the statement that the chemical condition of the bones found in union with those of the extinct animals has been proved to be identical (Marcel de Serres and others), it may be replied, that after a certain period had elapsed, the chemical condition of the former would remain the same, and that no conclusions of accuracy as regards the age of the bones could be deduced from such conditions. Some years ago, a party of geologists had placed before them a jelly made with gelatine extracted from bones taken out of the Kirkdale cavern, or from a similar source;* whilst a piece of an ancient Burgundian skull, supposed to be about 2000 years old, a fragment of the skull of an ancient Roman found in a tomb on the road between Cumæ and Baiæ, and a fragment of the skull of a young aboriginal female, taken from an ancient tomb at Picul, in Yucatan, were found to consist almost wholly of earthy matter, the animal matter having very nearly disappeared.† Bones placed in a porous bed, through which water can freely pass, will have their gelatinous portions washed away, in comparatively few years, by a process somewhat like that recommended by the eminent director of the French Mint, M. D'Arcet, for the extraction of gelatine for culinary purposes.‡ Sir Charles Lyell, in commenting on the chemical condition of the bones from the Aurignac cave, remarks:

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* Journal Geol. Soc., vii. p. 349.

† Prof. Geol., Oxford.

> "No doubt, had the human skeletons been found to contain more gelatine than those of the extinct mammalia, it would have shown that they were the more modern of the two; but it is possible that after a bone has gone on losing its animal matter up to a certain point, it may then part with no more so long as it continues enveloped in the same matrix. If this be so, it follows that bones of very different degrees of antiquity, after they have lain for many thousand years in a particular soil, may all have reached long ago the maximum of decomposition attainable in such a matrix." (p. 187.)

A French chemist, M. Couerbe, has recently proposed a definite rate of loss as occurring in ossific remains. He concludes, from experiments made on skeletons dug up at the Chateau of Vertheul, that the organic nitrogenous elements disappear at the rate of three per cent. in a century. But what is the value of such theories, if the following statement be true? Lieutenant-Colonel Hamilton Smith, before referred to, tells us,§ that whilst one fossil bone from the Yealm Bridge cave, under the influence of hydrochloric acid, was "reduced to a spongy, flocculent mass, which, having become lighter than the fluid, rose to the surface in the shape of a mere pellicle," in a metatarsal bone of an hyæna from the same cavern "the animal substance remained so abundant that the bone retains its complete form, is only translucent, and remains at the bottom of the liquor as if it were a recent specimen, of which it preserves all the characters." The jaw-bone of the gravel bed of Moulin Quignon has been subjected, as may be imagined, to a very close and lengthened investigation. As this *reliqua* is a very important one, and its history extremely interesting, we shall give an account of it in the following quotation from the 'Edinburgh Review,' premising, however, that the *procès-verbaux* of this *cause*

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* Transac. Ethnolog. Soc., vol. ii. p. 127.

† For further and interesting information on this point, the reader may refer to a 'Description of a Deformed Fragmentary Human Skull found in an ancient Quarry-cave at Jerusalem, &c.,' by J. Aitken Meigs, M.D. Philadelphia, 1859.

‡ Transac. Ethnolog. Soc., op. cit.

§ Op. cit., p. 99.

célèbre may be found in the June number of the 'Natural History Review.'

"In March last, the workmen at Moulin Quignon, near Abbeville, brought to M. de Perthes a human tooth, which they declared they had found in the usual site. Having directed that special care should be taken to report to him the first appearance of further relics, on the 28th of the same month a workman named Vasseur announced that a bone projected about an inch from the matrix. This was extracted under the eyes of M. de Perthes himself, and proved to be one-half of a human jaw. A flint axe was not many inches distant. The exact depth of the jaw from the surface was 4½ metres, or 15 feet. The bed in which it lay was a sandy one, in contact with the chalk, and dark-coloured, from the admixture of iron and manganese. There were found by M. de Perthes on the same day in the yellow sand belonging to the same bed, and 3½ metres from the surface, fragments of mammoths' teeth. When the discovery was published, geologists flocked to the spot both from Paris and London, especially M. de Quatrefages, Professor of Anthropology at the Paris Museum of Natural History, from the former; Messrs. Prestwich and Evans, Drs. Carpenter and Falconer, from the latter. The verdict given on the spot seems to have been entirely favourable to the genuineness of the relic. The jaw-bone was conveyed to Paris, and one tooth and some hatchets to London. It appears that at the time no doubt was entertained by any of those who visited Moulin Quignon on the 14th and 15th of April that the jaw was authentically found in the locality described, and where it was seen by M. Boucher de Perthes. The Englishmen, however, moved partly by the subsequent opinion of skilled antiquaries that the hatchets were forged, as they presented no palpable proofs of antiquity, and partly by the fresh condition (when sawn open) of the interior of the single tooth in their possession, surrendered their first opinion.

"Dr. Falconer, in a letter to the 'Times' of April 20th, declared that M. de Perthes had been deceived by the men. He further added that the undoubted osteological peculiarities of the jaw which led the most skilful naturalists to consider it as bearing internal evidence of remote antiquity were merely accidental, though presenting an extraordinary coincidence with the alleged circumstances of its discovery. The Parisian naturalists, however, and especially M. de Quatrefages, who had possession of the jaw, firmly adhered to the first opinion. Under these circumstances, the controversy might have been hopelessly prolonged, had not the happy idea been entertained and acted on of holding a meeting of *savans* of both nations, which took place at Paris, under the able presidency of M. Milne-Edwards, from whence it was adjourned, on the 12th of May, to Abbeville. The assembly consisted of MM. Milne-Edwards, De Quatrefages, Lartet, Delesse, and Desnoyers, from Paris; and Drs. Falconer and Carpenter, Messrs. Prestwich and Busk, from London. Fresh excavations were undertaken beneath the very eyes of the commission, and were attended with the discovery of several hatchets, which were believed to be genuine, though not possessing the *patina* or other proofs of antiquity formerly relied on. These results, together with a full investigation of the circumstances attending the discovery of the jaw, terminated in the conviction of every individual present at the inquiry on that occasion, that no fraud had been practised.* . . . . (To speak rigorously, Dr. Falconer, while perfectly satisfied of the authenticity of the flint-tools, expressed in the presence of the Commission, on the 12th of May, and also of the jaw itself, declined to commit himself to the authenticity of the tools discovered near the jaw; and on the 28th of March, Mr. Evans, who did not take part in the conference either at Paris or Abbeville, and who, therefore, was not a witness to the extraction of the five

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* Edin. Rev., p. 271.

'haches' in presence of the commission, still denies the authenticity of those not possessing the criteria of patina, dendrites, or worn edges; and it is proper to add, that the strong doubts he has expressed on this subject are still entertained by many geologists of eminence. The facts stated in the text are based on documentary evidence; but we are informed that at recent meetings of the Geological Society of London more than one of the English commissioners has seen reason to retract the opinion he formed at Abbeville. These frequent alterations of judgment have thrown doubt on the whole transaction. It is certain that many genuine remains have been found at Abbeville, but it is not less certain that many spurious objects have been introduced into the beds of gravel there.*) . . . . The reader must not, however, suppose that with the admission of the relics being truly found, as alleged, in an undisturbed bed, at the depth of fifteen feet, coincidence of opinion as to the age of the fossil was thereby attained. Dr. Falconer and Mr. Busk restated the doubts they originally entertained as to the absolute age of the jaw, which was now sawn across, and displayed an amount of freshness inconsistent, in their opinion, with its being coëval with the remains of the extinct quadrupeds. These doubts do not seem to have been shared by the French members of the Commission; but the eminent physiologists who belonged to it, especially MM Milne-Edwards and De Quatrefages, expressly held themselves uncommitted to any opinion as to the geological age of the Moulin Quignon beds." (Op. cit.)

The particular reasons why Dr. Falconer and Mr. Busk have withheld their assent to what have been termed "the startling conclusions of the *procès-verbaux* of the Commission," are *quoad* the jaw-bone as follows: (*a*) the black coating due to the matrix was washed off with the greatest readiness; (*b*) there was no infiltration of metallic matter; (*c*) there was no appearance of dendrites externally or within; (*d*) the outer surface was rather smooth, and quite unlike the condition in which buried bones are usually found; (*e*) the substance of the bone was firm, and when sawn gave the peculiar odour produced when the saw cuts through fresh bone; (*f*) the dentine of the tooth was white, and the enamel quite brilliant, and in every way resembling that of a recent tooth; (*g*) the dental canal was lined with a layer of grey sand, which was not mixed with the black matrix, and seemed to indicate that the bone had been at one period in a ferruginous sandy deposit; (*h*) the mere adhesion of the particles of the black *gangue* to the bone did not indicate that the bone had remained in the deposit for any considerable time, as experiments proved that when this substance was applied in its soft state to any surface, no matter how smooth, it adhered to it when dry with the greatest tenacity. The characters of the bone and tooth, observes a writer in the 'Popular Science Review,' October, 1863—

"Are remarkable, when contrasted with those of bones of an earlier date than the beds of Moulin Quignon. The human lower jaw from a gravel heap of an Ipswich coprolite bed alluded to in the discussion, although retaining some of its gelatine, is completely infiltrated with iron, the Haversian canals being filled with red oxide, and a section of the fang proving that the dentine has been penetrated by the same metal. This demonstrates that a human jaw, if favourably placed, is equally susceptible of impregnation with metallic matter as the bone of any other mammal. It is worthy of note, that the tooth which had been so carefully examined in London, and had been forwarded from

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* Note in Edin. Rev., p. 272.

Abbeville, was not permitted a place in the evidence, it having been insinuated by the French *savans* that it might, through some error, have been confounded with some other specimen. The French men of science relied almost exclusively on circumstantial evidence, and seemed to reject the intrinsic variety which was regarded by the English as of more importance."

The third portion of Professor Huxley's work gives an admirable summary of what is known of the history and of the author's critical investigation of the Engis and Neanderthal skulls. In the former, Professor Huxley tells us (p. 156) that he cannot find any character which, if the cranium were recent, would give a trustworthy clue as to the race to which it might appertain. Its contours and measurements agree very well with those of some Australian skulls, particularly as respects the occipital flattening which is to be met with in some of the latter. But it must be admitted that all Australian skulls do not present this flattening, and the superciliary ridge of the Engis skull is quite unlike that of the typical Australians.

"On the other hand, its measurements agree equally well with those of some European skulls. And, assuredly, there is no mark of degradation about any part of its structure. It is, in fact, a fair average human skull, which might have belonged to a philosopher, or might have contained the thoughtless brains of a savage." (Huxley, p. 156.)

With respect to the "Neanderthal skull," it has been observed by Dr. Hermann von Meyer that the possession of dendritical crystallizations covering both its inner and outer surfaces, and upon which stress has been laid as indicative of its extreme antiquity, is no real criterion of its age, since such dendrites have been found upon Roman bones. Sir Charles Lyell remarks upon the fact that "the skull and bones of the Neanderthal skeleton had lost so much of their animal matter as to adhere strongly to the tongue, agreeing in this respect with the ordinary condition of fossil remains of the post-Pliocene period." (p. 78.) But a piece of the "Jerusalem skull" examined by Meigs (op. cit.), and thought to be Turanian, of historic time, adhered to the tongue likewise. We have already learnt, however, how small a dependence can be placed upon this part of the argument. When the Neanderthal skull and bones were first exhibited at a scientific meeting at Bonn, in 1857, some doubts were expressed by several naturalists as to their being truly human. Even Professor Schaffhausen, who did not share these doubts, admitted that the thickness of the bones was very extraordinary, and that the elevations and depressions from the attachment of muscles were developed in an unusual degree. Sir Charles Lyell remarks: "When on my return to England I showed the cast of the cranium to Professor Huxley, he remarked at once that it was the most ape-like skull he had ever beheld." (p. 79.) Since a more recent examination of casts and photographs from it, the anatomist just mentioned allows, with Messrs. Schaffhausen and Busk, that this skull is the most brutal of all known human skulls, resembling those of the apes, not only in the prodigious development of the superciliary prominences and the forward extension of the orbits, but still more in the depressed form of the brain-case, in the straightness of the squamosal suture, and in the complete retreat of the occiput forward and upward from the superior occipital ridges. But, writes Professor Huxley, in his chapter "On some Fossil Remains of Man":

> "In no sense can the Neanderthal bones be regarded as the remains of a human being intermediate between men and apes. At most they demonstrate the existence of a man whose skull may be said to revert somewhat towards the pithecoïd type—just as a Carrier, or a Pouter, or a Tumbler, may sometimes put on the plumage of its primitive stock, the *Columbia livia*. And, indeed, though truly the most pithecoïd of known human skulls, the Neanderthal cranium is by no means so isolated as it appears to be at first, but forms, in reality, the extreme term of a series leading gradually from it to the highest and best developed of human crania. On the one hand, it is closely approached by the flattened Australian skulls of which I have spoken, from which other Australian forms lead us gradually up to skulls having very much the type of the Engis cranium. And, on the other hand, it is even more closely affined to the skulls of certain ancient people who inhabited Denmark during the 'stone period,' and were probably either contemporaneous with or later than the makers of the 'refuse heaps,' or 'Kjokkenmöddings' of that country. . . . . Where, then, must we look for primæval man? Was the oldest *homo sapiens* Pliocene or Miocene, or yet more ancient? In still older strata do the fossilized bones of an ape more anthropoïd or a man more pithecoïd than any yet known await the researches of some unborn palæontologist?" (p. 157.)

It must not be forgotten that the peculiarity of the original position of the Neanderthal skeleton, and the want of the remains of extinct or other animals, render it necessary that any inferences regarding its antiquity must be drawn with the greatest caution.

With respect to the "fossil-man of Denise" it may be said that no trust can be placed upon it. The high prices given for "human fossils" at Le Puy, and the known fact that certain fabrications had there been made, render it quite impossible to say that perpetration of some kind of fraud did not take place in this especial instance. As far as relates to the Natchez case, the want of direct and sufficient evidence as to the *find* must be clear to every unprejudiced person. Even were it not so, the association of the human bone with the remains of a mastodon and megalonyx may be, under the particular circumstances, explained in the way Sir Charles Lyell stated in his 'Second Visit to America,' in 1846—viz., that "the former may possibly have been derived from the vegetable soil at the top of the cliff; whereas, the remains of extinct mammalia were dislodged from a lower position, and both may have fallen into the same heap or talus at the bottom of the ravine." This idea has also occurred to Colonel Wiley, of Natchez. We will now consider the "flint implements." With reference to them it may be remarked, that there are not wanting geologists and antiquaries who take very different views of their age and origin to those usually prevalent. The sceptics, no doubt, form the exception, and are almost ridiculed by some of the opposite party; still our duty is to state all possible objections. Mr. Wright, no mean antiquarian authority, has endeavoured to throw doubt on the artificial origin of the "flakes," or more simple implements, and of which the number found has been very large. He maintains that they might have been produced naturally by a violent and continuous gyratory motion (perhaps in water), in which they were liable to be struck by other bodies in the same movement. According to Mr. Wright, the mere number also of "flakes" found in the same locality renders it improbable that they can be other than natural phenomena. We ourselves have been forcibly struck with this circumstance. We hear of "basketfuls" of flint implements having been collected, and then of the assumption of the localities in which they were found having been places for the *manufacture* of such articles, and of the early hunting and fishing tribes of the times of the mastodon frequenting the same spots for hundreds or thousands of years in succession, so that

> "The number of the stone implements lost in the bed of the river need not surprise us. Ice-chisels, flint-hatchets, and spear-heads may have slipped accidentally through holes kept constantly open, and the recovery of a lost treasure once sunk in the bed of the ice-bound stream inevitably swept away with gravel on the breaking up of the ice in spring, would be hopeless. During a long winter, in a country affording abundance of flint, the manufacture of tools would be continually in progress; and if so, thousands of chips and flakes would be purposely thrown into the ice-hole, besides a great number of implements having flaws, or rejected as too unskilfully made to be worth preserving." (Lyell, p. 141.)

Of the *ingenuity* of such explanations there cannot be a doubt, but we feel unsatisfied as we think over them. Further, we would remark that Mr. Whitley has recently found beneath the surface soil at Croyde Bay, North-Devon, at the mouth of a small transverse valley, broken flints in considerable number. About ten per cent. of these had more or less of an arrow-head form, passing by insensible gradations from what appears to be perfect arrow-heads of human manufacture to such rough splinters as are clearly the result of natural causes. It has been maintained by others, that although "flint implements" are found which are unquestionably the work of human hands, those of the Valley of the Somme were fabrications, or modern imitations, introduced surreptitiously into the gravel, and afterwards brought to the scientific men as having been disinterred from there. At one time, M. Boucher de Perthes was accustomed to give from two to five francs for an implement—a price very likely, in the opinion of many, to lead to the practice of deception. But it has been said that such a price would not really pay any one to make these weapons. As the writer in the 'Edinburgh Review' remarks, perhaps not at first, but now that the English market is craving flint instruments, it might answer the purpose to manufacture them. Certain it is, that such things are fabricated; and no one reading the *procès-verbaux*, in the 'Natural History Review' can avoid perceiving that the learned in such matters cannot always distinguish the genuine from the manufactured article. The grounds upon which Messrs. Falconer and Busk found their scepticism as to the genuineness of the particular Moulin Quignon "haches," are (*a*) they possess a form different from all others previously found at Abbeville or Amiens; (*b*) their angles are sharper than those of any other specimens except those of La Porte Mercadè; (*c*) they present no staining nor discoloration; (*d*) they exhibit neither dendritic markings nor adherent matrix ; (e) the great sameness of character presented by all the specimens, as though they had all been made by one or two persons ; (f) none of the specimens were tinted with iron, although all the larger pebbles of the *couche noire* were so tinged ; (g) the matrix of the St. Gilles' specimen contained "unquestionable traces of recent vegetable structure." In reference to M. Desnoyer's theory of the cause of the striæ on extinct mammalian bones, it will be sufficient to state that M. Eugene Robert, in reply to it, maintains that all the grooves, gashes, and scratches, which the former attributes to the action of the flint weapons used by an early race of men, have been produced by the implements of the artisans employed in extracting, and afterwards preparing the fossils. M. Robert's attention was first directed to such an origin by the remark of a person engaged in the French School of Mines, to the effect that they were due to the awkwardness of those who removed the earth from the specimens. In one instance, M. Robert was enabled, from the character of the injury, to infer that the mark had been produced by a workman's pickaxe. A few words will suffice relative to the testimony of the deltas of large rivers. We have already seen that Mr. Horner and the French *savans* differ as to the rate of deposition, some calculating it to occur at twice the rate believed by others. But are either right? The experiments instituted by Mr. Horner to obtain an accurate chronometric scale are not considered by experienced Egyptologists as at all satisfactory. It has likewise been objected that the Arabs can always find whatever their employers desire to obtain ; and that the artificial objects got up by the borers might have fallen into old wells which had become gradually filled up. It may not be out of place, also, that we recall the following fact: In the course of making the excavations for the Thames Tunnel, the difficulties that arose from the nature of the soil in some parts induced the contractors to procure a diving-bell, for the purpose of examining the bottom of the river. On the first inspection, a shovel and hammer were left on the spot by the divers; but these tools were, contrary to their expectations, nowhere to be found on the next visit. In the progress of the excavations, however, while advancing the protecting wooden framework, the missing shovel and hammer were found in the way of it, having descended at least eighteen feet in the ground, and probably resting or mixed up with some ancient deposit. It now remains for us to notice another interpretation, which has been put upon these strange pages in the great "stone-book of nature," which so puzzle us. The facts, speaking generally, are admitted; the caves and gravels are held to be above suspicion; and the remains and objects said to have been extracted from them are believed to have been actually found in them. This view admits, then, that we have proof that man was contemporary in Europe with two species of elephant, two species of rhinoceros, at least one species of hippopotamus, with the cave-bear, cave-lion, cave-hyæna, and many smaller extinct animals. But it prefers to regard the geologic and palæontologic facts which necessitate this admission, as being more in favour of the prolongation of the mammalian period, or of the days of the great extinct pachyderms, down to recent times, than of the carrying back of the existence of man into the remoter ages. Our contemporary, when commenting on M. Lartet's account of the Aurignac cave, observes:

> "Assuming all the conclusions from the observations of M. Lartet to be correct (and from the great majority of them we see no cause to dissent), it appears to be almost incontestable that the result is unfavourable to the idea of assigning an almost measureless antiquity to those numerous deposits which are proved to be coëval with extinct mammalia, and of which we have treated in this article. It goes a long way to convince us that the existence in Europe of the cave-bear, cave-lion, rhinoceros, and mammoth must be approximated much more towards recent times rather than that the creation of man must be drawn back into a region of quite hypothetical remoteness on account of his association with the extinct species. But Sir Charles Lyell and M. Lartet (who appears to be a thorough disciple of his school) try to persuade us that absence of any mark of important change in the physical condition of the country about Aurignac is no proof that the antiquity of the tomb may not be indefinitely great. Great, no doubt, it must be, but every fact connected with its position and discovery seems to show that it belongs to what we may (somewhat vaguely, no doubt) call *the present age of the world.* There is nothing unreasonable in assuming that these mammals survived to a later period of the world's history than geologists have usually allowed. Even the changes of climate which they were once considered to establish has disappeared as a difficulty." ('Edinburgh Review.')

But once admitting the contemporaneousness of man with the extinct pachyderms before mentioned, his duration upon the globe must under any view of the case, be still allowed to be of a far greater antiquity than has hitherto been suspected. But we are warned that we have already exceeded our usual limits, and we must relinquish our obscure, though very interesting theme. It will be recollected then that the theory of Cuvier was, that the earth, when sufficiently free from water to support animal existence did so in the successive order of—1. An age of reptiles; 2. Of palæotheria; 3. Of the mammoth and mastodon; and 4. Of man. Down to the time of Cuvier and Buckland, no very definite idea existed that man was contemporaneous with the extinct pachydermata of the third series. In their day, however, a few explorers of the labyrinthine passages of the past ventured, as we have seen, to hint that there was testimony to show that Man did tread the earth, for however short a period, with the cave-bears, hyænas, and rhinoceroses which have been extinct for ages. Now we find many of our highest geologic and palæontologic authorities strenuously supporting such an opinion, and carrying back the origin of man, geologically speaking, into the post-Pliocene period of the upper tertiary series. Whether he is to be finally located in these strata along with the mammoth and cave-bear, or whether he is to be brought forward again into a brighter daylight in company with certain extinct species, which were once thought to have been lost before they actually were so, or whether after all, certain grand mistakes have not been made, must be regarded as yet—in our opinion, at least, quite undetermined. Not only, however, do Sir Charles Lyell and others, for whose opinion we should have the highest respect, lean towards the belief that man inhabited the earth in far distant ages, along with the elephant and the rhinoceros in the valley of the Ouse at Bedford, and in "post-Pliocene" times fashioned the flint implements of Amiens and Abbeville, but they maintain that the "recent period" of his existence here has been of so long duration as to carry it back to an incalculable antiquity, or one at least, to which five or six thousand years are but an insignificance, and in comparison with whose archives the records of the valley of the Nile must be considered as extremely modern. The evidence for thus lengthening the "præ-historic period" of man will be found displayed in a most interesting manner in Sir Charles's recent volume. It is founded upon the history of Danish peat and its remains, upon the Kjôkkenmödding of Denmark, the lacustrine habitations of ancient Switzerland, the "crannoges" of Ireland, &c. &c. To discuss it would require nearly as many pages as we have already consumed. In conclusion, we feel bound to say, then, that however *suspicious* we still feel in respect to the nature of the testimony which has been offered us in proof that our race existed in past ages, almost bewildering to think of, we consider that neither the scientific world nor the general reading public have received for some time past two more welcome gifts than those of Sir Charles Lyell and of Professor Huxley. We do not know which to rate more highly—their scientific importance, or their general interest.

REVIEW X.

1. *Report of the Commissioners appointed to Inquire into the Sanitary State of the Army in India, with Précis of Evidence.* 8vo, pp. 265.
2. *Minutes of Evidence, Reports from Stations in India and its Dependencies, &c. &c.* 2 vols. folio, pp. 943 and 959.

THE appearance of this Report, with its documentary evidence, will form an era in the literature of State Medicine, and of the Hygiene, not of armies and military establishments only, but of communities and peoples also. From the very wide extent and varied nature of the field of inquiry, as well as from the comprehensive and searching system of investigation pursued, and the highly-intelligent character of the testimony received, whether orally or in writing, an amount of valuable information on many of the most important subjects relating to public health is collected together, such as has never before been obtained. The Commission was appointed in the summer of 1859, and the Report is dated May 19th, 1863, so that their labours extended over four years. The period had, alas! not passed away without leaving more than one blank in the roll of the original members. The brave and earnest-hearted Alexander, who, by the sheer vigour of his straightforward, resolute character, had swiftly raised himself to the highest post of professional ambition, was early cut off, to the infinite regret of his brother officers and numerous friends. Who that knew him, as he stood at the door of his Malta hut, "in the front," by the side of the Woronzoff road, or ever accompanied him over the hospitals of the Light Division, of which he was so proud, can forget his manly bearing, his bold independence, whenever he felt himself in the right—his detestation of everything savouring of unfairness or deceit—his most friendly intercourse with all the medical officers serving under him, and his hearty appreciation of their services—can refrain from dropping a tear to the memory of a gallant, able, and upright man? And then, too, the pure and noble-minded Sidney Herbert, worn out by slow sickness, and forced, despite heroic endurance, to yield his post to another, sunk at length, "weary with the march of life," to his long rest, amid a nation's sorrow. The date of his latest appearance at the meetings of the Commission appears to have been within a month or two of his decease. His place was filled by Lord Stanley, than whom no fitter or abler man could be found; and that of Mr. Alexander by Dr. Gibson, the present Director-General of the Army Medical Department. The other medical members of the Commission are Sir Ranald Martin, Dr. Farr, and Dr. Sutherland. The guiding, and indeed the originating spirit of the whole, has, of course, been the first of these three eminent men; and well may we, while according high praise to all, specially congratulate him on the success of the great object of his professional life. Rarely has it fallen to the lot of a medical man to witness so completely the attainment of the long-cherished and the long-pursued object of a most worthy ambition, and by the attainment of it to confer a great benefit on his country.

The paper drawn up in the early part of 1835 by him, while Presidency Surgeon at Calcutta, and addressed to Sir Charles Metcalfe, the Acting Governor-General of India at the time, contains the germ of the large and comprehensive inquiries which have been carried out by a Royal Commission five-and-twenty years subsequently. Basing his suggestions on the valuable work of Hennen on the 'Medical Topography of the Mediterranean,' he proposed that "the medical boards at each Presidency be directed to require from all medical officers, civil and military, serving under their orders, a report of the medical topography of the province, district, city, or cantonment, with the localities of which they may happen to be best acquainted, in which they may be serving at the time." These reports to be collected into a memoir, to be printed and supplied to all staff-surgeons and officers of the Quartermaster-General's Department. Besides minute topographical, meteorological, and climatic details, with remarks illustrative of the vital and medical statistics of the native populations, their dwellings, mode of life, &c., the most prevalent epidemic, epizootic, and epiphytic diseases, minute information is required as to the situation, construction, arrangement, and existing condition of barracks and military hospitals, with special reference to the drainage of the ground, the size and airiness of the wards, the quality of the water-supply, &c. &c., adding the very significant queries, "Whether any particular disease has ever been traced to them?" and "Whether any patients have ever laboured under any diseases that could fairly be attributed to the locality of the hospital?" The Medical Board of Calcutta, to whom Mr. Martin's paper was referred by the Indian Government, cordially approved of the scheme proposed, and expressed their readiness to co-operate in the furtherance of its important objects. The result was, that the board was instructed by council to direct the attention of all the medical officers throughout the Presidency to the subject; and "in calling on those under your orders for topographical memoirs, it should be impressed on their minds that it is not on mere geographical position that climate and its salubrity depend, and that a fair estimate of the influence of detached spots on the health of the troops or other inhabitants is what is most wanted."

In 1842 he addressed to the Colonial Office a memorandum in which he pointed out the enormous loss of life, injury to health, and sacrifice of money resulting from the faulty mode of locating and constructing barracks and hospitals for the European troops in India and in many of Her Majesty's colonies, in consequence of these matters having usually been "left to the fancy of individuals, some of them very able military engineers, but too often persons who have had neither the time nor opportunity to consider the subject in all the importance it derives from the considerations of health." He proposed that standard or model plans be prepared for barrack and hospital accommodation, proportioned in scale to certain given numbers of men, and suited to the various climates occupied by our troops, and that these should in future be adopted, in place of leaving the matter to the caprice or fancy of different individuals. For this purpose, he suggested that a committee of competent engineers and medical officers be convened in London, to obtain the necessary evidence and report to the Government thereon. With the customary acknowledgment and thanks to the writer the matter dropped for the time; and it was not till the disasters of the Crimean war forced the subject of military hygiene on public attention—notwithstanding the oft-repeated expostulations of medical officers, and, although many terrible warnings, both in the East and West (as, for example, at Hong Kong, Kurrachee, Barbadoes, and Bermuda), had been given not many years before of the danger of neglect—that there seemed to be a prospect of any useful result from his labours. With nations, it is often as it is with individuals—they need to be afflicted to learn the statutes of true wisdom and humanity.

The admirable 'Suggestions for promoting the Health and Efficiency of the British Troops serving in the East Indies,' addressed by Mr. Martin, in 1857, to the chairman of the East India Directors, enter more fully into the details of the important subjects of the selection of the proper locality and of the best structural arrangements in barracks and hospitals, thus anticipating the chief recommendations on these particulars in the present Report. The question of suitable hill stations for the troops is largely discussed, and the necessity for a series of careful scientific examinations being made of the ranges of hills throughout India is pointed out, to determine the best spots, which shall be free alike from the malarious fevers of the plains and from the disorders of the bowels which have infested too many of the mountain positions which had hitherto been selected. Lastly, the great importance of having a competent medical officer of health for the sanitary duties of the army at each of the Indian Presidencies, and whose office it should be in peace to examine and report on the sites and condition of all military stations, the plans and structure of all buildings, and on everything relating to the health and comfort of the soldier, while in war; being attached to the quartermaster-general in the field, he would study the medical topography of the scene of action, advise as to the best sites for camps, &c., and as to all other matters having reference to the sanitary condition of the army. It is thus seen that the scheme sketched out in 1857 constitutes a sort of programme of the inquiry which has been conducted by the Commission. The voluminous evidence which accompanies the Report occupies no less than 1900 folio pages of small print, and consists of the oral testimony of forty-eight witnesses, chiefly medical and military officers who have served in India, of the detailed replies to the elaborate queries prepared by the Commission from upwards of a hundred stations in the three Presidencies and in Ceylon, of a lengthened series of statistical returns of the royal troops from 1847 to 1856 prepared by Dr. Balfour, of the War Office, of numerous statistical tables illustrative of the health of the late East India Company's army prepared by Dr. Farr, together with a Report by Mr. Glaisher upon the meteorology of India, extending over nearly 160 pages. To diminish the labour of perusing such a huge mass of evidence, a very useful *précis* of the oral evidence, arranged as an analytical index under the different heads of inquiry, has been drawn up by Mr. Baker, the active Secretary of the Commission, and is appended to the Report; while the abstract of all the stational reports prepared by Dr. Sutherland, along with a valuable paper of comments on these reports, contributed by Miss Nightingale at the request of the Commissioners, will enable the reader to form a good idea of their chief sanitary contents.

The numerous subjects discussed in the Report are arranged in distinct divisions. It begins with the statistics of mortality and disease among European troops in India, and is followed by a similar estimate of the mortality among native troops, among the native population, and among Europeans and mixed races in the country. The various influences affecting the health of the soldier are then minutely examined under the different heads of the topography and climate of India—the sanitary condition of stations, native lines, towns, and bazaars—the site and construction of barracks and hospitals, their defects and requirements as to drainage, water-supply, &c.—the diet, dress, duties, and recreations of the soldier, together with the subject of intemperance, and of the prevalence of syphilitic disease among the European troops, &c. &c. The important question of hill stations attracts much attention. With an account of the present state of sanitary administration in India the Report closes, and, after a recapitulation of the leading results of the inquiry, the recommendations of the Commission on the numerous topics which they have examined are set forth at length. It is no easy matter, within the bounds of an ordinary article, to give the reader anything like an idea of the value of the information accumulated in such a wide and varied investigation. Its interest and importance are national, not merely professional, and no rapid or superficial sketch of its more prominent features can do it justice. The difficulty is to compress into a short space what we have to say on its manifold and varied contents. We begin with the statistical data.

On April 1st, 1861, the entire British army consisted of 227,005 officers and men. Of this aggregate, 82,156 belonged to regiments in India at that date, and 144,849 to regiments at home, or in the colonies. The strength of the regiments serving in India in the previous year was 94,829. Thus a large proportion of our military force is stationed in India, and a still larger number may be called to that part of the empire in great emergencies. In April, 1862, some of the changes resulting from the incorporation of the Indian forces in the royal army had been carried out; and, at that date, the European officers in India amounted to 3962,* the non-commissioned officers and men to 70,489, the native officers and men to 108,382, exclusive of 11,652 in the Punjab local force, and of numerous police corps which have been created in all the provinces. The general distribution of this large force was as follows:—In the Bengal Presidency, including the North-West Provinces and the Punjab, 85,966 troops were stationed, consisting of 46,299 Europeans (officers 2160), and 39,676 natives. With few exceptions, the European forces of this command are stationed in the plain of the Ganges, and near the chief cities on the rivers. The force in the Bombay Presidency amounted to 41,879, and consisted of 12,198 Europeans and 29,681 natives. They were commanded by 852 officers. The Madras Presidency had under its command a force of 54,136, consisting of 14,161 Europeans, besides 950 officers, and of 39,025 native officers and men. This force includes the troops in the British province of Pegu, in addition to Arracan and Tenasserim in Burmah; also those in Prince of Wales' Island, and in Singapore, at the extremity of the Malacca peninsula.

And now, what has been the rate of mortality in this large Indian army of ours? This is the first important question we have to consider:

> "The deaths," the Report states, "in the 57 years 1800–56 among all the Company's non-commissioned officers and men, including invalids in India, amounted to 40,420, out of an aggregate of 588,820 years of life, obtained by adding up the average annual strength in those years; so the annual rate of mortality has been 69 in 1000 during the present century.
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> "The mortality-rate was as high as 134 in 1804, in the first Mahratta war, and it was as low as 41 in 1852. It was high again in the years of mutiny, and it has been subsequently lower than the Indian standard. From the rate of 55 in 1770–99, the rate rose to 85 in the 30 years 1800–29; and the mortality fell to 58 in the 27 years 1830–56; so that the death-rate of the British soldier since the first occupation of the country down to the present day has oscillated round 69 per 1000.
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> "If the mortality is set down at 69 in 1000, it follows that, besides deaths by natural causes, 61, or, taking the English standard, 60 head per 1000 of our troops perish in India annually. It is at that expense that we have held

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* The returns for Bengal include the non-commissioned officers and men only. There is a great want of uniformity in the returns of all the Presidencies.

dominion there for a century; a company out of every regiment has been sacrificed every 20 months. These companies fade away in the prime of life; leave few children; and have to be replaced, at great cost, by successive shiploads of recruits." (pp. 18, 19.)

Throughout the Report, the death-rate of 69 per 1000 is assumed as the rate hitherto of the mortality of our European forces for the whole of India. Exception has been taken to this estimate, as a correct representation of what the health-condition of the Indian army has been now for many years past. The Secretary for India, Sir Charles Wood, in moving the budget for India in the House of Commons, on the 23rd of last July, objected to it on the ground that it was based on facts spread over an unnecessarily long period of time, during the first half of which there had been many most destructive wars, and when, moreover, far less attention was paid to the condition of the men than during the second half. We give below an extract from Sir Charles's speech for reference.*

At the recent meeting of the British Association of Science, at Newcastle, Dr. James Bird also appears to have taken a similar view, and to have maintained that the estimate adopted by the Commission was considerably too high for fair comparison with the present or recent death-rates of our troops elsewhere, or with the civil population at home. The question is too important to be passed over without examination. The following table, from the appendix to the Report, will explain the cause of the dispute, and enable the reader to form his own opinion. It gives the death-rates in groups of years, from 1800 to 1856, in each of the Presidencies, and for the whole of India:

Years.	Deaths annually to 100 of strength.			
	India.	Bengal.	Bombay.	Madras.
1800–1810	7·37	9·15	8·45	5·48
1810–1820	8·48	6·87	9·96	9·70
1820–1830	9·07	8·45	9·79	9·52
1830–1840	5·57	6·01	4·63	5·55
1840–1850	6·54	7·95	6·83	4·35
1850–1856	5·07	6·78	3·11	4·43
1800–1856	6·86	7·41	6·60	6·35
1800–1830	8·46	8·06	9·54	8·43
1830–1856	5·77	6·96	5·04	4·73

From this table we see that, during the first thirty years of the century, the death-rate for all India was 85 per 1000; that it was

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* "I think that a careful examination of the documents contained in the Report would have shown that the general conclusion as to the mortality, though perfectly correct, was based on facts going so far back, that they do not afford a very good index of the sanitary state of the Indian army at the present moment. There is not the least doubt that the habits of the soldiers were more intemperate in former years than now, and that the rate of mortality which prevailed some forty years ago is no good criterion of the rate of mortality at present. If we look to the appendix of the Report, we find that the conclusion arrived at is founded on accounts embracing four or five years of extraordinary mortality, which serve to swell up the average. For instance, in the Burmese war, during 1824 and the two following years, the mortality was 129, 157, and 158 per 1000. In the war of the Caubul massacre, the mortality was 107, and in the first year of the Sikh war it was 124 per 1000. These few years swell the average in a wonderful way. These were periods of war; but if we take

58 during the next twenty-seven years; and that it was lower still, about 51, during the last seven years alone of the period. Colonel Sykes, some years ago, estimated the death-rate in the late Company's European troops for all India, during the twenty years, 1825–1844, at between 54 and 55 per 1000; highest in Bengal, where it was 74; while in the Bombay Presidency it was 51; and in the Madras, 39 per 1000. Dr. M'Pherson states that, during the twelve years, 1827–1838, the death-rate among the European troops in the Madras Presidency averaged between 48 and 49 per 1000; but that during the fifteen years, from 1842 to 1857, it was only 33 per 1000, indicating thus a marked improvement in the health-state of that portion of the Indian army.

As to the rate of mortality from all causes in the royal army serving in India, we learn from Sir A. Tulloch, the average of the thirty-nine years, from 1817 to 1855, was, at the least, 70 per 1000 for all India, as shown in the following summary of the tables he has prepared for the troops in each presidency:

	Strength.	Deaths.	Ratio per 1000.
Bengal	377,980	29,970	79·2
Madras	249,012	15,462	62·9
Bombay	165,947	10,152	61·1
	792,939	55,584	70·0

Allowing for the deaths from the casualties of war, Sir A. Tulloch estimates the losses from disease alone at 60 per 1000 annually, over the entire period of the thirty-nine years previous to 1855. During the latter years of this period, the amount of fatal sickness among the Queen's troops had begun very notably to decline, owing, doubtless, to their improved sanitary condition as compared with what it had been. In the six years, 1850–1856, the death-rate in the Bengal Presidency appears to have been between 52 and 53 per 1000, in the Madras Presidency about 36, and in that of Bombay not more than 25 or 26 per 1000 of the strength; the average for the three Presidencies together not exceeding 38 per 1000.

Many important amendments, it is right to remember, had been carried out by Lord Ellenborough, when Governor-General, for promoting the health and general welfare of the troops. Wherever he journeyed he inspected the barracks and hospitals, and made many and salutary changes in them. He issued directions for the landing of fresh troops at the proper seasons of the year, and for sending them up to their destinations without unnecessary exposure to injurious influences—a subject which previously had been much overlooked, notwithstanding

periods of peace, the decrease of mortality is remarkable as we approach the present time. Before the Burmese war, the mortality was 75 per 1000, and in the next period of peace after, it was respectively 53, 50, 42, and 32 per 1000. . . . . The mortality, therefore, as stated in the Report of the Commission, is not a fair representation of the ordinary mortality, sufficient allowance not being made for the casualties of the service and for the deaths of persons from wounds and from diseases contracted in service. The ordinary mortality in India in times of peace is nothing like that which has been stated in the Report; and I think it right to make this statement, because some alarm might otherwise arise in the public mind."—(From the 'Times' of 24th July.)

its great hygienic importance. The character, too, of the troop-ships was, about the same time, much improved, so that the health of the men was generally better on their arrival in India than it used to be. During the administration of Lord Dalhousie, too, signal improvements, especially in the condition of the barracks in various parts, were effected; and among other salutary changes, not the least was the operation of the Limited Service Act, whereby the soldier in the Royal army was no longer, upon enlistment, tied up for life, but could claim his discharge if he wished it after ten years' service. Intemperance among the troops had decidedly diminished—a point to which the late Lord Herbert, in bringing forward the Army Estimates in 1853 specially alluded, and on which occasion he mentioned, in the House of Commons, that the mortality during the previous year among the Queen's troops in the three Presidencies had been at the rate of 42 per 1000, whereas, for the thirty-five years previously, it had been as high as 74; and that at Hong Kong it had fallen from 150 per 1000 to 69.*

During the ten years, 1847–1856, the mortality among the non-commissioned officers and men in H.M. infantry regiments serving in India was, according to Dr. Balfour's tables (which afford, he admits, only a near approximation to the desired stational information), not more than 38 per 1000 of strength in the whole peninsula; being nearly 55 in Bengal, 29·4 in Bombay, and 28 in Madras. In the cavalry regiments, at the same time, it was at the rate of nearly 31 in Bengal, 19·2 in Bombay, and nearly 18 per 1000 in Madras; or of about 23 for all India.† But as the deaths of invalids on their passage to England, which averaged 48 annually, are not included,‡ a slight addition must be made to the above ratios; so that they may probably stand thus: Bengal 56 per 1000, Bombay 33 per 1000, and Madras 30 per 1000; or of nearly 40 per 1000 for the whole peninsula.

Sir A. Tulloch states that, since the mutiny, the mortality, exclusive of casualties in the field, has been little more than half the average of the 39 years from 1817 to 1855. Of 20 regiments which went out to India in 1857-8 on account of the mutiny, and were not in action, the death-rate during the next three years averaged 34 per 1000; and in

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* From a return, ordered by the House of Commons in 1850, it appears that the death-rate *from disease alone* in H.M. troops serving in the East Indies and China during the nine years from 1840 to 1848 inclusive, averaged 68 per 1000 of the strength; exclusive, doubtless, of the deaths of invalids on the homeward voyage. The ratio of mortality varied from 98 in 1842 (the first Chinese war year) to 39 in 1848.

† We had prepared a table to compare the death-rates among the cavalry and infantry at the same stations in the Bengal Presidency; but the periods of service of the two corps at these stations differed generally so much, that no fair conclusions, we soon found, could be drawn from the comparison. At the only two stations where the period of observation for both cavalry and infantry extended to ten years, 1847–56—viz., at Umballa and Meerut—the mortality among the former was at the rate of 28 and of 20 per 1000 respectively, while among the infantry it was at the rate of 55 and 45 per 1000.

‡ The total number of invalids of H.M. troops embarked from India during the ten years was 8491, and of deaths on the passage 477, giving an average per-centage of 5·65 to the number embarked.

8 regiments which were in India at the time of the mutiny, but were not engaged, the proportion was a trifle less. In 1860 and in 1861, the latest date of any published returns, the mortality of the European troops over all India, including the deaths of invalids on their passage to this country or at home, was at the rate of 35·2 and of 36·7 per 1000, according to the statements in the annual reports of the army medical department.

A table has been prepared by Dr. Farr to show the influence of the term of residence in India on the death-rates of European troops serving there. It is based on the returns for the ten years 1846-49 and 1851-56, in the Bengal and Madras Presidencies, and for the ten years 1846-49 and 1851-56 for the Presidency of Bombay, during which decenniad the average death-rate for Bengal stood at 67·2 per 1000, for Madras at 39·5, and for Bombay at 38·4, or for all India at 61·2 per 1000.

> "The mortality (65·2) during the first year of residence is higher than it is subsequently. It decreases gradually, and becomes sensibly less in the fifth year, 44·1; it then rises slowly, and is 47·0 at the second quinquenniad (5-10); 52·8 at the third. The reduced number, 43·0, at the fourth period of residence (15-20 years) arises probably from the elimination of the sickly by invaliding, which then becomes active (6 per 100). This, however, although it takes away a fourth of the numbers annually, does not prevent the mortality, partly from advancing age, rising to 62·8 per 1000 in the last periods of service." (p. 21.)

The general inference which Dr. Farr draws from this examination, and from a comparison of the mortality in the several corps and Presidencies, is that "the fatal causes in operation produce nearly the same fatal results in India at all the ages from 20 to 55, among men exposed to the same extent to their influences." The great abridgment in the average duration of life among European soldiers is shown by other tables, from which it appears that the mean after-lifetime, or, as it is sometimes called, the expectation of life at the age of 20, is only 17·7 years in India, while in England it is 39·5 years. At the age of 40, it is 15·5 years in India, and 29·5 in England. An annuity at the age of 20 is worth not more than 12 years' purchase on a soldier's life in India, or less by 10 years than an annuity on an average life of that age at home. At the age of 40, the difference is 6 years against the former.

The rate of mortality among European officers in India has always been very much lower than among the men; and this was to be expected, considering not only the vastly better accommodation of their quarters and other sanitary advantages, but also their salutary privilege, after ten years' service, of leave of absence for three years (which counted as a part of the twenty-five years entitling them to retire on the full pay of their respective ranks), and a further absence in case of sickness. Among the Company's officers during the 20 years, 1814-33, the death-rate was 38 per 1000. In the Royal army the death-rate was 34 per 1000. "The annual loss of the 5298 officers of both armies was 197. Now of men of their ages in England about 53 die annually; consequently 144 officers died every year in India in excess of the natural deaths." Of the 3936 officers who died in the 20 years, only 122 were killed in the field or died of wounds.

Among the civil officers of the Company, the death-rate for the first

20 years of service in India has averaged not more than 2 per cent., affording thus a marked contrast to that in the military service.

So much for the life and death-rates of Europeans in India. Let us now see what has been the ratio of mortality among the native troops serving with them. Colonel Sykes estimated it at 18 in the 1000 for all India—in the Bombay Presidency at 13, in the Bengal at 18, and in the Madras at 21. Dr. McPherson gives the rate in the Madras Presidency from 1842 to 1852 at 18 per 1000. There, as well as in the other Presidencies, the mortality has varied much at different stations and in different years. While it has not exceeded 8 per 1000 at one place, it has been not only 18 but 28 at another. During the three years 1842–44, the native troops serving in Scinde died at the rate of 43 per 1000. The Commissioners have taken 20 per 1000 of the strength as a fair average mortality for the native troops throughout the peninsula—the same rate, it will be observed, as among the civil officers of the late Company. Dr. Mouat, however, points out a source of fallacy as respects the alleged death-rate among the native troops which must be kept in mind, especially if we seek to compare it with the mortality among European troops. When asked if he considered the ratio of 16 per 1000 of strength (taken by Sir A. Tulloch and other statisticians as the average loss by death in the native Indian army) to be a correct estimate, he replied that he did not, and for this reason—"The chief fallacy is, that a man who has not served a sufficient length of time for a pension, if very sick, and if the sickness be of such a kind as to make it practicable to grant him leave, applies for leave to go home, and he goes home, where he dies from the effects of disease which he had contracted with his regiment; such a case is not brought into the mortality register. . . . They were ultimately struck off the rolls of the regiment, from overstaying their leave." And when the doctor is asked what allowance he would make, for the number of deaths which take place away from their regiments, he says that it would have the result of more than doubling the estimate of 16 per 1000. The diseases, from which the Sepoys chiefly die when they go home, are the sequelæ of fever, and a sort of atrophy or general wasting of the body. This point, raised by the remark of Dr. Mouat, evidently requires to be worked out more fully, before we can accept as a correct estimate of the mortality among the native troops the rates hitherto given. Before dismissing the subject of the Indian military death-rates, we may glance at the past condition, in this respect, of our troops serving in other tropical countries; such comparisons are always useful.

During the 20 years from 1817 to 1836 the yearly mortality among the white troops in the West Indies, exclusive of the deaths among invalids on the voyage home or after arrival, averaged nearly 120 per 1000 of the strength. The rate varied much in different islands. In Barbadoes, it was not quite 60 per 1000; in Jamaica, it was upwards of 140 per 1000. In bad yellow-fever years the losses were enormous, above anything known in the East Indies—a third, a half, nay, two-thirds of the force, in some stations, were swept away in the course of the twelve months. A fourth part of a body of recruits has been swept off within five weeks of their landing! While the mortality among the white troops was at the above rates, the mortality among the black troops usually averaged between 30 and 40 per 1000.

By a Parliamentary return in 1850, the mortality among the European soldiers for the 9 years from 1840 to the end of 1848 was found to have become greatly reduced; it averaged between 50 and 60 per 1000, ranging from nearly 10 per cent. in 1841 to scarcely 3 per cent. in 1847. This was a great reduction; still there was an exceedingly high death-rate. Some important changes had been made, during the interval between the two returns, in the hygienic condition of the soldier. The number of salt-ration days had been diminished from five to two in the week, and, besides other alterations, the men were no longer huddled together, one tier above another, in ill-ventilated barracks. Since 1848 the death-rate among our white troops in the West Indies has decreased very much, having been, it would seem, under 30 per 1000 on the whole. The average of the last three or four years has not exceeded 20 per cent., and among the troops located on high ground, as at Newcastle in Jamaica, it has been much below that mark. The mortality among the black troops in the West Indies has also very sensibly declined since 1848. We thus perceive the successive diminution in the waste of soldier life, just in proportion to the diminished neglect of those obvious conditions on which health depends.

Invaliding.—To estimate aright the extent of permanent losses to the public service, whether in the army or navy, regard must ever be had to the amount of men annually discharged in consequence of damaged constitutions or irremediable infirmity, as well as to the mere number of deaths in and out of hospital. Unless this be attended to, we shall be continually understating the death-rates among our soldiers and sailors, when comparing these rates with the mortality in civil life. Colonel Sykes states the ratio of invaliding among the Company's European troops over all India at about 29 per 1000; being nearly 22 in the Madras, nearly 32 in the Bombay, and 36 in the Bengal Presidency. Among the Sepoys, the ratio is given at 19 per 1000 for all India; highest in the Bombay, and lowest in the Bengal Presidency. In the royal army, the proportion of men invalided among our European troops in India has been less than in many other parts of the world, averaging 18 or 19 per 1000 of the strength annually. The nature of the diseases and infirmities which occasion the invaliding will be seen from the following return, prepared by Dr. Balfour, showing the diseases of invalids of H.M.'s troops found unfit for service, and discharged at Chatham from 1847 to 1856. The total number discharged was 4522, and from the following specified causes:

Infirmities of age and worn out	924	Paralysis	123
Rheumatism and chronic pains	716	Venereal diseases	96
Pulmonic and thoracic disease	560	Dropsy and visceral diseases	82
Wounds and fractures	572	Hernia	73
Dysentery and hepatic disease	394	Contractions	69
Eye diseases	332	Epilepsy	61
Mental diseases	187	Stricture and urinary disease	30
Cachexia and scrofula	176	Dislocations	10
Ulcers and varix	135		

The sickness rate, or the annual proportion of attacks of sickness in an army to its strength, affords another test of its health condition. Colonel Sykes estimated this at nearly 1600 per 1000 in the whole European troops of the Company, and at somewhat under 700 per 1000 among the native troops, from year to year. The tables of Dr. M'Pherson for the Madras Presidency give a considerably higher ratio for the native troops, and they moreover indicate an increase, of recent years, in the sickness rate among the army generally in that presidency, compared with what it was prior to 1857. Has this increase arisen from a greater amount of positive sickness among the troops, or only from greater vigilance in the detection and early treatment of disease? In the royal army, the sickness rate among the infantry regiments in India from 1847 to 1856 averaged about 190 per cent. of the strength, or nearly two attacks of sickness to every man in the course of the year. It was always highest in the Bengal presidency, where it averaged 208 per cent.; while in Bombay it was 176, and in Madras 151 per cent. Among the cavalry, the amount of sickness has very generally been considerably less than among the infantry; during the above ten years, it was about 160 per cent. of the force for all India. It is scarcely necessary to add, that the sickness rate, like the death-rate, has varied extremely in different stations and in different years, ranging from 99 per cent. at one station, to upwards of 300 per cent. at another station in the course of the same year. Among our troops serving in the West Indies, the sickness rate has of recent years greatly declined—by a third, and sometimes by nearly a half—from what it used to be. Among the black troops it has been, and it continues to be, much the same as among the Sepoys. As to the number of men in the European regiments constantly off duty from sickness, it appears, from the returns of H.M.'s troops during the ten years, 1847–56, that in the Bengal Presidency this averaged nearly 85, in Bombay 53, and in Madras 65 per 1000 of the strength. With such a large number of men constantly in hospital, the effective strength of our military force is at all times seriously impaired. At the Bengal rate of sickness, an army of 70,000 British in India has, so to speak, a vast hospital of 5880 beds constantly full. There is little evidence on this head in respect of the native troops. If we may judge from the return for the year from the 1st of April, 1852, to the 31st of March, 1853, it is probably little more than half what it usually is among the Europeans. During that year, it was 46 per 1000 in the Bengal Presidency.

Having now discussed the death and sickness rates of our Indian army, let us ascertain what are the principal maladies which occasion so great an amount of permanent and temporary losses to the service from year to year:

> "The great endemic diseases of India, those which injure the health or destroy the life of the British soldier, are fevers, dysenteries, diseases of the liver, and epidemic cholera, which has for many years engrafted itself on the endemics of the country. Compared with these, all other diseases are of minor extent and importance.

"The four diseases here mentioned are almost exclusively diseases of the sultry plains of India, and when any of them are found in the elevated lands they are generally modified in degree and reduced in proportionate frequency, certain forms of bowel complaints, when the sufferers are removed from the plains to the high grounds, being the only exceptions." (p. 24.)

The excess of mortality among the native troops also is due chiefly to cholera, dysentery, and fever, the same diseases therefore that are most fatal to Europeans; and the disparity in the deaths by these diseases is not considerable.

Fevers, and especially remittent fevers, constitute the most frequent source of sickness among the white troops. Throughout India, 50 per cent. of all the admissions into hospital are from some form or another of fever, so that of all the causes which reduce the strength of our armies there, this is the principal. Besides the large mortality directly from this source, a large amount of the permanent disablement and consequent invaliding is always caused by the sequelæ of paroxysmal fevers. Continued fevers occasion, too, a large amount of sickness and death. The different forms of this type have hitherto not been very accurately distinguished and recorded. There seems good reason to believe that typhoïd or enteric fever is much more frequent in India, and in other tropical countries too, than has been generally supposed. A good many references to it are made in the stational returns. Inspector-General Dr. M'Pherson, in describing the station of St. Thomas's Mount near Madras, remarks, that "fever assuming typhoïd symptoms, with much gastric derangement, prevailed throughout the men from July to December, 1857; and it is worthy of observation, that *fevers of this character are on the increase at some of our large military stations.*" The difficulty of correct diagnosis is increased by the circumstance of typhoïd fever not unfrequently assuming a more or less distinctly-marked remittent type. It is probable that much of the fever which prevails among the natives, especially in the large towns, abounding as these do with all the elements of "pythogenic" mischief, will be found to be of a typhoïd character.

Alvine Fluxes.—Next to fevers in frequency, but still more fatal, comes dysentery. In its origin it is often intimately associated with malarial fevers of all types, so that wherever they prevail dysenteric disorders are never far off. Dysentery in India is most prevalent in the plains, and during the hot and rainy seasons. It is particularly common and destructive to the health of the troops in Lower Bengal, more so than in almost any other part of India. The mouths and deltas of great rivers in hot climates are the especial *habitat* of bad alvine fluxes; hence they are always the principal cause of the excessive mortality in our ships of war, &c., at Canton, and other like estuaries on the coast of China, Africa, &c. In several of the stations in the Bombay and Madras Presidencies, it is a comparatively mild disease. Sir R. Martin states that 11 cases of dysentery occur among the European soldiers for one case among the native troops. This is a most significant, and ought to be a most instructive, fact.

Malignant Cholera.—Since 1817, this formidable pestilence, although known previously to prevail as an occasional epidemic in different parts of the peninsula, has engrafted itself on the endemics of the soil, and has become of annual occurrence at many of our large stations. It commences generally at the beginning of the hot season, and always shows a marked predilection for low, damp, crowded, and ill-ventilated situations, especially when the water-supply is also impure. Long fatiguing marches have often, when due hygienic precautions have not been adopted, been followed by fatal outbursts.

Diseases of the Liver.—Whether as original or secondary affections, these constitute a very frequent and important class of diseases in the East Indies—more so there, it would seem, than in most other tropical countries. The numerical hospital returns do not yield, says Sir R. Martin, anything like an approximation to a true estimate of the existing amount of hepatic diseases. From 1833 to 1854, out of an aggregate force of 331,775 men serving in the Bengal Presidency, there were 18,765 admissions and 1345 deaths from diseases of the liver. On ætiological grounds, it will be important to ascertain their relative frequency and fatality, not only in the different Presidencies, but in different divisions of each Presidency. Acute hepatitis is stated to be "essentially a disease of the hot plains." The greater prevalence of liver disease in the East than in the West Indies has never been satisfactorily accounted for. Off-hand explanations of this and other medical-problems of the sort cannot be accepted.

To these three groups of disease—viz., fevers, dysentery, &c., with cholera, and hepatitis—more than four-fifths of the deaths among the Europeans in India are due. In Bengal, alvine fluxes alone, including cholera, occasion nearly one-half of the whole mortality. Dr. Farr has, with great labour, prepared a table with the view of comparing, as far as the imperfect data at his command will allow, the calculated death-rates per annum to 1,000,000 strength, among the non-commissioned officers and men of all the European forces in the Presidency of Bengal during the sixteen years 1830–1845, and among 1,000,000 males living of the age 15–45 in England, during the seven years from 1848 to 1854 inclusive. This table, notwithstanding its acknowledged imperfections, is altogether so suggestive in many points of view, that we must find space for at least an abstract of it:*

Diseases.	Bengal.		England.
All causes	66,940	...	10,082
Zymotic diseases	57,853	...	2320
Fevers, remittent	8145	...	8
" intermittent	2929	...	7
" continued, typhus, typhoïd, and ephemeral	6255	...	904
Spleen disease, &c.	229	...	3
Diarrhœa	4554	...	86
Dysentery	15,453	...	51
Hepatitis and liver disease	4559	...	193

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* In calculating the mortality, a correction has been made for the causes not specified, by distributing them over the various specified causes of death. The facts are from the medical returns, which do not include deaths in action, or other deaths which fail to come under the notice of the medical officers.

Diseases.	Bengal.	England.
Cholera	13,191	546
Small-pox	280	112
Catarrh, influenza, and bronchitis	362	228
Pneumonia and pleurisy	1065	382
Phthisis and lung-disease	2115	3874
Asthma	117	66
Apoplexy	2905	170
Other cerebro-spinal diseases	446	588
Alcoholism, or delirium tremens	1141	98
Enteritis	336	102
Rheumatism	540	87
Heart-disease	—	346
Dropsy	453	153
Phlegmon and ulcers	392	13
Eye-disease	87	—
Fractures and contusions	107	506
Wounds	219	63
Drowning	—	221
Other violence	382	69

We see that, besides the zymotic, or rather the miasmatic order of the zymotic, diseases, there is another group of disorders which causes an enormous excess of mortality among the troops in Bengal as compared with the middle-aged male population in England, and that is, the diseases of the cerebro-spinal system, including delirium tremens and other consequences of intemperance, as well as apoplexy and sunstroke, &c. The relative deaths in the two countries are 4492 and 856! There is also an excessive disproportion between the rates of mortality from rheumatism; but the disproportion may be much less than appears on first sight, as no deaths from heart-disease are set down in the Bengal column, and 346 occur in that for England. Diseases of the respiratory organs, if pulmonary consumption is included, are much more frequent and fatal in India—and, we may add, in other warm and tropical countries—than most persons imagine. They occasion the proportionate mortality of 3559 deaths in Bengal to 4550 in England. Consumption is very considerably less fatal in the former; but bronchitis and pneumonia are vastly more so—a fact full of significance as respects the clothing, &c., of the troops.

That a large proportion of the mortality in India is due to the operation of noxious influences, which may be mitigated or altogether removed, is proved to demonstration by the present inquiry. The very same causes of excessive sickness and death are at work in some of the countries of Europe at the present day; and they were equally destructive in our own land in former times. The Commissioners have done well to compare the existing state of things in the chief cities of Hindostan with what it used to be in our metropolis in the seventeenth century, by quoting the instructive extract from the Registrar-General's annual summary for 1859, comparing the fatality of some of the diseases in London in that year and two centuries ago, and which our limited space only prevents our giving at full. The yearly death-rate during the twenty years, 1660–1679, in the city and in Southwark, was more than 7 per cent. of the population. Comparing the relative mortality from different diseases, or groups of diseases, then and now, we find that the deaths from the different kinds of fever, continued and paroxysmal, were in every 100,000 living as 749 to 59; or, including scarlatina, quinsy, and croup, as 759 to 227. Small-pox slew 357 in the first period, and 42 in the second period. Now, not above 8 in 100,000 die of dysentery; then, out of the same number, 763 perished in the year; and this was exclusive of the mortality from diarrhœa and from cholera; from the former of which 11 died then, and 120 die now; while from cholera the deaths were 7 in 1859, and 130 annually in the twenty years, from 1660 to 1679. The deaths from scurvy then and now are as 142 to 2. Consumption and diseases of the respiratory organs were then very fatal, and still continue to be so, although in a much less degree; the relative numbers of the annual deaths from this source are 1079 and 1611. Children were rapidly swept off; from convulsions and teething 1175 died then, 136 die now. Syphilis appears to have been twice as fatal formerly as it is in the present day. Besides these and other endemic and permanent causes of death, our forefathers were subject to recurring outbreaks of the terrible plague, which, upon an average of the twenty years, annually carried off 1132 lives. In epidemic years the carnage was frightful; in 1665, nearly a third of the population of London perished. What has been done for London, and for England generally, may be done, and with like results, for Calcutta and for the whole of India; for similar agencies produce similar effects on health everywhere—modified, indeed, by climatic and other influences, but still fundamentally leading to the same great issue.

These influences, as affecting the health of troops in India, are either general, climatorial, and geographical, operating more or less actively over the whole country; or they are topographical, local, and personal, being connected with peculiarities of site, the sanitary condition of buildings, &c., the hygienic arrangements in respect of food, clothing, duties, &c.

That the high heat, large amount of atmospheric humidity, and the great variations between night and day temperature—the three principal characteristics of the climate of India generally—are liable to produce marked deteriorating effects on the constitution of most Europeans, when they are long exposed to their influence, cannot be doubted. Excessive heat and moisture serve moreover very generally to favour the development and intensity of certain miasmatic diseases, whose origin is usually connected with the decay and decomposition of organic substances, and also to give force and malignancy to congenerous epidemics of occasional occurrence, while at the same time they tend to increase the susceptibility of the human system to their invasion. Many of the most formidable outbreaks of pestilence throughout the world have taken place under the fostering influences of excessive heat and moisture, especially when these conditions have been associated with atmospheric stagnancy. The production of that subtle agent "malaria," whose existence is known only by its effects on health—for of its material or physical nature little or nothing has been discovered—is intimately connected with the exposure and drying of the earth's surface after it has been soaked with water. Hence, when the autumnal season has set in, in September and October, malarial fevers and alvine fluxes are usually most prevalent. Malaria shows a marked preference for low levels; it is most active at night, and its power is most felt near to the surface of the ground. It may be wafted from its source just as fogs are apt to be, and thus reach localities at a considerable distance. Its production is always lessened, and has often entirely ceased before regular cultivation of the soil; and, on the other hand, it becomes rapidly increased by lands being deserted and allowed to run to waste. Whatever lowers or disturbs the tone and vigour of health renders the system more susceptible of its action. An insufficient or improper diet, excess of all sorts in food, drink or venery, over-prolonged fatigue, exposure to night or early morning air on an empty stomach, sleeping in close unventilated rooms and quick transition into the open air, a frequent course of chills, mental depression from whatever cause; these and all such influences give potent aid to malarious agency. It is by wisely acting on this knowledge that the medical officers will best counteract its effects on the hygiene of troops in India; for the day is yet far distant before we can reasonably hope to eradicate the mischief itself. The whole country, especially the great plains along the course of its mighty rivers and their countless tributaries, is one vast malaria-producing land. Bengal is more malarious than the other Presidencies, and Lower Bengal is the worst region of all. Hence the all but invariably greater sickliness and death of our troops there than elsewhere; and, unfortunately, a large part of the army has generally been stationed near the banks of the huge foul stream of the Ganges in this very part of the peninsula. At Calcutta and at Dum-Dum there is accommodation for above 3000 troops, in positions so low that the tide washes into all the neighbouring ditches and saturates the subsoil with water. Thence along the valley of the river, as far as Lucknow, there are upwards of 15,000 troops located at stations on a nearly dead flat, and not much above the sea-level, with no means generally of natural drainage, and where the ground becomes wet or marshy in the rainy season. And such is the character of numerous other localities away up to the North-West Provinces, and, indeed, in the plains in almost every region of the peninsula. Unhappily, the natural disadvantages of the land have not unfrequently been not a little aggravated by the little attention hitherto paid in the selection of the sites of military stations. At Allahabad, which is partly 40 feet above the Ganges, one-third of the station is actually lower than the level of the river. At Berhampore, the station is below the river Bhagirutty, which skirts it. The position of the European barracks at Cawnpore is declared by Sir P. Cautley, one of the principal engineer officers in the Company's service, and a member of the Commission, to "be about the worst that they could have occupied; they are positively upon the lowest spot of the whole ground. The drainage from the bazaars passes through the lines of the barracks. I would condemn the infantry barracks at Cawnpore altogether, both on account of site and material." It is needless to multiply instances of the same sort; they abound throughout the evidence, oral and documentary, from all the Presidencies. The elaborate and excellent report of Inspector-General Dr. Macpherson, on the stations in the Madras Presidency, is especially worthy of notice on this head, and its value is not a little enhanced by the clear and emphatic remarks of the Commander-in-chief, Sir Patrick Grant, which accompany it. Referring, therefore, the reader for details to the stational reports, which contain so much instructive matter to every military medical officer, we shall briefly throw together a few memoranda on the selection of sites for encampments and stations in tropical climates.

An open, dry, and somewhat elevated site, having natural facilities for land and domestic drainage, should always be preferred. A gravelly soil is of course the best; as the soil or subsoil is retentive of wet, it is correspondingly undesirable. Valley bottoms, with near high land around them, are never safe, especially in hot climates; neither are the lowest slopes or the ground close to the base of the hills, or wherever the water of the uplands finds its natural outlets. This holds true at all elevations; for regard must ever be had not merely to the altitude of the spot above the sea-level, but also to its relative height in respect of the surrounding district. The character of the immediately adjacent land, and its condition as to wood and undergrowth, the existence of marshes, stagnant ponds, or sluggish streams, particularly to windward, have to be considered; likewise the general direction of the prevailing winds, the water supply, &c. Camping close to recently dried-up river-courses, or on ground liable to be inundated after heavy rains, has often been followed by much sickness. The surface may be dry, and even parched at times, yet the atmosphere may be damp, as hygrometric observations will show. Wherever partial fogs settle and persist, or where wearing apparel or leather accoutrements become speedily mouldy, we may suspect an excess of atmospheric humidity, although rain may have been long absent and the surface of the ground is arid. Broken and irregular ground, full of hollows, where moisture and vegetable decay may abound, will render a position, otherwise unobjectionable, unhealthy. Proximity to deep damp ravines or hemmed-in hollows in tropical countries is often attended with the prevalence of malarial exhalations. The mouths of gorges and the passes between hills in these climates are very generally unhealthy; they are subject to great and rapid atmospheric changes, from oppressive stagnant heat at mid-day, to cold, damp, and it may be also miasmatic currents of air sweeping through them in the evenings and mornings. This was one, at least, of the causes of the dreadful mortality among the troops at Hong Kong for some time after its occupation; and the same results have occurred elsewhere.

It is a general belief that sites close to the coast, where they are freely open to the sea breezes, are and ought to be salubrious. Yet it is well to know that in various regions of the world such spots have proved most deadly to the soldier, while an inland and elevated spot not far off has been comparatively healthy. This has been most markedly the case in seasons of epidemic sickness, which has often been then confined to, or been most severe in, stations near the shore. Much—very much—will depend on the artificial and acquired, as well as on the natural, characteristics of the locality. Besides an excessive amount of aerial humidity—a very influential element in the development of many diseases—on the sea-coast, the atmosphere is apt to be most perniciously tainted by the shore becoming the depository of much decaying waste matter of all sorts, brought down from the land or washed up from the sea, and also of divers filth and garbage which rot on the surface or partially sink into and impregnate the soil. If to these sources of pollution be added a semi-stagnant stream, whose foul waters, commingling with the salt water at every slight rise of the tide, every now and then overflow and recede from the low ground about, then indeed there are some of the most potent elements for the production of disease in a hot climate. Yet on spots like these barracks have been built, scarcely raised off the damp ground, and with no attempt to prepare a dry foundation; and men have been huddled into them, to die off like rotten sheep within six months of their landing. Such accommodation was as bad, but probably not worse, than no accommodation at all, as was the case with the regiment that was sent out from this country in 1825 to Sierra Leone and landed in that deadly climate before any barracks had been built, and of whom two-thirds perished before the end of the year! So much for the health-organization of our army at that time; nor was it much better in the first China war, only twenty years ago, when from thorough mal-administration and culpable neglect of all the most obvious sanitary precautions in respect of location, &c., the troops at Chusan perished by hundreds within a short time of being landed. One regiment was almost swept away within two months; at one time it could not muster twenty men in health! Even in more recent years instances have occurred where no small mortality has been due to the unsuitableness of the spot chosen for the location of troops, in permanent as well as in temporary occupation. Little attention has yet been paid to the drainage of the ground of military sites, a matter of the highest importance in cantonments, barrack-yards, &c. Means should be taken not only for the prompt removal of surface water and the prevention of all pools and stagnant accumulations, but also for the drainage of the subsoil, so as to counteract the lodgment of water a few feet, it may be, below the surface, and whereby the superincumbent atmosphere in a spot is not infrequently kept charged with malarious humidity. The conclusion of the rainy season in the tropics is generally attended with great increase of disease; and the wetter the subsoil of a locality is, the more sickly it will in general prove. The floors of barracks and hospitals are often damp, and therefore unwholesome, from this cause. In the flat plains, where there is little or no fall of the ground, there will of course be often great difficulty in leading off the drainage water, except by artificial aid; but this difficulty only shows that the locality can scarcely fail to be insalubrious unless every sanitary precaution be used. And among these precautions, there is perhaps none more necessary than that of keeping the subsoil easily permeable, and the foundation and basement-floors of buildings thoroughly dry and fresh at all times. This remark leads us on to notice the important subject of barrack and hospital construction in tropical climates. A vast amount of information on this point has been gathered together by the Commissioners, and been subjected by them to a sharp and critical scrutiny. It would seem that there is not a thoroughly well-constructed or model barrack in India. They are either much too large, accommodating far too many men in one building, however spacious the rooms may be; or, if they are small or of moderate dimensions, they are badly built, badly arranged, or badly ventilated. Nothing certainly can be worse than the old large barrack-buildings, arranged so as to form a square, or three sides of a square, with long, low, ill-ventilated, and ill-lighted rooms, in which a hundred or even several hundred men were congregated night and day together, breathing impurity within, and exposed to malaria without. Such were the barracks at Secunderabad, Chinsurah, &c., which, after causing thousands upon thousands of unnecessary deaths, were condemned as incurably bad several years ago. The mere size and extent of the rooms in many of the recently-constructed barracks—as, for example, in the magnificent Dalhousie barracks at Fort William—are, in the opinion of the Commissioners, sufficient to render them unhealthy, notwithstanding the amplitude of accommodation and the large amount of breathing space allowed to each man.

> "An essential condition of every barrack-room is that the air in it should be as pure during the night as the air outside; but it is impossible to ensure this if the rooms are above a certain size, and contain above a certain number of men. Whenever the number exceeds twenty or thirty per room, it is practically far more difficult to ensure fresh air, and *beyond that number it soon becomes impossible.*" (p. 99.)

We are not aware of the evidence on which this strong assertion is made. It is added that—

> "Sad experience has proved that long rooms, like passages, with 100 or more persons sleeping in them, may become highly dangerous during epidemics, and absolutely pestilential if occupied by sick. The reason of course is that the direction which the foul air may take in the room depends on accidental circumstances, not under control, which may lead to its being accumulated at one end of the room, or over one group of beds." (p. 99.)

If such be really the case with all long rooms, notwithstanding every attention to their ventilating arrangements, the barracks at Fort William and elsewhere must inevitably have been found unhealthy; but this is not alleged, and their condemnation seems to rest more on theoretical grounds than on the results of experience. We have failed to discover any information, throughout the whole evidence, about the health-condition of the troops quartered in these new barracks as compared with previous barrack accommodation in India. This is to be regretted, as such a comparison could not have failed to be useful, by affording grounds for guidance, not only in India, but in other countries. Meanwhile, a strong prejudice may be excited against the healthiness of the Dalhousie and similar recent barracks by the censure of the Commissioners. Probably all will agree with the opinion of Sir John Lawrence, that not only on sanitary grounds, more especially when any epidemic sickness threatens or prevails in a station, but also on moral and social grounds, and for the general well-being and comfort of the soldier, small separate barracks for half a company, or for a quarter of a company, are preferable to larger constructions. It may increase the difficulty of ready supervision by the non-commissioned officers, and on this ground many leading officers whom Sir John consulted gave the preference to large barracks; "but I think," says he, with his accustomed straightforwardness, "that barracks of a limited size are better for the men," while admitting that the lofty, spacious structures which had begun to be built in the Punjaub on the plan advocated by Sir Charles Napier, were very fine barracks, and that the men were extremely well accommodated in them.

Two of the principal structural defects in Indian barracks hitherto are that they are not sufficiently raised off the ground, and that the roofs are generally made single. The plan of having a clear space between the ground and the basement of the building seems to have been very seldom adopted there, although it is common in barracks in the West Indies. The Commissioners recommend that all future barracks be erected on raised basements, with the air circulating under the floors, and that in all existing barracks the floors be raised as much as possible, and a free current of air allowed to pass beneath them. Other recommendations are, that they should not contain more than a quarter-company in each building, or at most half a company in two separate rooms, quite distinct from each other; that they should have single verandahs only; that the cubic space in each room per man be from 1000 to 1500 feet, and the superficial area be from 80 to 100 square feet, according to the airiness of the position; that there be never more than two rows of beds between the opposite windows; that the ventilation be sufficiently provided for independently of windows and doors; that there be a sufficient glazed-window space for the admission of light; and that the rooms be better lighted at night than hitherto they have been. These recommendations are most judicious, and cannot but be salutary. The foundation of the building must be made and kept thoroughly dry, and the surface of the ground beneath the basement maintained scrupulously clean and sweet. We have known neglect of this precaution prove very hurtful to the inmates of an otherwise well-contrived building in tropical climates. The roofs of barracks and hospitals in hot climates should always have ventilating turrets, or some analogous arrangement, for the withdrawal and discharge of the vitiated air from the wards; and for the same purpose the roof should be double, having a sufficient intervening space between the ceiling and the outer roof. This is indeed necessary in all climates: in a hot climate to keep the upper floors sufficiently cool during the day, and in a cold climate to keep them duly warm. The interposition of a stratum of air between the outer atmosphere and the atmosphere of the chamber not only facilitates ventilation, but also greatly aids in maintaining an equableness of temperature. Whitening the roofs of buildings serves materially in hot climates to reduce the heat within. That the verandahs should be single does not admit of a doubt; to have a verandah within a verandah is to do away in a great measure with one of its principal objects, and needlessly to darken the interior chamber. As to the cubic and the superficial floor-space for each soldier, the amount recommended by the Commission is anything but too great, if the barracks are to be one-storied buildings, and the same room is to be a day and a night-chamber. We cannot but think that permanent barracks in the plains of India should always be two-storied, the lower floor to be occupied with day-rooms, for meals, workshops, libraries, &c., and the upper floor to be used as sleeping-rooms only. No arrangement would so effectually contribute to the general welfare and improvement of the soldier, as well as to the maintenance of his health. Sir John Lawrence and Dr. James Bird, two most experienced authorities, strongly advise it. Although alluded to by the Commissioners, it is not expressly recommended by them. We hope that the subject will be well considered by the Indian executive before new barracks are erected.

As to the best means for maintaining the due ventilation of the barrack-rooms by night as well as by day, without permitting, on the one hand, stagnation at any time in their atmosphere, or, on the other hand, an excessive amount of chilling draughts upon the men, especially when they are in bed, no great difficulty should be experienced, we think, if the verandahs are provided, as they ought to be, with jalousies, the windows of the rooms are of proper dimensions and are glazed, and the doors, especially their lower panels, are louvred, the louvre-blades being movable, so as to admit (or, when need be, to intercept the admission of) the fresh air from the verandah when the door is shut. This mode of supplying air from without, near the flooring, is much preferable, we take it, to that suggested in the Report—viz., from under the eaves, along the top of the walls. In this plan the cool night air would descend towards the floor, and consequently, right upon the men when in bed; in the former it would ascend from near the floor towards the ceiling, whence, when it had become impure by the breathing of the inmates, it would find its escape.*

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* Sanitarians are still at issue on this important point in the ventilation of rooms—viz., the proper position of the inlet openings. The Sanitary Commissioners to the Army in the Crimea stated, in their Report in 1857: "While providing an exit for the impure air (in, or close to, the ceiling) it is necessary to have means for admitting fresh air. This can be most readily done in most instances through the windows, or by making openings through the wall near the level of the floor." In the plan of the model hut recommended by the Commissioners, the fresh air is admitted by slightly raising one of the lining boards near the flooring. The Barrack Commissioners, in their recent Report, altogether reprobate this plan, alleging that "the cold air admitted thus, intsead of mingling with the air of the room, blows among the men's feet to the fireplace, thus lowering the temperature near the floor, where it is always most important to have the air warm, and risking the health of the men." And so strongly

The condition of Indian barrack-latrines is, of course, thoroughly bad at almost every station; for where abroad has this subject attracted any attention in respect of the accommodation for the private soldier? In many parts the state of these places has been quite scandalous; nor can any one doubt that their extreme offensiveness has often been mischievous to the health of the troops. The atmosphere all round is continually polluted with the foulest effluvia, so that the very approach to the latrine causes nausea and disgust; and this, too, in a climate where every such nuisance is aggravated tenfold by tropic heat. It is difficult to exaggerate the magnitude of the abomination, or the crying necessity for the prevention in future of such an evil. The recommendations of the Commissioners on this head are in the following paragraph:

> "That, wherever practicable, iron or earthenware water-latrines, supplied with water, and drained to an outlet, be introduced instead of the present system; that, where this is impracticable, all cesspits be abolished, and metal or earthenware vessels, to be removed twice a day, substituted. That improved urinals, supplied with a jet for lavatory purposes, as well as with a free supply of water for the cleansing and drainage of the urinals, be provided." (p. 168.)

As to water-latrines, they are doubtless the best arrangement, if a suitable outlet can be provided, with the fair prospect that the discharged contents do not give rise to a stinking morass near the station; but herein will be the great difficulty in very many places. No mention is made of trenches at a suitable distance from the barrack-buildings, and where each day's ordure might be regularly covered with mould or with charcoal, until the trench be nearly filled up, to be then replaced by another trench in like manner. This plan has been occasionally used; and, at all events, it is infinitely better than that of allowing enormous accumulations in cesspits, to be imperfectly emptied twice or thrice a year. The trenches are sometimes concreted at the bottom, and then each day's or every two days' accumulation is removed by hand labour. The pictorial illustrations in Miss Nightingale's paper give a capital idea of the "Indian Drainage System." Whatever be the plan adopted, there must be systematic and very frequent removal from the neighbourhood of dwellings, so to prevent atmospheric and telluric contamination. Nor will attention to its mere removal suffice; the ordure must be conveyed to a sufficient distance, and means be used for its prompt destruction by interment or otherwise. We have heard of barrack-privies being emptied into pits, dug close at hand; and, as the work was done at night, no one was the wiser. The use of fire for the destruction of dry rubbish and refuse of all sorts has not been sufficiently tried: it affords a ready

are the low-placed inlet openings condemned that, whenever they were found in barrack-rooms and hospitals at home, they have been closed! Our own experience inclines us to the views of the Crimean Commissioners—viz., that the admission of fresh air, more especially in tropical climates, at the lower part of a chamber is, as a general principle, to be preferred. The best-ventilated hospitals have adopted this plan. This point in practical hygiene evidently requires to be well inquired into at the present time, when so many new hospitals, civil and military, are being built, both abroad and in this country.

and most effectual means of thorough purification. It was much more in vogue among the ancients than with us. In India, it seems to be generally resorted to for the consumption of the refuse of stables and cowhouses, but not of latrines; but if suitable for the one, why not for the other? We know, too, that cremation takes the place of sepulture among many of the natives; and the same thing was done in ancient Judæa with all the offal and garbage of the cities. In tropical climates, the brushwood, &c., around cantonments requires to be frequently kept down, and this can be best done by burning; the ashes too, spread on the surface, do good. An unlimited supply of charcoal might always be had; and the more freely this is mixed with all decomposing matters before removal, the better in every respect.

The arrangements for the water supply in Indian stations are universally defective, and the quality of the water must very often be bad, being derived from either open tanks, shallow wells, or from rivers—all liable to pollution. The descriptions given of the tanks by many of the witnesses must satisfy every one that the water, unless it be most carefully filtered and purified after it has been drawn, cannot but be loaded with organic matter. The universal method of distribution is by hand labour; nowhere in India does it seem that water has been laid on to buildings, either in military stations or in cities. It is drawn from the tank or well by dipping skins or other vessels, and is carried by "bheesties," or water-carriers, to the barracks, and there emptied into such receptacles as may have been provided. Although several of the statements in the part of the Report on this head will be considered by many to be chargeable with over-colouring, and to be somewhat dressed up to establish particular views, all will assent to the proposition of the Commissioners, that "an abundant supply of pure cool water for drinking purposes is an essential requisite for all barracks," and "that every hospital should be provided with a constant supply of pure filtered water. Whatever be the source, the water should be laid on to every barrack and hospital directly by gravitation; or, if local circumstances prevent this being done, the water should be raised by mechanical power, and stored in tanks (covered), at a sufficient elevation to enable it to be distributed by gravitation." They add a most useful suggestion, in which we heartily coincide—that drinking-fountains be provided at all necessary points, both in the barracks and near the stations generally. If the water be kept thoroughly cooled at the same time, no greater boon could be conferred on the soldier; for what traveller does not know the luxury of this simple beverage under a tropical sun? Ice might be easily supplied all over India.

This remark naturally enough leads us on to briefly notice the subject of intemperance—a frightful source, alas! of sickness, death, and crime among our soldiers everywhere, and especially of course when the heat of the climate occasions continual thirst. Every one of the stational reports draws attention to this terrible evil; and who can wonder at its wide prevalence, when the allowance to each man is, as Sir Charles Napier tells us, "two drams a day, and eight of these drams make a quart bottle? So the sober soldier swallows one-fourth of a bottle of raw spirits every day! You and I know them too well to doubt that the other three-fourths go down after the first." It has long been, and still is, a deep reproach to our country that this most mischievous practice is not only sanctioned but is actually instigated and encouraged by the Government for the maintenance of the "canteen fund," which pays the expenses of the theatre, fives-court, and other amusements for the men, and even for the covers of the caps which they wear. In the West Indies this state of things was (is?) still more disgraceful; for there the Ordnance authorities derived, a few years ago—and perhaps still derive—a considerable revenue each year from letting, at exorbitant rents, canteens; the proprietors of which, nevertheless, made large profits by the sale chiefly of the worst spirits at the highest price to the soldiers. The subject is pointedly mentioned in the description of the barracks in Jamaica, in the official report on that island in 1853. The Commissioners most justly reprobate the existing system, and urge its immediate discontinuance. As to the best drink for the soldier, no one will doubt that the use of sound malt liquor is greatly to be preferred to ardent spirits; but the constant or unrestricted use of malt liquors is not without its disadvantages in tropical climates. We agree with Dr. Bird that light wines, or the stronger ones diluted with water, effervescing and such-like beverages, with tea, coffee, and cocoa, are the safest and best drinks. No allusion is made in the Report to the use of tobacco. Smoking in moderation is, we think, rather useful than otherwise in these climates, provided always it be not associated with tippling. Few things are more refreshing after fatigue in sultry weather than a pipe or cigar with a glass of iced water: it invigorates without excitement.

The subject of diet of the soldier is discussed at considerable length by the Commissioners; but our limits prevent us following them on this matter or on that of dress, offering but the single remark in reference to the latter, that experience is strongly in favour of woollen clothing next to the skin, and of the outer dress being loose and easy. Then, too, the recreations and amusements of the men; the great and seemingly increasing amount of venereal disease in most of the stations; the restrictions upon marriage among the soldiers; the bad accommodation hitherto provided for the married men and their families; the too-frequent neglect of the poor women and children, with the frightful consequences contingent upon this neglect, as at Dum-Dum in 1858, the sanitary condition of the native lines, bazaars, and cities where so much of the disease among the European troops is bred—all merit attention if we had space. The evidence of Sir John Lawrence on the marriage of soldiers should be read by every one interested in the welfare of the army. Besides these questions, the great subject of hill stations and of sanitaria, whether on high elevations or on the sea-coast, or away from India, as at the Cape, Australia, or Tasmania (which, although not mentioned in the Report, is worthy of notice), would require an article to itself to do it justice. The general scope of the evidence, both in India and in other tropic lands, seems to be that heights of about 2000 feet above the surrounding plains offer, on the whole, the most suitable positions for barracks in the hills. We have been surprised to find that the camping out of troops once or twice a year has been so rarely resorted to as a hygienic and sanitary precaution in stations on the plains; it has long seemed to us that it would be highly useful for the health of the troops and the wholesomeness of the barracks. The limitation of the period of service in India to ten years, and the location of at least a third of the whole force on hill stations in rotation, are excellent suggestions. It requires but the efficient carrying out of these and the other recommendations of the Report to bring about, and that speedily, an immense diminution in the sickness in our Indian army, and a reduction in the mortality by at least one-half—say from 50 to 25 or even to 20 per 1000 of the strength in the year. And while so much is being done for the one service, let not the other, equally entitled to the nation's solicitude, be overlooked. The losses by disease in the Navy on the East India station of late years have been excessive, considerably greater than they used to be, and even beyond the losses in the army. The subject clearly demands inquiry, so that the cause or causes of this unsatisfactory condition of our ships of war may be discovered, and the proper remedy applied. Moreover, our military and other establishments in the West Indies and other distant possessions stand much in need of a like searching inquiry as that directed to our empire in the East. It is only a few years ago that an entire fourth (between three and four hundred men) of a garrison there were swept off by malignant fever within two or three months. Wherever we turn, abounding evidence will be found that an enormous proportion of the sickness, wretchedness, and death in communities and peoples is due to causes patent as noonday, and easily capable of mitigation or removal. The field is a glorious one to the medical philosopher and the philanthropist; and the appearance of the great Indian Report will, we trust, serve to stimulate inquiry and exertion everywhere; for wherever the field is intelligently and earnestly cultivated, a rich reward of the truest beneficence will infallibly be reaped.

Little did we think, while writing these pages, that the distinguished man whose opinions we have several times quoted should now be on his way out to undertake the Government of India. May his life be long spared to carry out, among other social reforms, the measures necessary for improving the health, and promoting the moral and religious welfare of the soldier!

PART SECOND.

Bibliographical Record.

ART. I.—1. *The Causes and Nature of the Vascular kind of Bronchocele, and of the Pulsations and Palpitations termed Anæmic. By* THOMAS LAYCOCK, M.D., &c. (Reprinted from the 'Edinburgh Medical Journal' for July, 1863.)

2. *Vascular Bronchocele and Exophthalmos.* By J. WARBURTON BEGBIE, M.D., &c., Edin. (Reprinted from the 'Edinburgh Medical Journal' for September, 1863.

THE two papers before us refer specially to the *causation* of the interesting disease which forms their subject. The first, to attempt to establish a relation betwixt, and to ascribe to a common cause, its three characteristic symptoms—viz., vascular and cardiac excitement, enlargement of the thyroïd gland, and protrusion of the eyeballs—was Dr. Begbie, senior. In 1849 that physician read to the Medico-Chirurgical Society of Edinburgh a paper, in which he attempted to prove that this remarkable triad of symptoms originated in an impoverished state of blood. This paper forms one of those elegantly-written and practically-valuable essays in the author's 'Contributions to Practical Medicine.'

Since Dr. Begbie, senior, made known his theory of the anæmic origin of exophthalmic bronchocele, another view has been advanced, ascribing the disease not to a *humoral* origin, but to a special affection of the nervous system, termed a *neurosis.* (Stokes, Trousseau, Aran.)

In the first of the papers placed at the head of this notice, Dr. Laycock seeks to apply the results of modern physiological research in explanation of the various symptoms of this form of bronchocele. He believes that all the phenomena are due to a "neurosis" of the cerebro-spinal tract, or rather of several vaso-motor centres in the spinal cord. For example, the exophthalmos is explained by supposing an affection of the "oculo-spinal" centre, which Budge and Waller showed to extend from the first cervical to the sixth dorsal nerve, and which, as Bernard has recently shown, sends its nerves to the eye through the first and second dorsal nerves. The bronchocele again, which is essentially a dilatation of the vessels of the thyroïd gland, Dr. Laycock believes due to a lesion, of a paralysing kind, of the trunk of the sympathetic. The cardiac palpitations are attributed to a lesion (of an irritative description ?) of the newly-discovered centre for cardiac excitation—of a tract of the spinal cord extending from the seventh cervical to the fifth dorsal nerve (Von Bezold). As a further explanation of the cardiac and vascular excitement, an increase in the heat of the blood (from paralysis of the capillaries in certain parts) is shown to have a stimulating effect on the muscular tissue of the heart and arteries. Finally, the therapeutic action of cold is explained on the supposition that it diminishes the cutaneous production of heat.

As showing the nervous mechanism of various symptoms, Dr. Laycock's paper is very interesting. But in furnishing us with the *causation* of this curious group of symptoms we think it fails. What is a "neurosis?" Is it an irritative or a paralysing lesion, or a mixture of the two? Dr. Laycock's theory would imply the latter. The causation of one symptom demands an *irritation*, of another a *paralysis*, of the vaso-motor nerves. Thus, exophthalmos is artificially produced, not by division of the cervical sympathetic or of the two first dorsal nerves, but by galvanic irritation of these nerves or their spinal centre (Bernard). Again, the bronchocele which originates in a dilatation of the vessels of the thyroïd would point to *paralysis* (or in an experiment, section) of the cervical sympathetic. Adopting Bezold's experiments, an *irritation* of the cardiac centre produces increased cardiac action (palpitation). The exophthalmos implies irritation of the sympathetic trunk: the enlarged thyroïd implies an opposite condition of the same nerve: a neurosis would thus seem to be a strangely-compound lesion. Dr. Laycock would have done well to have pointed out this difficulty in his theory, and to have given some explanation of what appears to be a flaw in his physiological argument.

There are other points of interest on which we would remark, but space forbids, and we hasten to notice Dr. W. Begbie's paper, which may be regarded as an able defence of the anæmic origin of this disorder. Whilst Dr. Laycock considers exophthalmic bronchocele as a disease of innervation, Dr. Begbie advances arguments tending to prove it to be a disease of nutrition; or rather, while admitting the nervous mechanism of the disorder, he attributes its cause to an impoverished state of blood. He points out the frequency with which the sufferers from bronchocele have presented adequate cause of blood impoverishment (uterine hæmorrhage, hæmorrhoïdal flux, leucorrhœa, diarrhœa, &c.), and also the frequency of the signs and symptoms of anæmia itself; that the remedial means are those of anæmia, and that the structural changes in the heart (noted by Sir H. Marsh and others) are such as long-continued functional disease in connexion with anæmia would produce. The order of sequence in the symptoms are stated to be—1, blood impoverishment; 2, cardiac and vascular disturbance; 3, enlargement of the thyroïd; 4, protrusion of eyeball. As to the mechanism of the disease, the author thinks that the impoverished blood acts on the nerves of the bloodvessels themselves. Other writers hold that it is the central organs that are affected. To us it appears that there is just as much actual *proof* of the one view as of the other. In adopting anæmia as a cause of the disease, we have certainly something more appreciable and definable than the so-called "neurosis." But we believe, that, although anæmia is, as Dr. Begbie and his son have shown, a very constant element in these cases, something more is required to produce such an odd group of symptoms. In favour of the anæmic theory, it is further remarked that the treatment most beneficial is that of blood impoverishment, the salts of iron, good diet, &c. Benefit may also be obtained from belladonna or atropia, internally and externally. Dr. Begbie observed spleen enlargement in two cases.

Whilst in our opinion neither of these papers have quite settled the pathology of the exophthalmic form of bronchocele, both have contributed to our knowledge of this disease. Dr. Laycock shows his ability in applying the discoveries of physiology to elucidate pathological questions, whilst Dr. W. Begbie fully sustains his reputation as an accurate and profound clinical observer.

ART. II.—*A Treatise on Gall-Stones—their Chemistry, Pathology, and Treatment.* By J. L. W. THUDICHUM, M.D., &c. Illustrated with Plates.—*London,* 1863. pp. 323.

THE paper-duty is remitted; printing is not very costly; highly educated young physicians and surgeons abound, by whom practice is wanted; time drags heavily without employment; "cards" and advertisements are forbidden them; papers read before the select few, the conscript fathers of the medical societies, bear little fruit, and pamphlets none at all: *ergo,* the facility and desirability of publishing a book which may make its author's name a "household word" in connexion with the chosen theme. The determination taken to make a book and practice by it, the aspirant's further course is easy; the well-filled shelves of the great medical libraries, particularly if fortune has cast his lot in London, are open to his explorations, and, with the aid of "systems" and monographs of the well-known and quoted authors and the little-known ones—especially if their wisdom be hidden under the cloak of a foreign tongue—time and industry are the only other conditions required for the purpose. No marvel, therefore, that bookmaking flourishes, particularly on pet subjects, with the public.

We are not so uncharitable as to hurl condemnation on all such monuments of bibliothecal research and industry; for if their constructors be well informed, and gifted with analytical talent and critical acumen, the resultant works are often of service, particularly to the busy practitioner and hard-working student, who have neither time nor opportunity to read monographs and weighty original treatises in which the hypotheses and lessons of the handbook are elaborately wrought out and illustrated.

These remarks have been suggested because Dr. Thudichum, like most authors, deems it necessary to explain why he should write upon gall-stones. He rightly attempts no apology for so doing, but affords very satisfactory reasons. His first reason—the intrinsic importance of his subject—might be anticipated, but for the second, few, we think, will be prepared, judging from the many books written, the search for topics, and the ardour with which pathological and chemico-pathological researches have of late years been pursued—viz., "that there has not been published a monograph upon it in the English language for upwards of a century." He has other equally valid, or, as should be said, more excellent reasons for writing on gall-stones; foremost among which is the fact that he has made them the subject of especial investigation, particularly with reference to their chemical nature and composition—a task which Dr. Thudichum, as a well-known and practised chemist, was better able to undertake than most physicians. He tells us he aimed to accomplish for the chemical history of gall-stones what Morgagni has done for their physical description — namely, "a complete account of the analysis, ingredients and composition of all varieties of gall-stones of man and animals, together, if possible, with an analysis and explanation of the morbid process to which they owe their origin."

In carrying out his plan he was necessarily led to study the chemical nature of the bile, its decomposition by putrefaction, and other questions relative to that fluid, and thus arrived at a new theory of the origin and nature of gall-stones, and at an amended classification of them. With true Germanic patience, he has also ransacked the libraries for the literature of his subject, undeterred by the antiquity, the quaintness, the obscurity, or the language of authors. His historical and descriptive notices of works occupy therefore much space in his book, and would be out of place in any other publication than a monograph, where they may prove of use to the special student for the purpose of reference. We doubt not that this portion of his labours grew, so to speak, insensibly and unintentionally under his hand. Indeed, he intimates that he was drawn into it by the prevalent lazy practice of authors, particularly of the bookmakers, who, to show their acquaintance with preceding writers, quote references to them from other works without verifying their correctness.

> "During my literary inquiries," he remarks, "I found that most systematic writers were so incorrect in their statements and references, and, not rarely, so unintelligible in their accounts, that it became an essential part of my work to read or consult as many original authors as were accessible to me. The results of these labours have been corrections of numerous mis-statements, propagated by compilers, as I could observe in some instances, from one to the other, through the long space of two centuries. The necessity of these rectifications, together with the positive results of this search in books, appeared to me to afford some justification of the length of my first chapter."

Dr. Thudichum's literary research on his chosen subject is prominent throughout the book. On every topic that arises in the course of discussion he stays to tell us what this or that physician, of this or a past age, said or did in the matter; and the "results of this search in books" are by no means included and concluded in the first chapter especially devoted to them, but reappear in another chapter of sixteen pages, as "Historical Notes on the Chemical Analysis of Human Gall-Stones," as well as in numerous paragraphs on the chemistry of those concretions in the lower animals. Readers intent upon at once finding out what is actually known or supposed at the present day with regard to the nature and formation of gall-stones, may tire in the perusal of past opinions, and will be inclined to turn over the pages until they can discover what Dr. Thudichum has made out and what theory he has to propound. Such readers—and those will form the majority—will therefore value the book for this latest and clearest information, disregarding the hypotheses, both pathological and chemical, invented when pathology was more obscure than it is at present and animal chemistry comparatively in its infancy. Having worked so hard at the literature of his subject, the author might justly pride himself on the accuracy of the matter he has compiled; but we think his criticism on Frerichs too severe and contemptuous, considering the high position that physician holds in Europe for his thorough exposition of diseases of the liver, and regret the following remarks: "The latest account of gall-stones is contained in the work on 'Diseases of the Liver,' by Frerichs (vol. ii. 1861). It opens with what, in the English translation, is termed an 'Historical account,' which is historical only in this respect, that it contains almost as many errrors as sentences." This may be true, but it is not, we would suggest, the way to express the fact, nor is it the sort of recognition of so comprehensive a treatise that an eminent author like Frerichs might expect from a contemporary who has his reputation to make.

The chapter on "The Physical Description of Gall-Stones" allowed little scope for originality, but it makes some points more clear. These morbid products are not the result of a fortuitous concurrence of particles, but of a specific and definable process, and their composition determines their structure. They may undergo considerable changes in form, owing to secondary deposits on their surface formed during the time that they remain after the specific disease of the bile which has produced them has ceased. It is a prevalent notion that biliary calculi are lighter than water, and will float when mixed with it. This is an egregious error. "Their specific gravity was always found greater than that of water, and greater than that of bile," when observed immediately after their removal from the body. In the process of drying, however, they absorb air into their interstitial cavities, and thereby some of them acquire such a degree of lightness that they will float on water. In the natural state these cavities are filled with bile.

Most gall-stones present a brown or black nucleus, which, when scooped out, appears under the microscope in the form of dark, broken masses mixed with fragments of cholestearine. To examine the shape of the dark particles, the cholestearine must be dissolved out by means of benzole or ether, without shaking. A number of calculi from the gall-bladder of a case Dr. Thudichum examined, contained casts of the biliary ducts. This interesting observation he has given in detail, with figures of the casts, and he employs it subsequently (p. 178) in the construction of the following hypothesis of the origin of gall-stones in the biliary ducts:

> "The formation of these calculi is due to a disease of the bile, and a partial obstruction of one or more of the biliary ducts. . . . . The bile is in a state of

putrescence. Every one of its particles as it leaves the liver-cells is decomposed, and precipitates both the cholestearine and cholochrome. The epithelium of the bile-ducts, whether by a failure of nervous influence and of nutritive material (as in the case of the epithelium of the tongue in syncope), or by direct contact with the diseased bile, is mortified and shed; it concretes into masses with the cholochrome and cholestearine and other matters, and assumes, or perhaps retains, the shape of the tubes from the surface of which it was shed. Such a cast either obstructs an entire duct by itself, or a number of casts collect and produce an obstruction. The bile continuing to collect behind the obstruction, deposits more matter and enlarges the duct throughout, and more particularly at the obstruction, to the extent of letting out a portion of the bile. Every portion of the bile as it passes leaves an amount of matter deposited upon the obstruction, which thus grows in size, expands the gall-duct in its turn, and is ultimately found in a bag formed by the enlarged gall-ducts."

The prevailing hypothesis of the origin of gall-stones from inspissated mucus, or inspissated bile, the author rejects *in toto.* "Mucus (he says, p. 166) is very rarely found in gall-stones, and inspissation in the midst of fluid bile is quite incomprehensible. Moreover, even an indubitable inspissation of bile could not lead to an insoluble concretion. . . . . The binding material of the nucleus of gall-stones is cholic acid, or choloïdic acid, or both." These acids, with cholochrome and earthy salts, are the essential ingredients of ordinary gall-stones; the cholestearine they contain is a secondary deposit.

"The process by which gall-stones are formed appears analogous to that which produces that rare description of calculus in the urinary passages—the phosphatic or fusible calculus. It is a decomposition of the bile akin to putrefaction. The compound amido-acids split up into their constituents, under the influence of a cause which remains to be ascertained, but is probably a putrid ferment absorbed from the intestinal canal."

The constituent "cholestearine is dissolved in the taurocholate of soda; but as soon as the acid of this salt is decomposed, the cholestearine is set free, crystallizes, and deposits upon any particle that may happen to be within easy distance."

The preference existing in a writer's mind towards any one science will always betray itself in the opinions and explanations he may have to advance in regard to any other, wherever any relation can be set up between the two. Dr. Thudichum is a distinguished chemist, and we owe to this circumstance a purely chemical hypothesis of gall-stones. We cannot in any way contest the accuracy of most of the statements advanced, but we consider he has overlooked the probable agency of some physiological and mechanico-vital conditions concerned in the formation of these morbid products. The primary link in the chain of causes he assumes—the ferment absorbed from the intestinal canal—is purely hypothetical; whilst his categorical rejection of the possibility of inspissated bile existing in the biliary ducts undissolved by the bile, instead of being favoured by recorded facts, is opposed to them; as also it seems to be indeed to his own partial admission of its occurrence, for at p. 177, he writes, "The calculi found in the bile-ducts in the substance of the liver sometimes contain some inspissated bile which is soluble in water;" and again, at p. 135, he tacitly admits the occurrence of gall-stones at least partially composed of unaltered thick bile.

Moreover, we are not quite prepared to admit the author's fundamental proposition that a state of diseased bile akin to putrefaction necessarily precedes the formation of gall-stones. He advances it in order to find an explanation of the origin of the ingredients he discovers by analysis in the fully-formed calculi, losing sight of those earlier phases of their existence when the chemical processes which he illustrates by the experiments of the laboratory were probably not in course of operation. On the contrary, we know that the hepatic cells may be gorged with bile, that the solid portion may soon preponderate, and that the ducts of the liver may become more or less obstructed with the inspissated secretion; and in this obstruction we can well conceive the first elements of calculi to take their origin, and to lead to the throwing off of such casts as Dr. Thudichum has found to constitute the actual nuclei of gall-stones, around which subsequent deposits take place in the manner he has so well described. We do not perceive the necessity of surmising an antecedent putrefactive process in the bile, for a plugging of the hepatic ducts by epithelium loaded with secretion is even more readily conceivable than the similar proceeding, admitted by all to take place in the tubules of the kidney.

The nucleus once formed, the future growth of the calculus, not simply by accretion but by a chemical precipitation of material derived from the bile, now become diseased, is readily explicable, much after the manner pointed out by Dr. Thudichum. Thus, this able chemist well illustrates the changes that bile undergoes in the act of putrefaction; and although we cannot believe that any process precisely like putrefaction takes place within the hepatic ducts or the gall-bladder, so completely cut off from communication with the air, yet we can readily understand the progress of molecular chemical changes in the obstructed collections of biliary secretion and epithelium. May not, indeed, the presence of epithelium or the secretion of the mucous lining of the ducts constitute the ferment necessary to start the series of chemical transformations? The very fact of the obstruction of ducts leads to changes in the chemical nature of their secretions, and we are disposed to attribute the *fons et origo mali* to vitiated bile, coupled with a diseased state of the epithelial lining of the hepatic ducts, and the consequent throwing off of casts or epithelial cells from them, which serve as nuclei of the gall-stones. The vitiated bile may itself be the consequence of congestion of the liver, or of other disorder connected with the state and circulation of the blood.

"The majority of gall-stones are formed in the gall-bladder, even though their nuclei . . . . have originated in the smallest biliary ducts;" and Dr. Thudichum assumes that there is an adherent film of fermenting or putrefying matter surrounding those nuclei, on their entrance into the bladder, which suffices to set up and ultimately extend the decomposition throughout the whole collection of bile contained within that viscus.

The chapter on the "Chemistry of Gall-Stones" is very complete, setting forth the results of their analysis by the best-known chemists of the past as well as of the present time, together with those of the author himself, and the best modes of proceeding. On these subjects we cannot now enter.

In the course of his chemical investigations the author was led to propose the following new classification of gall-stones:

First Series. Pellucid or pure cholestearine calculi.
Second Series. Mixed calculi, with prevalence of cholestearine.
Third Series. Calculi, with prevalence of cholochrome.
Fourth Series. Calculi, with prevalence of modified cholochrome.
Fifth Series. Gall-stones, with prevalence of bile acids.
Sixth Series. Gall-stones, with prevalence of fatty acids.
Seventh Series. Gall-stones, with prevalence of carbonate of lime.

Of the first series the author has but one example to adduce, and undoubtedly such calculi are very rare. Those of the second series contain from 80 to 90 per cent. of cholestearine, the other ingredients consisting chiefly of inorganic salts. Of the third series he quotes two analyses, and of each of the others several.

The chapter on the chemistry of gall-stones is supplemented by a section on the composition of the gall-stones of animals, particularly of those of the ox.

In the very excellent chapter on "The Pathology and Treatment of Gall-stone Disease," Dr. Thudichum reviews the records and opinions bearing on the relative age of persons suffering from those morbid products; on the influence of sex, of hereditary predisposition, of obesity, and of sedentary habits, &c., in their production. He cites several recorded examples of the presence of biliary calculi in children; but on the whole matter of the relative proclivity of various ages beyond that of childhood, which enjoys a comparative immunity from them, he does not find data for general conclusions. As a general but not certain inference from cases on record, women appear to be rather more liable to the malady than men. Hereditary predisposition and obesity are ignored as actual causes. On the question of the alleged effects of sedentary habits, these have in his opinion not been proved to favour their production. "So far as my own experience goes, active habits, on the one hand, do give no protection from gall-stones, if they are not joined with moderate habits of living; and sedentary habits, if not accompanied by excesses in eating and drinking, do not by any means predispose to gall-stones."

The symptomatology of gall-stones, whether in the ducts, gall-bladder, or intestinal canal, is simply illustrated by cases, and there is a good *résumé* of published instances of biliary fistulæ, collected from the catalogues of museums, and from a multitude of writers, ancient and modern.

In the treatment of the passage of gall-stones, the author uses chloroform or ether inhalations as giving much more speedy relief than can morphia or opium, which moreover require to be administered with great caution, on account of the danger of sudden narcotism. Besides, chloroform or sulphuric ether can also be given by the mouth in doses of twenty minims upon a piece of sugar, and washed down with water. This dose may be repeated every twenty minutes, unless sickness is provoked, when the medicine must be omitted. Vomiting at the outset, however, to remove the contents of the stomach, is salutary, though it must be counteracted when it passes into painful retching and threatens to exhaust. Blood-letting, which is recommended by Dr. Stokes and by Frerichs under certain circumstances, is, in Dr. Thudichum's opinion, always unnecessary. A contraction of the duct is needed for the expulsion of the calculus; to arrest it by bleeding is to endanger its impaction and permanent jaundice. "In gall-stone colic we have the same indications as in parturition with excessive pain—namely, to mitigate the pain without stopping the expulsive contractions."

The author rejects the formerly much-extolled virtues of turpentine as a solvent of biliary calculi whilst in the gall-bladder or ducts, and he cannot agree with Frerichs in the reputed effects of a thin bile in lessening such calculi; but "as we can effect (he proceeds to say) the secretion of a somewhat alkaline bile without interfering with the function of the liver or the efficiency of the bile, it is reasonable to make further inquiries and experiments in this direction." With this view he recommends the employment of phosphate of soda, which is normally present in bile.

This chapter on treatment has appended to it a number of illustrative cases, with the record of which the treatise terminates.

Monographs on particular lesions will always be valued by the profession when they convey the results of original observation and experiment; among such productions we are happy to number this treatise by Dr. Thudichum, and to recommend it to careful study.

ART. III.—*Outlines of a New Theory of Muscular Action.* By the Rev. SAMUEL HAUGHTON, M.D., F.R.S.—*Dublin*, 1863. pp. 23.

THOSE who have followed the progress of physiological science during the last few years cannot but be aware of the valuable contributions which the Rev. Dr. Haughton has made to our knowledge of the relations which exist between the waste of the body, as indicated by its excretions, and the work performed by it in the discharge of those various physical and mental functions which go to make up the sum total of human life. This little *brochure*, which is the substance of a dissertation read by its author for the degree of Doctor of Medicine before the University of Dublin, is not so much an extension of his previous researches as a special application of them to one particular form of muscular action. We must, however, before giving a brief outline of its contents, remark, *in limine*, that the title of the work does not appear to be a particularly appropriate one. So far as we can discover from a careful perusal of its pages, they contain purely a record of facts, without any suggestion of a theory in explanation. It is possible that Dr. Haughton may have a theory on the subject in view, but there are but very slight indications of it in his present publication. We mention this not with the view of detracting at all from the merit of his researches, but to prevent our readers being misled, as we ourselves were, into expecting to find in the work what it really does not contain.

Dr. Haughton divides his subject into three parts, the first of which is devoted to an investigation of the rate of muscular action. He commences by referring to the observations made by Dr. Wollaston and recorded in the Croonian Lecture for the year 1809, on the frequency of the susurrus, or peculiar series of sounds which are produced by the muscles when in a state of contraction, and which is familiar to all who have ever put the tip of one finger into the ear, and at the same time brought the muscles of the obstructing hand or forearm into strong contraction. From an ingenious device which he employed Wollaston was led to conclude that the muscular contractions which contribute to the production of this sound occur with a frequency that varies from fourteen to thirty-six in a second—their average number being between twenty and thirty. In the course of some observations which Dr. Haughton made on the same phenomena, both in his own person and in those of some friends, he noticed that the peculiar tone of the muscular sound corresponded with the notes known to musicians as C C C, or D D D—that is to say, two octaves below bass C and D; and these notes indicate thirty-two and thirty-six vibrations per second respectively. This is a curious corroboration of Wollaston's conclusion, and is more particularly interesting from the fact that, although in the experiment above mentioned the susurrus is demonstrated by means of voluntary muscular contractions, it may be shown to be altogether independent of voluntary action. Dr. Haughton even goes so far as to assert his belief of its being independent of muscular action of any kind, and of its being a sign of the rate at which nervous action takes place in the brain. But of this he gives no proof whatever, though he hopes at some future time to do so. We may also mention another corroboration of the rate of the susurrus which is derived from its exact similarity to the noise of cab-wheels as it may be heard any night in Hyde-park, which Dr. Haughton calculates is caused by 35·2 impulses per second.

The second portion of Dr. Haughton's work is devoted to an investigation of the amount of work which is stored up in human muscles. For this purpose he selected the force by which the arm is kept extended in a horizontal position, as one of the simplest cases of muscular action, that position being maintained by the action of two muscles only—viz., the supra-spinatus and the central portion of the deltoïd. From a series of simple mechanical data he infers that the amount of force exerted, in his own case, during an interval of seven minutes, at the end of which period the muscles became thoroughly exhausted from fatigue, was equivalent to that expended in raising 1083 pounds, or nearly half a ton, one foot in the air. On dissecting and carefully weighing these muscles in a well-developed male subject, he found that their weight exceeded slightly five ounces; whence it follows that one pound weight of the same kind of muscle is capable of raising 1·56 ton through one foot before it becomes exhausted. This result will probably considerably astonish those who are unacquainted with Dr. Haughton's previous researches on the amount of force expended by the human body* in the discharge of its several functions, but the considerations upon which it is founded do not appear to be open to any serious objection. It is somewhat singular that women and boys seem, from Dr. Haughton's experiments, to be capable of sustaining the continuous action of the above muscles, before becoming exhausted, for a much longer period than men can.

In the third and most interesting part of his thesis the author discusses the amount of work done in a day by the human heart. Taking the mean of the weights of the heart, as given by various authorities, at 9·39 ounces, it follows, by a simple calculation on the data just referred to, that the work done by the heart in the twenty-four hours is equivalent to raising the enormous weight of 178·09 tons one foot, or more than one-third of the daily labouring force of the whole body, which is estimated as well by Coulomb and Lamandé, as by Dr. Haughton himself, at less than 400 tons. No fact can be more suggestive than this of feelings of astonishment and admiration at an arrangement by which a small organ like the heart is thus capable of wearying out nearly 200 times its own weight of the strongest muscles of the body without itself becoming exhausted; and no fact can be better calculated to demonstrate the vast energy which is stored up in organized structures.

It can hardly be surprising that even Dr. Haughton himself should have felt that so startling a conclusion as this required some corroboration from other and independent sources, and it is no small proof of the originality and ingenuity which have distinguished all of this gentleman's researches, that his profound mathematical acquirements should have enabled him to find in another and totally different set of considerations a most efficient check upon the accuracy of his prior conclusions. Dr. Hales long ago examined, with some care, the force with which a column of blood was projected from a large artery, such as the carotid or crural, in the horse. His experiments, which are detailed in his well known 'Statical Essays,' showed that the maximum elevation of such a column was, at the commencement of the experiment, nine feet, but that it gradually declined to two and a half feet, as the animal became exhausted, and died from loss of blood. These heights denote the hydrostatic pressure exerted by the left ventricle under varying conditions of fulness of the vessels; and as each cubic inch of blood weighs 268 grains, the pressure exerted to maintain the higher of the two columns is equivalent to a pressure of 4·2 pounds on each square inch of the left ventricle. By a most ingenious application of a happy observation made on a jet of blood escaping from the external epigastric artery of a patient in the operating theatre, Dr. Haughton is induced to believe that the pressure exerted by the left ventricle of the human heart is very nearly equal to that which Hales established for the heart of the horse; and by a further extension of the mathematical portion of the problem, he estimates from these data that the work of the left ventricle in the twenty-four hours would amount to eighty-eight tons raised one foot. On adding to this sum $\frac{5}{13}$ths of its amount for the work done by the right ventricle, the thickness of which bears to that of the left the proportion of 5 to 13, we get a total of 121·82 tons for the whole daily work of the heart. The very close coincidence of this number with the one obtained by Dr. Haughton by calculations founded on his previous observations—viz., 124·6 tons, gives at least a very strong probability in favour of the accuracy of his estimate.

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* Dublin Quarterly Journal of Medicine, 1859–60.

We take this treatise to be one of the most valuable contributions which has been made to the science of exact physiology for many years. No one can read it without seeing that its author is not only possessed of such an acquaintance with the mathematical element of physiology as to give him great facilities in dealing with some of its more recondite problems, but also that he is a man of no ordinary originality and power of observation. We shall look with the greatest interest to the further development of his researches, and we hail with pleasure the accession to the ranks of our profession of a gentleman who promises to do such good service in its advancement.

ART. IV.—1. *Mentone in its Medical Aspect: being Letters addressed to a Medical Friend.* By JAMES LEWIS SIORDET, M.B. Lond.—*London,* 1863. pp. 111.

2. *Menton; Essai Climatologique sur ses differentes regions.* Par le Dr. JAQUES FRANCOIS FARINA.

Essay on the Climate of Mentone. By Dr. J. F. FARINA.

THANKS to the efforts of Dr. Bennet, one of the first pioneers in the cause, and of the other medical valetudinarians whom a too zealous devotion to their profession has driven to follow him in his rambles in search of health, the merits of the little village that nestles in the sheltered bay which is bounded by Capes St. Martin and Murtola, and which has, within the space of thirteen years, belonged successively to the Prince of Monaco, the Kingdom of Sardinia, and the Empire of France, are by this time pretty well established. One would have thought that enough had now been written to afford the public the most ample information on all points connected with its situation and climate upon which they could possibly have desired to be enlightened, had not long experience taught us that such subjects as this possess an irresistible fascination for a certain class of authors, and that of "making books" on them there is literally "no end."

Here are two very creditable little *brochures* on Mentone in its medical aspect, each of which is so like the other in its general character that, apart from the peculiarities of style by which every book is distinguished, and from the somewhat different "stand-point" from which a casual English medical visitor and a French physician practising in the town, may be supposed to view certain questions, either might have been written by the author of the other. The object of Dr. Farina, whose name by the way is pleasantly suggestive of that odour which is nominally supposed to be manufactured in Cologne, but of which no small quantity "hails" from the less classic shores of Jersey, is plainly stated in the preface to his work, which, with a judicious foresight, he dedicates to "Monsieur le Préfet," to be "to plead the cause of a district which recognises in the question of climatology the elements of its future prosperity." We have hardly any right to be surprised if, under these circumstances, the hue which tinges our author's description partakes somewhat of the colour of the rose. If the future prosperity of Mentone is to depend upon the reputation of its climate, it is clearly the duty of every patriotic Mentonian at least to say nothing which can discredit its reputation, even if he does not feel it to be his implied duty to give its defects the thickest coat of whitewash. We have, therefore, reason to feel satisfied with the candour which admits that it is not in every case of phthisis that the climate of Mentone is calculated to be beneficial, and which points out to its inhabitants the necessity of backing up the flood of good fortune which has so unexpectedly set in upon their shores by the construction of appropriate residences for the increasing influx of strangers, the laying out of new roads, and a regard for various improvements, sanitary and otherwise, which the rapid enlargement of the town renders indispensable. Although Dr. Farina's work is written mainly, as he himself implies, with the view of awakening the attention of his fellow-citizens to the importance of these considerations, it may be read with advantage by those who, in going to Mentone for the recovery of their health, may be desirous of a concise account of its principal characteristics from a writer whose professional avocations and lengthened residence may be supposed to give to his statements the authority of experience.

Dr. Siordet addresses himself more directly to the wants of the invalid, and his work, though less pretentious in size than those of some of his predecessors, is essentially practical in its aim. He describes with a clearness which makes his account well fitted for the perusal of the non-professional reader, the climate, locality, and resources of Mentone, pointing out with impartiality its weak points, and recommending those precautions which his own experience as an invalid has suggested as desirable. The intending traveller to Mentone who wants a readable, and at the same time a concise and trustworthy guide-book, may safely consult Dr. Siordet's work, which is furnished, as is also that of Dr. Farina, with copious tables of meteorological observations.

ART. V.—*The Natural Laws of Husbandry.* By JUSTUS VON LIEBIG. Edited by JOHN BLYTH, M.D., Professor of Chemistry in Queen's College, Cork.—*London*, 1863. pp. 416.

THOUGH it is foreign to the design of our Review to notice works on agriculture, we are for once tempted to make an exception in favour of Baron Liebig, to whom we are so deeply indebted for the chemical science which he has brought to bear on the most important of the arts—which agriculture surely is—for the instruction he gives to the farmer, and also for his warnings to the statesman and the landed proprietor.

Though few members of the medical profession engage in agriculture, yet most of them have, or should have, as physiologists, a knowledge of its principles; and as they are daily in habits of intercourse with farmers, they have ample opportunities of calling attention to bad practices and to modern improvements in husbandry.

It is not our intention now to review this admirable work of Baron Liebig, but chiefly to impress on our readers one of the great defects of agriculture—as it is generally conducted in Europe—that of the exhaustion-practice; that of taking from the land in over-cropping more than is returned to it, from undermanuring, as if the earth, *per se*, were inexhaustible, and tillage could be made *in perpetuo* a substitute for manure.

Liebig throughout his work raises his voice against this mal-practice, and powerfully descants on its impoverishing evils. The impoverishment he deprecates is the loss of the fertilizing elements belonging to the soil essential to vegetation, varying according to the nature of the crop, and not the gaseous elements derived from the atmosphere. If the former are not returned in the same proportion as they are extracted, he demonstrates that the land eventually must become barren. Happily he is able to illustrate his views on this subject by an account which is given in the appendix to his book of the Japanese system of husbandry, extracted from a Report to the Minister of Agriculture in Berlin, by Dr. H. Maron, Member of the Prussian East Asiatic Expedition. That system is singularly contrasted with the modern European system, and in accordance with the Baron's mature scientific views. The Japanese farmer is described as master of his land, mastering it by a drill method of husbandry, by the manuring of every crop, and by the careful preservation of human excrement. Fallows are unknown to him. He imports no foreign manure; he stands in need of no foreign supplies of grain. The land is made self-supporting; and yet the country has a population exceeding proportionately that of Great Britain and Ireland. And why is this? It is because the income and expenditure of the soil are always kept evenly balanced, the farmer carefully avoiding the impairing the productive power of the virgin land, never breaking up a plot unless he possesses a stock of manure which he may invest in the ground.

All the details—and they are minutely given—of this excellent agriculture, by Dr. Maron, are deserving of being read and re-read, and of being treasured up in the memory of the farmer. We cannot but feel surprised in perusing them that the art of husbandry should have been brought to such a pitch of excellence without the aid of chemical science, merely by the common sense of an acute people taught by experience.

We shall give one passage—that descriptive of the Japanese privies, on which their husbandry mainly depends for manure, and the prevention of that exhaustion which threatens our fields.

"The Japanese does not construct his privy as we do in Germany in some remote corner of the yard with half open rear giving free admission to wind and rain, but he makes it an essential part of the interior of his dwelling. As he ignores altogether the notion of a 'seat,' the cabinet, which, as a general rule, is very clean, neat, and in many cases nicely papered, or painted and varnished, has a simple hole of the shape of an oblong square, running across and opposite to the entrance door, and serving to convey the excrement into the lower space. Squatting over this hole, with his legs astride, the Japanese satisfies the call of nature with the greatest cleanliness. I never saw a dirty cabinet in Japan, even in the dwelling of the very poorest peasant. We in Germany construct privies over dung-holes, and behind our barns, for the use of our farm-servants and labourers, and provide them with seats with round holes. With even only one aperture, it is too often found that after a few days' use they look more like pig-styes than closets for the use of man, and this simply because our labourers have a decided, perhaps natural, predilection for squatting. The construction of the Japanese privies shows how easy it would be to satisfy this predilection.*

"To receive the excrements, there is placed below the square hole a bucket or tub, of a size corresponding to it, with projecting ears, through which a pole can be passed to carry the vessel. In many instances a large earthen pot, with handles, is used, for the manufacture of which the Japanese clay supplies an excellent material. In some rare instances, in the town, I found a layer of chopped straw or chaff at the bottom of the vessel. As soon as the vessel is full, it is taken out and emptied into one of the larger dung-vessels. These are placed either in the yard or in the field. They are large casks or enormous stoneware jars, in capacity from eight to twelve cubic feet, let into the ground nearly to the brim. It is in these vessels that the manure is prepared for the field. The excrements are diluted with water, *no other addition of any kind being made to them*, and stirred until the entire mass is worked into a most intimately intermixed pap."

Further particulars are given respecting the fermentation of the manure: it is especially mentioned that under no circumstances is it ever used in the fresh state, leaving the ammonia *exposed to decomposition by the action of the sun and its volatilization by the wind, but taking the greater care to shield the solid ingredients from being washed or swept away by rain, &c.*

Now, could this valuable manure be protected from waste, applied to our fields instead of being allowed, owing to our fastidious water-closet plan, to flow into our streams and pollute their water, how vast would be the gain; millions would be saved at present expended on the importation of guano and bones, and that exhaustion of the soil which is threatened, and is, without change of system, as certain as the final exhaustion of our coal-fields, would be arrested. As has been well observed by the father of agricultural chemistry, Sir H. Davy, when treating of this kind of manure, that "that which would offend the senses and injure the health if exposed, is converted by gradual processes into forms of beauty and usefulness; the fetid gas is rendered a constituent of the aroma of the flower, and what might be a poison becomes nourishment to animals and man."

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* That it is a predilection, we may remark, can hardly be doubted; in Turkey and Asia Minor the privies are of the same kind as in Japan, though not with a view to agricultural profit, and also in the Eastern world generally.

ART. VI.—*A Survey of Human Progress, from the Savage State to the Highest Civilization yet attained.* By NEIL ARNOTT, M.D., F.R.S., &c.—*London*, 1861. pp. 200.

IT is very gratifying to see a man of advanced age, who has withdrawn from the laborious exercise of his profession, exerting himself to benefit his fellow-men by giving them his accumulated experience, and especially on a subject of so much importance as that of the education of the people. In the work before us, which, as its author observes, might as fitly be named 'A Treatise of Education' as the title it bears, Dr. Arnott affords an example which we cannot too much admire of this most praiseworthy labour. It is gratifying too to witness his faith in the advancement of society, as expressed in his title-page, that it is "as little perceived by the multitude in any age, as is the slow growing of a tree by the children who play under its shade—but which is leading to a new condition of mankind on earth."

By taking a survey of human progress from the earliest historical period to the present time, and of the different races of men now existing, the living representatives of the several stages of social man, he shows in a very striking and convincing way how much depends on education, and that man, according to his training and the degree of acquired information, the result of education which he possesses, is either the ignorant and brutal savage, the *homo ferus;* or powerful in knowledge and refined in manner, the *homo sapiens.*

Though, as Dr. Arnott remarks, the art of medicine is of the greatest amplitude, in its full range comprehending what regards mind as well as body, and resting on the four departments of human knowledge—physics and chemistry, life and mind—yet we need hardly say that his great subject is scarcely suitable to other than a brief notice in a purely medical journal such as is our Review. Elsewhere, as in one or other of our literary Quarterlies, we hope it will have the attention that is its due, and equally so whether we consider the nature of the author's undertaking, or the ability, the originality, and the sobriety which he has displayed in the treatment of it.

His main intent is to show that the whole system of education, such as till recently was persisted in at our English universities—unchanged from a remote period—in which the instruction was chiefly confined to the dead languages and to the mathematics, and for proficiency in which, and in these alone, all their honours and prizes were bestowed, is altogether inadequate to meet the wants of the present advanced stage of society. What he advocates is, an enlarged liberal scheme, one commensurate as much as possible with existing requirements. He holds that our own language is more deserving of careful study than that of the Greeks and Romans; not that he would have

Greek or Latin altogether neglected, but when taught, taught as Milton recommended in his 'Tractate of Education,' so as to be of easy and pleasant acquirement. He holds that the four fundamental sciences, the mechanical, the chemical, vital, and mental, should be early and methodically taught, and that no curriculum of liberal study is complete without them. He enforces his arguments very happily and often eloquently by various and appropriate illustrations. We wish that our limits allowed of our giving some examples of these, as many of them, both in the text and in the additional notes, are most admirable.

The fitness of the author for the task he has undertaken is well shown by what he relates of himself, and the manner he acquired his experience; this, we may remark, is a concise autobiography. He introduces the sketch, when pointing out the evils of denominational schools, as fitted to render permanent, according to his opinion, religious differences, and so perpetuate numerous errors, and indicating the advantages of an opposite scheme of secular education in mixed schools. We trust our readers will thank us for quoting the passage, *in extenso,* relating as it does to so distinguished a man. He says:

"The writer of this has a strong conviction of the importance of such mixed schools from his own experience, during his education in Scotland early in this century. The disruption of the Established Church there had not then taken place, and there was no activity of sectarian strife. Whatever the strict rule of law might be, the grammar school (at which Lord Byron happened then to be a pupil) and the University of Aberdeen practically received students of all classes, without reference to the creeds of their parents. The consequence was, that many congenial minds were drawn together, and warm friendships were formed which remained afterwards unbroken, notwithstanding differences of religious denomination. Among the students were members of the Presbyterian Church of Scotland, of the Church of England, and of the Roman Catholic Church. The writer had dear friends and relatives in all of these. Under these circumstances, ordinary sectarian animosities could not arise, and permanent benign effects in character were produced on all sides. When the writer afterwards completed his professional studies in London, and had been appointed chief medical officer of an East India Company's ship, which on that occasion had to convey troops to India, the accidents of a protracted and peculiarly eventful voyage carried him from Europe to parts in the other three quarters of the world, as Brazil, Cape of Good Hope, and various stations in Asia, including China. He had thus experience, still larger than before, of differences of religious training, bringing under notice, besides denominations of Christians, also Hindoos, Buddhists, and Mahometans; and owing to considerable detention in various places, he had the opportunity of studying the effects in character. It was interesting to observe among all classes the earnestness of individuals in their different creeds, who would doubtless have made great sacrifices to perform what they had been taught to deem their duty to their Maker. On his return to London, to reside permanently, he had yet further experience of the same kind, when his appointment as physician to the French and Spanish Embassies gave him opportunities of knowing intimately men of superior intellect, who had been otherwise trained than people are in England. Had he been asked to state reasons why, if placed from infancy exactly as some of these had been, he would have believed and acted otherwise than as they were doing, he must have replied that he could not. With such education and experience of the world as fell to his lot, the writer thinks it impossible that an ordinary mind could have conceived sectarian hatred against any person honestly following the lessons taught by the parents whom it had pleased Providence to give them as their guardians and instructors. The national schools in Ireland commence such an education; and the effects of the increasing freedom of commerce over the world, aided by railways, steam navigation, and the electric telegraph, by opening all nations to friendly intercourse with one another, must render more efficacious the Divine precepts, 'Love one another,' and 'Do to others as you would be done unto.'"

In connexion with this subject, we would refer to an interesting paper by Professor Arnold in a recent number of 'Macmillan's,' on the system of public education as at present conducted in France.

ART. VII.—*Anatomy of the Parts concerned in Femoral Rupture.* By GEORGE W. CALLENDER, Assistant Surgeon to, and Demonstrator of Anatomy at, St. Bartholomew's Hospital.—*London*, 1863. pp. 51.

MR. CALLENDER'S little work has a double object; in the first place, and chiefly, to give an accurate description of the parts which are concerned in femoral hernia, as they exist in nature, irrespective of the descriptions which have been handed down to us; and secondly, to recall to the recollection of anatomists and surgeons what there may be deserving of attention in the writings of the older authors on this subject—writings which the vast increase, and we hope we may be allowed to say, the preponderating merit, of modern works have well nigh buried in oblivion. In both of his objects Mr. Callender may be said to have worked successfully, but the former is to our minds far more important than the latter. After perusing the quotations which Mr. Callender's industry and love of such researches have enabled him to collect from a host of ancient writers on anatomy, we must own to having found the greater number excessively obscure and crabbed; and as we presume that the passages quoted would be found to be at any rate average specimens of the style and matter of the authors, we are well content to remain in humble ignorance of the rest. Some of the passages quoted, however, are interesting: such as the elaborate and distinct directions published by Franco in 1560 for performing the operation for hernia without opening the sac—an operation usually attributed to Petit. The passage (note on p. 49) is too long to quote; but it will be sufficient to say that this author describes the operation as distinctly as it could be described in a modern book of surgery, except that he calls the parts by obsolete names; and it is difficult to suppose that he could have accurately laid down all the steps of so peculiar an operation unless he had practised it. A few others of Mr. Callender's gleanings from old books are interesting, and tend to raise our estimate of our predecessors; but the real importance of the work consists in its being a quiet and faithful protest against the practice, which has too long prevailed in our schools, of describing the parts of the body *anatomically*, that is to say as they can be made to appear by dissecting, or cutting them to pieces, instead of describing them *physiologically*—that is to say, as they really exist during life, and according to the uses and offices which they are intended to serve. No part of the body affords a more striking instance of this vicious custom than that which Mr. Callender has undertaken to describe. The little portion of fibrous tissue which, intervening between the superficial fascia of the thigh on the outside of the body and the peritoneum in the interior, and bounded towards the middle line by the pubic bone, and on the other side by the femoral vein, has been divided by the ingenuity of anatomists into about a dozen separate tissues, and has given immortality to at least half a dozen separate authors; its detailed description occupies the best part of forty-five quarto pages in the elaborate work of Mr. Gay; while the number of different objects which the student was apparently expected to recognise, and the list of names which he was expected to remember and to be able on occasion to define, threatened to lengthen with each successive author. Meanwhile it was certainly disheartening to the zealous youthful anatomist, after that he had by some triumphant expenditure of minute toil succeeded in demonstrating to his own satisfaction and that of his teachers the crural septum and the deep crural arch, the saphenous opening, the cribriform fascia, Hey's ligament, Burns's ligament, the crural opening, and the crural canal (the last and most difficult triumph of the dissector's art), to be told that few of these objects have any real existence in nature, and that a man might have operated successfully upon a hundred cases of femoral hernia without having ever heard of any of them; in fact, that they are portions into which the tissues may be divided, rather than divisions which nature has pointed out; and that they are described in our schools rather from respect to the anatomists who first demonstrated them, and to the examiners who still exact a knowledge of them from the students, than from any conviction on the part of the lecturer of their existence in nature. Each of these celebrated names comes out from the investigation of Mr. Callender shorn of much of its importance. Thus he says of the septum crurale, "If the sub-peritoneal fascia be reflected from the inner side of the lower extremity of the external iliac vein, or if the fascia lata be removed from the inner side of the crural vessels (here surrounded by dense reticulated tissue), a small collection of fat, held together by connective tissue, is exposed to view." (p. 38.) And in a foot-note to this passage, he says, "Cloquet has described some fascia, perforated by openings for the lymphatics, as extending between the vein and the posterior edge of the external oblique tendon (septum crurale)." So it is plain that Mr. Callender has not been able to satisfy himself of the real existence of the structure described by Cloquet. As to the saphenous opening and the cribriform fascia which closes it, Mr. Callender, correctly as we think, regards the opening as in most cases an artificial formation, and the fascia by which it is covered as a part of the fascia lata, and he gives a drawing (Plate ii.) which represents to our minds most accurately the state of the parts, and which shows the fascia lata extending uninterruptedly over the femoral vein and the opening for the saphenous vein. Mr. Callender does not however deny that a "saphenous opening" is really present in some cases; but this he regards as the result of "the wasting or absorption of portions of the fascia lata, probably from the pressure of enlarged glands." Touching the deep crural arch, and the ligaments (or ligament) of Hey and of Burns, Mr. Callender makes the following very just and very sensible observations:

> "The whole upper free edge of the iliac fascia lata is commonly called 'the falciform process,' whilst its deeper fibres receive the name of 'Burns's ligament.' Hey's femoral ligament would appear to consist of distinct fibres connected with the inner fold of the iliac fascia, which extend immediately beneath the tendon of the external oblique to the sub-peritoneal fascia. The upper border (cornu superius annuli cruralis anterioris, Hesselbach) of this opening thus receives, by an unfortunate complication, the names of 'Falciform process,' 'Femoral ligament,' 'Burns's or Hey's ligament.' . . . . The various divisions of the iliac fascia lata depend in great measure upon the skill of the dissector, and are, in my opinion, artificial." (Note, p. 19.)

The crural opening and the crural canal are made, as Mr. Callender remarks, by the femoral hernia, and have no existence before that tumour protrudes. In fact, it has appeared to us that the simplest, the most natural, and, above all, the most surgical way of looking at a femoral hernia would be to describe it as a mass of protruded omentum or intestine covered by two sacs lying under the subcutaneous tissue—the inner hernial or serous sac formed by peritoneum, and having usually a distinct neck, the outer or tendinous sac formed by fascia lata, or, which is the same thing, by the sheath of the vessels, and having its neck at the crural opening. The constriction of a strangulated hernia takes place either at the neck of the tendinous sac or at the neck of the serous sac. In the former case an incision into the tendon of the external oblique (Gimbernat's ligament) or into the neighbouring attached portion of the iliac fascia lata (Hey's ligament, Burns's ligament, deep crural arch), will relieve it; and if the tendinous sac do not adhere too closely to the serous at this part, it may be possible to relieve the stricture without opening the serous sac. If, on the contrary, the constriction is at the neck of the serous sac, the latter must necessarily be opened, and in dividing it some of the fibres of the neck of the tendinous sac will also usually be divided, when, as is generally the case, these two necks correspond. We may remark, however, that the necks do not always correspond, and that the seat of stricture in the neck of the peritoneal sac may be at any part of its circumference. In operating a short time since on a case of inguinal hernia we found it impossible to return the intestine until a tense band at the back of the sac had been divided. If this simple view of the ground plan of a hernia were more generally taught to students, we venture to think that their comprehension both of the disease and of its operative treatment would be much facilitated. At present, all the ingenuity of anatomists seems to have been expended upon the task of chopping up the tendinous investment of the hernia into as many distinct parts as possible, and describing each by names which convey little meaning to the mind of the student, while the outer or subcutaneous investment of the tumour is passed over with hardly any notice, and the inner or peritoneal one is little dwelt upon. This is surely an error. In the *anatomy* of hernia the subcutaneous tissue, including the glands, plays a most important part; a part which we are glad to see that Mr. Callender restores to its due prominence (see Plate iii. and the comments on it). In the *pathology* of hernia, surely the contents, and serous covering, of the tumour are the essential parts to which attention should be directed. While in the *operation* for hernia the main element of success and safety is that the operator should be able to recognise the serous sac, so as to avoid mistaking for it the tendinous sac (as is constantly done), or the serous coat of the intestine, as has happened to very experienced operators. The dissecting-room refinements which occupy so much space in our books are little thought of in our operating theatres, and ought surely to be less pressed upon the attention of students than is now the fashion. In making these remarks nothing is farther from our intention than to depreciate the labours of the celebrated surgeons and anatomists who have promulgated these complicated descriptions; nor, we are sure, can so eminent a pupil of the school of Pott and Lawrence, as Mr. Callender is, intend to speak lightly of the labours of his distinguished predecessors. The minute anatomical examination which the regions of hernia have received is no doubt the foundation of the more rational and successful treatment of the disease in modern times. We would only reduce such anatomical niceties to their just value, and deprecate such an exclusive attention to them as would (nay, we think we may say does, in the case of too many students) draw the mind away from the consideration of the pathology of the disease. It is in this respect chiefly that we think Mr. Callender's little work of value: and regarded in that light its value is by no means to be measured by its unpretending size and appearance. We trust it may be only the first of a series of contributions to surgical anatomy from the same hand; and that Mr. Callender will do good service to the students who are fortunate enough to learn from him, in drawing their attention rather to the great principles of surgical disease, and to the mechanism which favours their production or their cure, than to the enumeration of little scraps of obscure tissue, about the correct description, or even the separate existence of which no two anatomists seem able to agree.

ART. VIII.—*Ulcus Corrosivum Duodeni.* En kasuistick Sammenstilling. Af Dr. F. TRIER. (Særskilt Aftryk af 'Ugeskrift for Læger,' 2^{den} Række XXXVIII. Nos. 20-24.)—*Kjöbenhavn*, 1863. 8vo, pp. 79.
Corrosive Ulcer of the Duodenum. A Collation of Cases. By Dr. F. TRIER. (Reprinted from the 'Ugeskrift for Læger.')—*Copenhagen.*

THE fact that the so-called corrosive (simple, round, spontaneous, perforating) ulcer occurs only in the superior transverse portion of the duodenum, and very rarely in the lower part of the œsophagus, is an indication which may possibly lead us in the right direction towards discovering the true origin and mode of development of the disease;

and these are precisely the points which, notwithstanding all theories, are in the present day most obscure, not to mention that the different situations and relations to neighbouring organs of the duodenum and stomach produce corresponding differences in the features of the disease, according as it is developed in the one or the other part.

The author proceeds in his observations to show in what respects the superior transverse portion of the duodenum may be considered as a transition from stomach to intestine. As bearing upon the origin of the ulcers in question, the fact that the contents of this portion of the intestinal tract have still in general an acid reaction, inasmuch as the liver and pancreas have not yet poured in their alkaline secretions, is probably of still greater importance than the structure of its mucous membrane. Thus, instead of the earlier, more or less defective, explanations of the origin of the gastric ulcers (Cruveilhier considered them, as is well known, as proceeding from a follicular inflammation, Rokitansky as a further development of the so-called hæmorrhagic erosions), Virchow has lately propounded a view which has met with very general acceptance. This writer,* in fact, lays great stress upon the corrosive nature of the acid contents of the stomach, but he sees in the defined form of the ulcer a strong indication that its first origin must be purely local, while the corrosive action of the acid is the most important element in its further progress. Virchow assumes that the first and purely local affection proceeds from interruptions or from essential disturbances in the circulation of the part, and supposes from what he has seen, that these irregularities may most frequently be referred to morbid conditions in the vessels of the stomach, and to a hæmorrhagic necrosis proceeding therefrom. It is chiefly the arteries of the stomach, to whose morbid states or obstructions he attaches great importance in this respect, while he does not deny that irregularities in the portal circulation, and dilatations of the vessels of the stomach thence proceeding, or acute and chronic catarrhs, especially when they are accompanied by violent vomiting and violent spasmodic contractions of the stomach, may give rise to lesions of nutrition in limited points, rendering the tissue accessible to the action of the gastric juice. This view finds support in various observations—among others, in those of Rokitansky,† according to whom, as the first commencement of the ulcer, "we find the mucous membrane in a circumscribed, round, elliptical part, changed to a pulpy and dusky, or to a solid yellow (diphtheritic) crust, after the removal of which, the submucous connective tissue lies exposed within the limits of a sharply cut off margin of the mucous membrane, or of a fringe of that which is changed to a yellow crust."‡ In the next place it is not unimportant to observe that the favourite seat of the ulcers is the region adjoining the curvatures of the stomach, precisely where the larger arteries send in their branches directly under the mucous membrane. With this agrees also the usual symmetrical occurrence of ulcers lying directly opposite to one another on the anterior and posterior surface of the stomach (more rarely of the duodenum), which arrangement is best explained by the fact that the vessels, after entering the curvatures, send symmetrically situated branches to both walls. When, therefore, the disturbance of the circulation is conveyed through such a vascular trunk, it will, for example in embolism, often happen that its results show themselves on symmetrically situated parts of the two mucous surfaces turned towards one another. Finally, with reference to the importance which Virchow attaches to the acid nature of the contents of the stomach in explaining the further progress of the ulcers in depth and circumference, the fact that, except in the stomach, the ulcers in question occur only in the lowest part of the œsophagus and in the first part of the duodenum, deserves special attention, for only in these parts can the contents in general be assumed to be acid, while they are alkaline in the whole of the remaining portion of the intestinal canal, whose vascular distribution is arranged in the same mode as that of the stomach, and which ought therefore, so far, as easily to become the seat of such an ulcerative process. The occurrence of the corrosive ulcer in the duodenum consequently forms an important point of support for the theory which is at present received by a great number of pathologists. In the experimental way, too, investigators have often succeeded in demonstrating the importance of irregularities of the circulation in the formation of ulcers of the stomach. L. Müller* and Panum† have (the former in rabbits, the latter in dogs) found extravasations of blood and loss of substance in the mucous membrane of the stomach after tying the vena portæ (Müller), or after artificially produced embolism in the arteries of the stomach (Panum). In the latter case embolism was met with only in the arterial branches which served to nourish the affected portions of the mucous membrane, while everywhere else it was absent. By these experiments it is proved that irregularities in the circulation *may* produce the morbid process here spoken of, without its following as a matter of course that there are no other causes capable of giving rise to the same effect.

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* Virchow's Archiv. 1853, Band v. pp. 362 et seq.

† Lehrbuch der Pathol. Anat., iii. p. 170, 1861.

‡ Rokitansky refers to this class also the ulcers of the stomach first described by Curling (Medico-Chirurgical Transactions, vol. xxv.), and met with still more frequently in the duodenum, which occur after extensive burns, when suppuration has set in, and which are preceded by a formation of crust in the mucous membrane alone,

The author observes, that from the infrequency of the occurrence of ulcer of the duodenum, the material at his disposal, which is derived partly from the writings of others, partly from an examination of the records of dissections kept in the medical division of Frederik's Hospital during a space of about twenty years—viz., from the 1st April,

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or at the same time in the submucous tissue, which crust has the characteristic form and boundaries of the ulcer, gives rise to hæmorrhages, and sometimes very quickly (in the course of a fortnight) to perforation.

* Das corrosive Geschwür im Magen und Darmkanal, Erlangen, 1860, pp. 272 et seq.

† Experim. Beitr. zur Lehre von der Embolie: Virchow's Archiv, 1862, xxv. pp. 488 et seq.

1842, to the 1st January, 1862, is insufficient to furnish any complete statistics of the disease. He therefore confines himself to three points: 1, the frequency of the occurrence of ulcer of the duodenum, compared with that of the stomach; 2, the sex; and 3, the age of the persons attacked.

1. As to the comparative frequency of the two affections, it would appear from the observations of Rokitansky, Jacksch, Müller, and from the records of Frederik's Hospital, that among 261 cases of corrosive ulcer or of cicatrices left by the same, 28, or 10·7 per cent., were found in the duodenum. The author remarks that probably these ulcers would be more frequently discovered if the stomach and duodenum were accurately examined in every post-mortem examination.

2. It is well known that ulcer of the stomach occurs much more frequently in females than in males: Brinton, who has collected the largest number of cases—namely, 654—makes the proportion as two to one, 440 having been met with in females, against 214 occurring in males. On the contrary, the corrosive ulcer of the duodenum is found five times more frequently in males than in females, the total number of cases on record in which the sex is mentioned being 54, of which 45 occurred in males, and only 9 in females. This contrast is the more striking, as these ulcers do not present any demonstrable differences, whether they are found in the stomach or duodenum.

3. Corrosive ulcer of the duodenum occurs most frequently in the age of manhood: about three-fourths of those attacked were between 30 and 60 years of age, and the average age was 42⅓ years.

Having dwelt so long upon the more introductory portion of Dr. Trier's interesting and instructive essay, we shall now, as the course most likely to be useful and acceptable to our readers, instead of entering into the details of the twenty-six cases recorded in the work, quote at some length from the conclusions drawn by the author himself from the facts he has collected:

"In a clinical point of view," he observes, "the cases which present themselves fall into two principal groups, consisting of those which run an acute and those which run a chronic course. But on examination after death, it is found that the former can be called acute only when they are considered from a purely clinical point of view; anatomical examination shows that they almost always have really had a tolerably long duration.

"In the acute cases the disease almost invariably terminates in rapid peritonitis, the result of perforation. The ulcer, under such circumstances, is generally situated on the anterior intestinal wall.

"The chronic cases resolve themselves into two subdivisions: 1. Those where only isolated symptoms of apparently slight importance had preceded the fatal result. 2. Those in which more serious phenomena had long before appeared and might have excited a suspicion of this or of some other dangerous abdominal disease.

"To the first of these subdivisions the same observations in a certain degree apply, which were made with respect to the cases running an acute course.

"The second subdivision affords especial opportunity for observing the results which ulcers in the duodenum of very long standing may induce in connexion with the surrounding organs. The ulcer may, according to its seat, effect adhesions with, and corrosion of the liver, the biliary ducts, and gall-bladder anteriorly; the pancreas, the vena portæ, the hepatic artery, the ductus communis choledochus, and the sub-peritoneal connective tissue posteriorly. It may also give rise to new formations of connective tissue around it, which by pressure, tension, and interlacement, may produce considerable functional disturbances. Among these, thrombosis in the vena portæ and occlusion of the ductus communis choledochus are the most important, which in the living body manifest themselves by symptoms of the hepatic affections produced by them, especially by jaundice. Sometimes the pains which the serous inflammations and their products cause by tension and interlacement give rise to nervous reflex actions of considerable violence (neuralgias, spasms). Besides these effects, ulcer of the duodenum in many cases produces considerable dilatation of the stomach with hypertrophy of its membranes.

"The diagnosis of chronic ulcer of the duodenum is based partly upon the peculiarities it possesses in common with ulcer of the stomach, and partly upon some which belong to it alone. The diagnosis from ulcer of the stomach, and especially from constriction of the pylorus, is, in the present state of science, and of our aids to diagnosis, impossible. As the most important grounds for the diagnosis we must mention: 1. Signs of dilatation of the stomach; 2. A sensitive tumour in the epigastrium, proceeding from adhesion with the pancreas; and 3. Jaundice or other hepatic phenomena. It is especially the two last-named symptoms, which are peculiar to the ulcer of the duodenum, which nevertheless, under such circumstances, may be difficult to distinguish from cancer in the pylorus, and from diseases originally developed in the liver, its vessels, or excretory ducts. In the acute cases, and in those chronic cases which run a latent course, the diagnosis is impossible."

ART. IX.—*Die Spermatorrhœa nach ihren Körperlichen Verhältnissen, ihrer Anatomischen Grundlage und dem Heilverfahren in derselben, in Nerven-, Gemüths-, und Geisteskrankheiten.* Von Dr. J. F. H. ALBERS.—*Bonn,* 1862. pp. 152.

On Spermatorrhœa, its Influence on the Body, &c. By Dr. J. F. H. ALBERS.

THE subject of this treatise has fortunately of late years engaged the attention of several qualified medical men, who have partially rescued it from the domain of a set of infamous charlatans. Nevertheless, it has not sufficiently arrested the attention of the profession at large, and there is both a deficiency of precise and accurate information respecting the exact pathological conditions of Spermatorrhœa and very divergent views entertained as to the gravity of its consequences, particularly in reference to the mental powers. Dr. Albers, one of the professors in the University of Bonn, and well known by various medical works of repute, has undertaken, in the treatise under notice, to portray the varieties of spermatorrhœa in relation with the peculiar pathological conditions associated with them, and particularly to examine the disorder in its effects on the mental functions. He also devotes a section of his treatise to the consideration of treatment—a matter further illustrated by the history of several cases.

He recognises spermatorrhœa as existing under three primary conditions:—1. As simply an abnormal discharge of seminal fluid. 2. As associated with morbid changes in the seminal receptacles and ducts, and in the bulbus urethræ and prostate. 3. As presenting a combination of the two foregoing conditions.

Altogether thirteen cases are recorded, the majority of them extracted from the essay of Lisle,* and accompanied with comments by the author. Although this plan, adopted by Professor Albers, of borrowing cases in illustration, and of comparing his own experience with those selected examples, may possess some advantages, it would in our opinion have been more satisfactory, looking upon his book as an independent treatise and not as a critique upon Lisle's essay, had the author presented us with his own clinical records to exemplify his views respecting each of the varieties of the disorder he takes into consideration.

Many have been unwilling to admit the directly mischievous results of spermatorrhœa on the cerebral functions insisted on so strongly by others, and have argued that the loss of the secretion can have no such dire results, inasmuch as many debauchees, given to great sexual excesses, apparently escape unharmed for a long time, though their seminal discharges are greater. This question is discussed by the author at page 55 and again at page 79; and his opinion is that in cases of spermatorrhœa, especially when present by day as well as night, there is greater exhaustion of the secretion; that it escapes with the urine and during the alvine discharges; that there is a morbid activity in the secreting organs and their ducts, and an abnormal fluid poured out defective in the true spermatic elements. On the other hand, we must remember that masturbation shows its ill consequences in the female almost equally as much as in the male, though no exhaustive discharge be induced; and we therefore entertain the conviction that the consequences to the nervous system and brain do not bear any intrinsic relation to the spermatorrhœa itself, but to the causes of that disorder, and to the nervous phenomena conjoined with them.

The deduction from Lisle's cases is, that involuntary seminal discharges, with irritation of the receptacles and ducts, exercise a very injurious influence upon the brain, and operate as a predisposing cause of insanity; and further, that the insanity so induced has features peculiar to itself whereby the physician may recognise it from other varieties of mental disorder, whilst the increase or decrease of the discharge is attended by a corresponding aggravation or amelioration of the malady.

Professor Albers calls particular attention to the fact that involuntary seminal discharges may occur without antecedent masturbation or attack of gonorrhœa, as illustrated in the third case he records; and further, that they are not always associated with impotence. Moreover, he concludes that both false and true spermatorrhœa depend much more on organic lesions and abnormal excretion of the *bulbus urethræ*, the ejaculatory ducts and the seminal vesicles, than on organic disease of the testicle and inertness of those parts concerned in the act of emission; and, lastly, that spermatorrhœa is commonly accompanied by an abnormally increased secretion of seminal fluid (p. 104).

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* Archives Générales de Médecine, 1860.

He distinguishes three varieties of spermatorrhœa:—1, the traumatic; 2, the onanistic; and 3, the gonorrhœal. The first kind follows upon injuries, such as blows upon the perinæum, as instanced in the tenth recorded case. The second form is attended by greater loss of tone or power in the parts, with wasting and anæmia: the urethra is exceedingly sensitive, and particularly in the vicinity of the bulb; and an involuntary escape of urine is frequently found during sleep. The third variety follows after gonorrhœa, and in it there is considerable thickening of the bulb and parts connected, the ejaculatory ducts and the prostate; the ducts becoming also widened. The stream of urine is reduced in size, and micturition rendered somewhat difficult, and seminal evacuations often occur when urine is passed, or when the bowels are relieved. The seminal fluid is mostly deteriorated; contains mostly only glistening corpuscles similar to the heads of spermatozoa, with or without epithelial *débris.* The testicles and sexual organs are well formed, and the constitution is mostly vigorous. If the submucous tissue has not become so thickened as to produce an actual stricture, *pollutio spuria* is often present; and when the lesion has not farther advanced, productive cohabitation may be regained. On the contrary, when an actual stricture is formed there is no seminal discharge with the stools, and the author doubts whether any true spermatic fluid escapes under these circumstances, even with the urine. Moreover, when a stricture is present, and especially if the stream of urine be broken or divided by it, no effective copulation can occur.

Reviewing spermâtorrhœa in connexion with the parts principally concerned in its production, Professor Albers treats of S. testicularis, S. vesicularis, S. prostatica, and S. urethralis, thus adopting in the main the pathological division of the disorder established by Dr. Marris Wilson in his treatise 'On Diseases of the Vesiculæ Seminales and their Associated Organs.' (London, 1856.)

He nevertheless attaches little practical importance to this division, for he remarks (p. 113) that, from his own observations on the genital organs of those who have for any length of time suffered, especially after gonorrhœa, it is not one part but many parts of the organs concerned in the elimination and discharge of spermatorrhœal fluid which are involved simultaneously. However, he discusses each variety in detail, so that he may examine the operation of the several pathological changes in the development of the symptoms of spermatorrhœa and in the production of its results.

It is satisfactory to be able to report that Professor Albers has not neglected British medical authorities. The views and researches of Marris Wilson are duly recognised, whilst those of Prout, Henry Thompson, and others, are referred to where they have been relevant to the subject under discussion.

The treatment must be regulated by the knowledge of the intrinsic lesions connected with the discharge, and of the patient's general condition and habits. If inflammatory symptoms show themselves around the seminal vesicles and their neighbourhood, leeches should be applied to the perinæum, and ice with cold hip-baths. When there is swelling and stricture, or chronic inflammation in the bulb or at the mouths of the ejaculatory ducts, caustic may be applied, according to the plan of Lallemand, Amussat, and Lisle. Digitalis, or digitalin, may be used internally to lessen the abnormal irritability of the nervous system. When *pollutio diurna* is a consequence of onanism, cauterization is usually very effectual; and for any urethral irritation that may be felt, camphor ointment or liniment rubbed in along the course of the passage is highly beneficial. In such cases likewise the introduction of bougies, prepared with camphor or tannin, proves advantageous, and Albers injects a weak solution of camphor in olive oil by means of a catheter into the sensitive urethra. The Faradization of the genital organs has been proposed by Dr. Clemens, of Frankfort, and Wilson recommends cauterization or counter-irritation to the perinæum, or the formation of an issue by means of potassa fusa.

Albers himself speaks in praise of tannin used by way of injection to produce constriction of the dilated mouths of the ejaculatory ducts and to lessen the abnormal secretion.

Such are the local and medicinal measures applicable in this sad disorder; it is unnecessary to dilate on the dietetic rules and moral treatment which must be enjoined with the former to render them efficacious.

This treatise by Professor Albers will repay perusal, not indeed on account of any originality in its teachings, but as a good description of spermatorrhœa, its symptoms and treatment, and particularly of its relations with mental disorder.

ART. X.—*Études Pratiques sur les Maladies Nerveuses et Mentales, accompagnées de Tableaux Statistiques, &c.* Par Dr. H. GIRARD DE CAILLEUX.—*Paris,* 1863. pp. 234.

Practical Studies on Nervous and Mental Diseases, accompanied by Statistical Tables. By Dr. H. GIRARD DE CAILLEUX.

THIS work is intended to convey the results of twenty years' experience of the author in one of the best asylums of France. M. Girard de Cailleux has earned for himself a high reputation in France, and an equally high one in this country also, having taken the van in the march of improved Asylum construction and management, and shown a more correct appreciation than most of his countrymen of the principles of treatment advocated and carried out in England. His long services as an asylum physician, as the originator and constructor of the far-famed asylum of Auxerre, and as its successful manager, have secured him the position of Inspector-General of Lunatics in the Department of the Seine. The first fruits of his labours in this new office are also contained in the present treatise, in the shape of two Reports to the Prefect of the Seine on the state of the insane treated in the great hospices of Paris, the Salpêtrière and the Bicêtre, and in some "General Considerations on the Entire Service of the Insane in the Department of the Seine." Though others had previously worked in the same direction, yet these reports appear to have finally determined the central authorities seriously to undertake to remedy the most unsatisfactory state of the insane of the metropolitan department of France, by resolving to erect several new asylums in the country, fitted for their proper care and treatment. These reports will consequently be read with much interest, and their appearance will mark an epoch in the history of insanity in France.

The bulk of M. Girard's book is occupied with the statistical history of the Auxerre Asylum during a period of twenty years. He examines and analyses his statistics, and briefly records the deductions he arrives at. Nevertheless, he is careful to note in his Preface that his facts and opinions are based on the experience of only one institution; and that to render them more satisfactory and trustworthy in several particulars, it is necessary to collect similar statistics from other asylums in order to institute a comparison, and thus, by a knowledge of peculiarities affecting the people in the several districts, their occupations and modes of life, together with the meteorological and geological conditions existing, to check, so to speak, the conclusions deducible from the history of one asylum by those gathered from others.

The subjects of the several chapters are, The Movement of the Population of the Asylum of Auxerre; The Causes of Insanity; The Mode of Attack; The Duration; The Prognosis; The Cure; The Mortality; Incidental Affections; Seclusion; Causes of Epilepsy, and the Morbid Anatomy of Insanity. These several topics are studied statistically, and illustrated by numerous well-compiled and copious tables. His Introduction contains an apology for statistics as applied to the study of medicine, and defends their use and value. The chapter on Causes is very elaborate; and the careful records of his institution have enabled him to construct tables showing the influence of the seasons on insanity, as well as that of the barometric and thermometric conditions of the atmosphere, and that of the winds, of elevation of country, &c.

The character of the book—viz., a sort of catalogue of conclusions deducible from an extensive collection of statistics—forbids the attempt to give an analytical review of it. It must be studied by each one for himself in order to become acquainted with its contents and their value. To the diligent inquirer into the nature and causes of insanity it affords an ample store of facts, calculated to throw light on many debatable points.

The author intimates in his Preface that he has a collection of other facts "illustrating the state of the intelligence; of the moral sensibility and of the will in insanity; the functional changes; the organism involved by the existence of mental derangement; the disordered condition of the digestive functions; of the circulation; of the secretions of the liver, kidneys, skin, &c., in the affection." We hope to see these observations shortly published. To interpret the many mysteries clouding the whole subject of mental aberration we need a large accumulation of facts, collected at the present day with the aid of present enlightenment and of the modern means of research; and it is to physicians similarly circumstanced to M. Girard de Cailleux that we rather must look for these facts, than to those who are still charged with the multitudinous and onerous duties of asylum management.

ART. XI.—*Outlines of Surgery; being an Epitome of the Lectures on the Principles and Practice of Surgery delivered at St. Thomas's Hospital.* By F. LE GROS CLARK, Surgeon to the Hospital, Consulting Surgeon to the Western General Dispensary, &c.—*London.* pp. 256.

IN the early part of his career the surgical student often feels the want of some readable introductory handbook which, short of completeness, may serve to convey to him information on subjects of which he otherwise often continues ignorant until informed of them in a too-crowded detail. Such a book as this lies before us under the name of 'Outlines of Clinical Lectures, by one of the Surgeons of St. Thomas's.' In its present form and dimensions we argue that it will prove of constant utility to the student, and hope to see it pass through many editions. It is not, like too many modern books, intended mainly for reference; and we may affirm that, with slight literary merit or pretensions, it is deserving of the notice of the medical world for the subject-matter's sake. Such a work is open, we allow, to the accusation of being mistakable for a "Popular Treatise on Surgery," a class of production we should abstain from approving; but it will be disappointing to sly patients, chemists, and back-woodsmen, who may indulge themselves in the acquisition of it, to find that the treatment of disease is that part of the performance which is left, designedly no doubt, the most deficient in elaboration.

En attendant the publication of the lectures of which the present outlines are the first fruits, we shall remark upon the merit of the work as it stands. It is intended, the author informs us in his Preface, as an encouragement to the clinical industry of students, and, as we interpret his remarks, to serve them in lieu of larger books. It is not a mere framework like 'Tyrrel's Syllabus,' nor a systematized handbook like that of Druitt. We are ourselves of opinion that the occasional perusal by the student of the best class of monographs on disease forms a healthy kind of reading for him at every stage of his career, though they may cause a little delay in passing his examinations. Clinical study is of course to be regarded as the great field of industrial improvement; still, as has been said—it is not enough to look for a thing, it is well to be informed where you are to look for it.

There are, it may be said, two main divisions in the work before us, one of which, constituted by the last section, stands somewhat in the attitude of excuse for the other. This last section comprises those subjects which, with something like repetition, are the most amply treated of; and, indeed, it leaves nothing to be desired either in vigour or completeness: it is every way satisfactory, the author having somewhat overstepped the narrow bounds he has prescribed to himself elsewhere. The preceding sections, where the art and science of surgery is sought to be comprised in less than two hundred pages, have fallen somewhat short of accomplishment, for the obvious reason that one part must necessarily suffer to give full development to the rest; and the promise has not been very fully maintained to make the ennunciation of great principles the main feature of the book. By such method alone could it have been included in its present proportions.

As may be expected in a digest of surgical matters of great importance by so well-known a surgeon, omission rather than error will suggest itself on the most critical survey. We confess we have read the chapter on the diagnosis of certain fractures with more satisfaction than any other part, but some there are which approach it in excellence. A chapter on suspended animation and the use of chloroform follows it. In advocating the use of the anæsthetic, the author bids us observe that the vapour of chloroform not only pollutes the air but prevents this from exercising its decarbonizing influence on the blood: he also insists on the practical advantage of a previous arrangement or compact with the patient, directing and obliging him to indicate by some mark of intelligence (pressing the assistant's hand, or other sign) the boundary beyond which volition, and with it sensibility, ceases.

The author's description of congestion, in the early part of the work, will give a fair idea of his style and method.

"Passive hyperæmia," he says, "or congestion, is the effect of mechanical obstruction, as where a vein is compressed or plugged. The consequence of such venous obstruction is distension of the capillaries, and, if continued, infiltration of tissues from excessive exudation of liquor sanguinis. Thus, œdema or local dropsy, ascites or abdominal dropsy, and anasarca or general dropsy, are produced. In this filtering process through the porous vessels the nutritive quality of the liquor sanguinis is modified by changes in the proportion of albumen and salts; so that, although the quantity of blood actually present in a part affected is unnaturally large, it is blood despoiled of its nutritive and stimulating properties, and therefore valueless for growth. Thus, congestion is an atropic condition; and its effect is the same as where there is simply a deficient supply of nutrient blood, without detention, as proved by the feebleness of muscular tissue, which is the seat of such congestion, the occurrence of ulcers in varicose limbs, &c."

The description of lithotomy is brief and good. We offer it the more willingly as a proper attention is given to making a free external incision, and to the management of the staff—two points of importance—

"The operation of removing a stone from the bladder by cutting is performed by making a lateral incision in the perinæum, between the crus penis and bulb, and thus reaching the membranous part of the urethra, which is then perforated; and the left side of the prostate is next divided to an extent sufficient to allow the introduction of the forefinger: a pair of forceps is then guided through this opening into the bladder and the stone seized. Various cutting instruments are employed, but a grooved staff of full size, either curved or straight, is essential to guide the operator in entering the urethra and bladder. If the operator take the staff into his own hand, when he enters the bladder it should be either straight or moderately curved; if he entrust it throughout to an assistant, it should have a large and long curve, and should be held immovable and well hooked-up under the pubes. The desiderata in this, the lateral operation, are:—1. To make an external, free and well-depending opening. 2. To open the urethra as far back as convenient. 3. To make a definite and sufficient opening in prostate. 4. To take care that all the cellular tissue intervening between the external wound and prostatic opening be freely divided. To accomplish these objects, the external incision should commence on the left side of the raphé, about an inch in the front of the anus, and be carried downwards and outwards between it and the tuberosity of the ischium. When the staff is depressed by the operator, it is safer to lay aside the scalpel with which the urethra is opened, and to substitute a beaked knife or gorget for incising the prostate, &c."

Our author writes discouragingly of the practice of gouging bone for the purpose of modifying caries or ulcerative process by inducing new action. He thinks well of the operation of excision of the knee-joint in its proper field of selection. Touching the *vexata quæstio* of opening the sac in strangulated hernia, he expresses himself as follows:

> "If the inexperienced operator elect to leave it unopened, he may do well to bear in mind that he may push back a rupture without relieving the stricture; that he can have no acquaintance with the actual contents of the sac and their condition; and that the inflammatory products within the sac are returned, together with its other contents, into the abdomen, instead of being allowed an external drainage. Inflamed and congested peritonæum, whether of sac or intestine, are not susceptible of ill consequence from exposure or incision as the healthy membrane is; and the assumed analogy between this operation and the taxis is not accurate; inasmuch as the success of the taxis is a measure of the resistant strangulation which may be estimated at much below that of a hernia in which it is necessary to enlarge the opening before the strictured intestine can be returned. As a rule, the advantages would seem to be decidedly in favour of opening the sac; in exceptional cases, especially of small and recently-strangulated femoral herniæ, the structure may be divided external to the sac."

Altogether, there is an incalculable amount of surgical matter in this small volume. The directions for the operation of tracheotomy are clear and good. In treating of injuries of the head, we can only regret that our author has not dwelt a little on the delicate point of practice, bleeding in cases of concussion; by a dash of the pen, he seems to disparage the advantage of it. We have occasionally to remark, in modern practice, that several things which used to be done with caution and judgment are now omitted altogether. This makes the surgeon's task easier, if it does not add to his importance. In that part of the work which is devoted to diseases of the joints, we cannot say that we are satisfied with an account of ulceration of the cartilages which omits to mention pain as a leading symptom. We take this as evidence that the author has not himself suffered from the complication.

"He best can paint them who shall feel them most."

Neither does the treatment laid down for this affection fully satisfy us.

Finally, we shall, with some deference, submit the following consideration:—In a regulated curriculum, which is perhaps a little too much calculated to oppress a conscientious student with feelings of timidity from overstrained anatomical detail, and a preciseness of direction which often partakes of pedantry, we are glad to see books like the present, which give him some idea of what is the final aim of his teaching. It is quite another thing to associate him with grand manœuvres of surgery. We hope the tyro may not have to tie large arteries, or force the prostate, &c., except on very great emergencies, without further preparatory study than this work conveys.

Art. XII.—*Excursions Scientifiques dans les Asiles d'Aliénés.* Par Dr. P. Berthier.—*Paris*, 1862. pp. 104.

Scientific Visits to Lunatic Asylums. By Dr. P. Berthier, &c.

The author of this treatise is principal physician of the public asylum of Bourg, in the department of Ain, and has favoured his countrymen with a notice "of his impressions" of the state of a considerable number of the asylums of France, gathered from personal inspection. Although he justly speaks with pride of several of the modern departmental asylums of his native land, he is not sparing of criticism where deserved, and boldly denounces some existing institutions as totally unfit for their purpose and discreditable to the country.

The notes on the several asylums are brief, and afford no very intimate insight into the details of construction or of management. The historical introduction to his account of each asylum will be read with interest, and his English readers will be rather struck with the generally patent fact of the slow progress made by the teachings of Pinel and Esquirol in most departments of France, and of the long strides still required to bring the institutions of many of them to a level with those of our own country. Curiously enough, Dr. Berthier discovers that France took the initiative in framing legal enactments to provide for the due care and treatment of lunatics, though no actual steps were taken in this direction until 1838. He arrives at this conclusion, inasmuch as the first enactment passed in England of which he has any definite knowledge is that of 1846, or eight years after the French decree. His acquaintance with the history of lunacy in England is evidently secondhand, borrowed from the random statements which pass current in France, and are copied and recopied, without any attempt to verify them, by each author who wishes to exhibit his knowledge of the past or present state of lunacy in the kingdom.

This book is honestly written, and the criticisms it advances on the state of many French asylums are well calculated to arouse the attention of the public, and still more of the Imperial Government, to the necessity of interposing with a view to their improvement. Public opinion in France is, however, little exercised and little valued in such questions, which are left to the discretion of the Government; and the operation of this central power appears much less energetic than might be anticipated, being much neutralized by the ignorance and local jealousies of its subordinates in the departments.

To the English physician proposing to visit the institutions for the insane in France, this treatise will serve as a useful guide, and particularly so as it is provided with an itinerary map of the asylums, and of the means of reaching them by road or rail. Moreover, the author proposes a second portion, giving details of the other establishments for the insane not described in the present one.

ART. XIII.—*Experiments on the Formation of Infusoria in Boiled Solutions, &c., &c.* By JEFFRIES WYMAN, M.D.

DR. WYMAN, in this pamphlet, first published as an article in the 'American Journal of Science,' relates very simply and circumstantially the particulars of a series of thirty-seven experiments recently performed by himself, with the view of throwing light upon the long-vexed question of spontaneous generation. The methods which he has adopted have been as follows (1 and 2):—An iron tube, filled with wire, having been connected with the neck of a flask, in which is placed the organic infusion which is to be the subject of the experiment, the latter is boiled for a length of time varying from fifteen minutes to two hours, the iron tube being in the meantime heated to redness. "While the contents are boiling," says Dr. Wyman, "the steam formed expels the air from the flask: when the boiling has continued long enough, the heat is withdrawn from beneath the flask, and as the steam condenses the air again enters through the iron tube, the red heat of which is kept up, so that all organisms contained in the air are burned."

3. The original method used by Needham—that, namely, of sealing up the organic infusion, without any precautions, in a flask, and then submerging the latter for a time in boiling water.

4. Finally, four experiments (Nos. 34–7) were tried in a Papin's digester under pressure.

Dr. Wyman's experiments lead to results vastly different from those to which his very beautiful and elaborate researches have conducted M. Pasteur.* The latter observer states that he has *never once* found any organisms where he had previously boiled the organic infusion and supplied it with air through a heated tube; except, indeed, where milk was the subject of the experiment, in which case he was led to believe that the alkalinity of the milk in some way protected the germs contained in it from the action of heat at the temperature of boiling water. He found, accordingly, that when the temperature was further raised, or ebullition at the ordinary temperature was prolonged, no organisms were developed. M. Pasteur's ordinary time of boiling his experiments was three minutes only.

Dr. Wyman, on the other hand, finds that in the vast majority of experiments performed according to his methods 1, 2, 3, organisms are formed—viz., in most cases, vibrios, bacteriums and spirilliums, and sometimes monads, and kolpoda-like bodies, some of them having ciliary movements. In the last four experiments, in which the ebullition took place under a pressure of two atmospheres, organisms appeared in two cases, and not in the other two.

We are far from being disposed to undervalue the importance of M. Pasteur's researches, or from grudging the tribute of admiration which has been so freely accorded to them; but cannot go so far as to say with Professor Huxley* that M. Pasteur has finally settled the question as against heterogenesis. This, in our opinion, no single observer can possibly do until his experiments have led to equally successful results in the hands of other competent persons. Moreover, we must freely confess that the uniform success of M. Pasteur's experiments, the remarkable correspondence of his results with his *à priori* views, and the whole character of his treatise, renders this even more than usually necessary in the present instance. At any rate, so long as observers of such reputation as MM. Mantegazzo, Joly and Musset, Pouchet and others,† are led to results the most opposite to those of M. Pasteur, it is but idle and misleading to speak of the question as being settled. At the same time, we cannot consider that Dr. Wyman has gone far in his present publication to throw doubt upon M. Pasteur's results, inasmuch as his own experiments as related in it are open to several serious objections. Thus, he does not give the length of his heated tube; and to judge from his drawing, it would seem to have been not more than a few inches long. This, if so, leaves room for the objection, which, whether valid or not, is not easily disposed of, that the length of time during which the germs in the air would be exposed to heat in passing through such a tube is insufficient to destroy them all. Again, the material of which the tube was composed—viz., "iron filled with (iron?) wires," was, we think, an unfortunate one, inasmuch as, at the same time that it destroyed the germs, it would decompose the air and fill the flasks chiefly with nitrogen. Nitrogen, it is true, may not be a medium very favourable to the development of organic beings, but whether it be so or not, it is an objection often urged against the employment of heated air in such investigations, that the constitution of the air may be affected by the process which it has undergone; and it is clearly undesirable to introduce an additional complication into a subject already almost hopelessly intricate.

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* Ann. des Sciences Nat. Series iv., tome xvi. p. 32.

It must be stated, however, that in a few of the experiments a glass tube, filled with spongy platinum and asbestos, was substituted for the iron one without affecting the results obtained.

ART. XIV.—*On the Influence of the Weather upon Disease and Mortality.* By R. E. SCORESBY-JACKSON, M.D., F.R.S.E.—*Edinburgh.* pp. 54.

THIS treatise consists of an analysis of a large mass of statistics which Dr. Scoresby-Jackson has collected on the influence of the weather, and we much regret that the results which they yield are so incommensurate with the labour that must have been expended upon them. It is always unsatisfactory to see labour of any kind unrewarded, and more especially labour which is given to an apparently good cause. Mankind—that is civilized mankind—has everywhere agreed that the weather does exercise some influence upon health, and common experience has established with pretty considerable accuracy that certain varieties of weather produce definite effects upon the human frame. That there is a relationship between the terms of comparison in these two cases is evident, but still it is a relationship of only a remote and artificial nature. Every one knows, for instance, that a severe winter is very fatal both to the very young and the very old; that an easterly wind is not by any means favourable to bronchitics; that a damp, mild spring is fertile in epidemics, and other aphorisms of a similar character: but it would be very interesting if we could determine precisely to what extent these effects are due to meteorological influences, and whether they are so in a primary or secondary manner. Various observers in this country, and on the continent, have attempted to solve this problem, but as yet with indifferent success. The conclusions at which they have arrived are in most cases so discordant as to show either that the modes of research which they have employed are not sufficiently accurate to lead to uniform results, or that the phenomena under investigation are exposed to the action of so many "disturbing causes" making it hopeless to think of reducing them to an invariable estimate. We cannot say that Dr. Scoresby-Jackson has done much to diminish the want of harmony which has hitherto reigned amongst the explorers of this district of science. Not only are the inferences to which his statistics appear to lead frequently opposed to those which flow from the investigations of other observers, but they are sometimes opposed to one another. Thus, in discussing the value of certain statistics collected to show the influence of humidity on mortality, Dr. Scoresby-Jackson observes:—"Here then we have conflicting results. Following the rain-fall as our guide to the hygrometric condition of the atmosphere, we conclude that a dry atmosphere promotes mortality: if we refer the humidity present in the atmosphere to the standard calculated by Mr. Glaisher, we are forced to an opposite conclusion." In the table constructed to show the influence of variations of barometric pressure on mortality, we are also told that "there is evidently a good deal of conflict." Indeed, notwithstanding a good deal of clever manipulation of arithmetical means, and a shuffling of statistical tables, which is eminently calculated to awaken the idea that if Dr. Scoresby-Jackson has failed to extract much grain from the ears he has gleaned, it is not from want of their being well thrashed, the positive conclusions at which he arrives are so meagre and unsatisfactory that we can hardly feel surprised if he appears to feel the necessity of magnifying a little their importance. When we find that the only positive results which are deducible from the laborious comparison of whole pages of numbers are resolvable into such statements as that "a protracted low temperature in winter largely increases the death-rate amongst children under five years of age," "that the north, north-east, and east winds decidedly tend to increase the death-rate from bronchitis," and that "extremes of temperature are always fatal, but eminently so when long sustained," it seems hardly worth while inquiring whether the statistics upon which these novel and interesting deductions are founded are themselves trustworthy or not.

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* Lectures to Working Men, p. 81.

† See Comptes Rendus, tome lvii. No. 12.

But even if the results of Dr. Scoresby-Jackson's investigations had been more positive and original than they are, we should have felt compelled to object to them on grounds which must strike at first sight every reader who peruses them. Let us take, for instance, the question of the influence of weather upon mortality alone. And here we may as well remark that, though from the title of Dr. Scoresby-Jackson's work it would be inferred that it included the influence of weather upon *disease* as well as mortality, the former subject is almost wholly ignored. Dr. Scoresby-Jackson alleges as a reason for this that after collecting a mass of statistics from several dispensaries and hospitals with the view of comparing the rate of *morbility* with that of mortality, he was obliged after much labour to abandon the *morbility* statistics as next to worthless. But it may be asked, How are we to separate the subject of mortality from that of disease? Death is not a condition *per se*, it is merely the climax of disease; and if the weather exercises any influence at all over mortality, it does so in virtue of its influence on the progress of the diseases which terminate in death.

But even admitting that we may, for purposes of comparison, arbitrarily separate the question of mortality from that of disease, what are the chances of our being able to establish any definite connexion between it and the weather? Given the rate of mortality, what causes other than the weather are likely to influence it? Without attempting to specify all the causes by which the death-rate may be affected in any district, we may mention as among the more important—sanitary conditions, such as drainage, ventilation, sufficient clothing, &c., proper kind and amount of food; peculiar occupations or pursuits; liability to epidemic disorders; accidents; aggregation of population in communities as compared with its diffusion over a wide tract of country. These and other conditions all exercise an important influence over the development of disease, and consequently on mortality, quite irrespective of that of the weather. How is it possible, we may ask, to eliminate the influence of these various agencies, or to determine to what extent the influence of the weather upon mortality is favourably or unfavourably affected by them? But, even supposing that we overcome this difficulty, we have yet another source of obscurity to clear away before we shall be in a position to place the slightest reliance upon any statistics that we may obtain on this subject. We will assume that in the month of April, for instance, our observations give us—and we take the illustration from Dr. Scoresby-Jackson's tables —a mean temperature of 42·7; a mean barometric pressure of 29·7; a rain-fall of 2·3 inches; winds varying between N.E. and S.E., and varying also in force; and, with all these meteorological data, a mortality in a known population of 243·6. To what extent are we justified in assuming a connexion between these two sets of conditions? It is very obvious that a large portion of the mortality of any given month is due to causes that are altogether independent of the month itself, from their having been in operation long prior to it.

The man who dies from paralysis in April may have received the first shock of the disease six months previously, and the disease may have steadily progressed since that date until it culminated in death. The same may be said of the great majority of chronic diseases. Acute diseases which run a rapid course might, it is true, be employed as a source of statistics for our purpose; and there are some diseases, especially rheumatism and pneumonia, with regard to which a series of observations on the influence of weather upon their production would be especially valuable; but this is a very different thing from an attempt to determine the influence of weather upon mortality from all causes *en masse*.

We might, if it were desirable to do so, adduce other objections to Dr. Scoresby-Jackson's statistics, more especially on the ground of their fallacious character in a mathematical point of view; but the results which he has obtained are of themselves so trivial that it is quite unnecessary to waste our readers' time in inquiring whether the actual numbers upon which they are founded are trustworthy or not. The fact is, that Dr. Scoresby-Jackson has not only attempted the solution of a problem which in the present state of our knowledge is perfectly insoluble, but the process which he has adopted is radically so unphilosophical in its character as to be incapable of leading to any satisfactory results. Even if it were possible to determine the relative value of the various disturbing causes by which the influence of the weather upon the human system is intensified, or counteracted, any investigation of the relation existing between the weather and the mortality of any given district would be about as useful as an inquiry into the connexion between the political tenets of the Republican party in North America and the actual number of slain in the battle of Bull's Run. What we really wish to discover is, what is the influence which the weather exerts on the development and continuance of the diseases of which the mortality is the result, in the one instance; and what is the influence which the spread of Republican sentiments has had in producing the state of things that led to the conflict at Bull's Run in the other? Until Dr. Scoresby-Jackson can give us some better reason than he has done for the separation of these two considerations, we must be excused for thinking that the labour he has expended on the collection of these statistics is in great part entirely thrown away.

ART. XV.—*Den almindelige Therapie, i fire Forelæsninger.* Af Dr. og Prof. O. BANG, Conferentsraad, Commandeur af Danebrog og Vasa, Danebrogsmand m. m.

General Therapeutics, in four Lectures. By Professor BANG.—*Copenhagen*, 1862. 8vo, pp. 56.

FROM the preface to the above work it would appear that no treatise on general therapeutics exists in the Danish language. The author, impressed with the advantages derivable by medical students from the possession of class-books, has published the present volume as a foundation for future lectures.

The author divides treatment into—1. *Cura causalis*, referring, not to what earlier writers designated causa proxima—the essence of the disease—but to the occasional or predisposing causes. 2. *Cura radicalis*, which may be based indifferently on theory and on experience, may be both Cura rationalis and empirica. 3. *Cura symptomatica, palliativa*: this is often as important as the foregoing; it is employed to remove dangerous or painful symptoms, and those conditions which might interfere with the efforts of nature; it is frequently adopted when we do not recognise the disease, but where active interference is nevertheless required. To these may be added two others—*Prophylaxis*, whose aim is to prevent the disease; and, lastly, which makes the malady or death easier to bear,—*Eupathia* and *Euthanasia*.

With reference to the treatment of the different diseases, the methods of cure may be divided into two classes, of which one has no subdivison, while the other has many subdivisons,—the *expectant* and the *active*.

Professor Bang calls especial attention, *in limine*, to the remarkable fact that—

> "Many a remedy, inactive in the dose hitherto recommended, becomes of great value when it is given in much larger doses; for example, in one which would prove fatal to a person in health, in whom its action must of course be different. We must not forget that the medicine, as an unusual stimulus, may, in a healthy person, produce an anomaly, a disease of greater or less gravity, while in a patient it loses its injurious action in the conflict with the disease; indeed, the fewer the symptoms are attending its administration in health, which manifest themselves on its employment in disease, the more the remedy is probably indicated. The large doses of opium, for example, and of mercury which a patient can bear without detriment, and seems to require, would, in a short time, kill or greatly injure a person in health."

The large doses of opium found beneficial in peritonitis from perforation, delirium tremens, &c., and the vast quantities of stimulants taken with advantage in certain morbid conditions, at once suggest themselves as familiar illustrations of the author's remarks.

Having made some observations upon the expectant, Professor Bang proceeds to consider the active method of treatment. This in general commences with the *Cura causalis*.

The cause having been removed, the *direct* means of cure come under consideration. These may be divided into two classes; one including those means which act upon the strength, the other comprising those which seem chiefly to have influence upon the material of the body.

Of these classes the former again includes remedies of two kinds:—1, those which promote; and 2, those which diminish reaction.

The remedies belonging to the first of these subdivisions may be further divided into stimulants, tonics, and astringents. Those belonging to the second may be enumerated as sedatives, comprising refrigerants and antiphlogistics, anæsthetics, hypnotics, anodynes, antispasmodics, &c.

The remedies comprised in the second class—i.e. those which act upon the proper secreting and excreting organs, are considered under the heads of errhines, sialogogues, expectorants, emetics, cathartics, diuretics, diaphoretics, emmenagogues, derivants, escharotics, &c.

Of an elementary class-book, such as the above, a detailed analysis would of course here be quite out of place; we have therefore contented ourselves with endeavouring to give our readers a general idea of the arrangement of Dr. Bang's useful manual.

Art. XVI.—*Proceedings of the International Temperance and Prohibition Convention, held in London, September 2nd, 3rd, and 4th,* 1862. Edited by Rev. J. C. Street, Dr. F. R. Lees, and Rev. D. Burns.—*London,* 1862. pp. 518.

One of the marked features of the present time is the great effort which is now in progress in the cause of temperance—an effort having its origin in the conviction of the vast evils produced by intemperance. Had we any doubts on this subject, the work the title of which we have given above would carry conviction to our minds that excess in intoxicating drinks is at the root of much of the poverty, much of the vice and crime of society in every country in which such drinks are abused; and that poverty, vice, and crime have ever been in a close relation, though not in any exact ratio, to the degree of the abuse.

The attempt to arrest the evil is meritorious, and has our very best wishes; and when we consider the scale on which it is made, the distinguished men by whom it is encouraged and aided, and the ability and energy displayed in carrying it out, we become very hopeful of a certain amount of success. The great object of the Convention, as put forth in its Proceedings and insisted on in various detail, is not only to put a stop to drunkenness, but to drinking; not to the abuse of intoxicating drinks, but to their use. Now, when we reflect on the history of these beverages, especially in those countries most advanced in civilization—measuring it by the intellectual and moral progress of the people, taken as a whole, we cannot be so sanguine as to expect that this its object will be accomplished; and, indeed, do not think, were it attainable, that it would be desirable.

What is there that is not liable to abuse? Intemperance in eating—gluttony—though not so injurious to society as the vice of drunkenness, is hardly less so to the individual who gives way to it. The golden rule of conduct is to be temperate in all things. This cannot be too strongly insisted on; and sure we are, that as it is an established and universally-admitted truth, in accordance with reason and the experience of every one, the inculcation of it would have full approval and encounter no opposition, such as the prohibitive system of the Convention has met with.

It is not our intention to notice entirely the "Proceedings" of the "Convention;" our remarks must be limited to but a few of them. The great subject of the work, the evils of intemperance, is discussed in an almost exhaustive manner, under a variety of points of view, "Historical and Biographical," "Educational and Religious," "Social and Sanitary," "Economical and Statistical," "Political and Legislative." In each of these sections, much interesting and valuable information is given, accompanied by summaries, to aid those readers who may not have perseverance or patience enough to peruse the whole,

The scientific and medical section is that which has had most our attention. We cannot express approval of all therein stated, whether theoretically or practically considered. Alcohol is denounced as a poison throughout; nor is it allowed, like other poisons, to have any medicinal virtues; it is condemned without qualification, and pronounced to be worse than useless in medical practice. Mr. Higginbottom is especially severe on it. In one place he states:

> "I have not known a single disease cured by alcohol. On the contrary, it is the most fertile producer of diseases, and may be truly considered the bane of medicine and the seed of diseases. It is entirely destitute of any medicinal principle implanted by the Creator in genuine medicines—such as emetiæ (*sic*), in ipecacuanha; rhein, in rhubarb; jalapin, in jalap; quinine, in Peruvian bark, &c. Alcohol is the invention of man, in the forms we use it, by the destruction of the good food which God has given us."

Further on, he says:

> "I have discovered a great truth and have made a great discovery—*that alcohol in every form may be dispensed with in medical and surgical practice, and is not required in a single disorder or disease.*"

The italics are his. He adds:

> "What evidence can be clearer or more satisfactory? My practice has been open to hourly inspection and observation for thirty years, in the centre of a large, populous town, surrounded by more than forty surgeons, most of them intelligent, discerning men. Surely some of them would have informed me of my insufficiency or mal-practice, had I been in error; but I have heard of no such remark from a single individual, although in daily communication with them."

We shall not comment on this experience, or on the style in which it is expressed.

Professor John Kirk, *Edinburgh* (*sic*), is equally decided in his opposition to alcohol, and condemnatory of its use, even as a medicine. Here is an example of his style, experience, and reasoning. He states:

> "A case came lately under our observation. A man advanced in years, but originally of a strong constitution, was dying of pressure on the respiratory nerves, and had been for above a week in great suffering from difficulty of breathing. The heart's action had been irregular and weak all that time. The pulse such as seemed to threaten sudden death at any moment. This continued until mortification of the extremities had visibly set in. Then, without anything whatever having been administered, both his pulse and breathing became regular and comparatively strong. The heart and lungs seemed to recover nearly full and healthy action. This is no uncommon case, as every one at all acquainted with the experience of the dying must know. Nor do we think it difficult of explanation. But place it over against the recovery of pulse by the action of alcohol, and it will be very difficult to adhere to the idea that the liquor

is a stimulant merely because its administration is followed by the same recovery of pulsation which is the effect of partial death of the animal frame."

He further remarks:

> "In those cases in which there is an apparent *rally* of the vital powers on the near approach of death, no explanation is required further than that which is found in all struggles in which a combatant feels relief and a partial recovery of vigour when he has given up the conflict. The heart and lungs have perhaps been for months labouring to keep up the vital circulation in defiance of growing obstacles in other parts of the frame of which they form so important a centre. The duty devolving on the circulating organs is almost all at once lessened in a very high degree by death at the members, and, consequently, the former feel relieved and rally, till the relentless agency of dissolution reaches their proper substance, and total death ensues. This would rather lead us to believe that partial and temporary suspension of life in the finer portions of the nervous system is the true effect of alcohol, and *not* stimulus. The analogy places the action of the liquor in the came category with death's power."

On such loose kind of writing and illogical reasoning we need not comment: we feel as if an apology were due to our readers for making the quotation; the place where we found it must be our excuse, and the title of its author. We regret that the cause of temperance should have been pleaded in this special, partial, and irrational manner; and that the Convocation has not found professional men of higher authority than those, the five in number, whose services it has engaged.

Granted that alcohol neither nourishes nor warms, neither administers to the formation of tissue nor to the production of animal heat, does it follow that it possesses no medicinal quality? The same question might be asked respecting calomel, corrosive sublimate, iodine, nitrate of silver, and other medicines innumerable, which, like alcohol, we owe to chemical science. We apprehend we might give offence to our readers were we to enter into any prolonged arguments on the subject, nor indeed is this the occasion for such a discussion. We prefer stating a case, one which came under our own observation, in which we were satisfied that recovery was due to wine. A robust man, labouring under severe remittent fever, had been copiously blooded; there was a daily aggravation of symptoms until he appeared to be moribund: so he was reported to us by two medical men who were attending him; they left him, they said, dying. We found him as they had described, hardly conscious, his pulse barely perceptible, his surface wet with cold sweat. Having some faith in wine as a stimulus, we had immediate recourse to it; with some difficulty we got him to swallow a teaspoonful of Madeira, and presently another and another, his pulse strengthening, the skin becoming warmer and drier; and before we left him all the worst symptoms had disappeared, and in less than a fortnight, without any reverse, he was convalescent.

That alcohol, like almost every other medicine, has been too often injudiciously prescribed we do not doubt; but it does not therefore follow that it can be of no use medicinally given, only noxious and poisonous. Extreme views are seldom correct, and one-sided views never. Throughout this book we are sorry to see too much of this tendency, as if there were no other influences than intemperance conducing to vice and crime, to poverty and disease. Great and widely spread as are the evils resulting from drunkenness, there are others, from unchecked passions and abused instincts, as much to be deplored and even more degrading. The sobriety of a people affords no just and exact criterion of their morality. Of this, without referring to history, especially biblical history, have we not sufficient proof in the deterioration of races, which must impress every intelligent traveller who proceeds from the north to the south, from the west to the east? Is it not a fact that in those countries where wine is so little used, as in Turkey, Hindostan—in brief, in the whole of Asia—there the brutal passions are in the ascendancy, morality, especially amongst the Hindoos, is at a low ebb, and the higher faculties are but little cultivated? What would be the effect on our northern races of the total disuse of intoxicating drinks—drinks which from time immemorial they have been accustomed to—we cannot pretend to say, except to express our opinion that it would not be a pure good: probably opium would be more used and abused, and also tobacco, and gluttony amongst the well-to-do would be more prevalent. But that these drinks will be given up *in toto*, whatever eloquence is employed, whatever arguments are used, or that the legislature will ever assent to prohibit their use, seems to us the most chimerical of ideas.

Would that the Convention had mainly exerted itself to accomplish what we would hope is less impracticable—viz., the putting a stop, as much as possible, to the sale of ardent spirits of all denominations. It is the use and abuse of these—the use too often leading to the abuse—which does so much harm. In excess, they are the poisons productive so often of delirium tremens, and of insanity, and of chronic organic disease. Even drunkenness produced by wine and malt liquors, however much to be deprecated, is comparatively less injurious, whether we consider the immediate effect or its consequences.

We express our opinion in this decided manner respecting ardent spirits, believing as we do that if their sale be permitted, as at present, they will always be abused, the temptation to indulge in them is so strong. Could we think the danger less, we would say no interference would be necessary, inasmuch as even ardent spirits, if properly used, indulged in to no excess, may be safe, and, under certain circumstances, even beneficial.

PART THIRD.

Original Communications.

Art. I.

Contributions to the Chemistry and Physiology of Fœtal Nutrition. Being an Inaugural Thesis to which the Medical Faculty of the University of Edinburgh awarded a gold medal in 1862. By Arthur Gamgee, M.D., late Senior President of the Royal Medical Society, Edinburgh, late Resident Physician in the Royal Infirmary, and Assistant to the Professor of Medical Jurisprudence in the University of Edinburgh.

Part I. *On the Chemistry and Physiology of the Milky Fluid found in the Placental Cotyledons of Ruminants.*

There are few subjects more worthy of study, or which offer more interest to the physiologist, than an inquiry into the chemical changes which take place in the placenta. Whether certain portions of the maternal blood percolate unaltered into the fœtal capillaries, or whether the maternal portion of the placenta elaborates a nutrient material, which is afterwards absorbed by the fœtal portion of the organ, is one of the most interesting questions which present themselves in such an inquiry, and it is with the hope that I may supply some of the data which are required for answering this question, that I am led to publish the observations which I have made.

The attention of naturalists and physiologists has long been directed to the milky fluid which can be seen in the placenta of some of the lower animals, and more especially to that which is found in the placental cotyledons of the ruminantia.

In this order, the villi of the chorion become developed in patches all over its surface, and these villi, dipping into the mucous follicles of the uterus, give rise to those rounded or oval bodies which have been named cotyledons, and which must be regarded as small placentæ. These cotyledons, which differ in number, size, weight, &c., in the different members belonging to the order, can be easily divided into a fœtal and maternal portion. The fœtal portion, which is of a deep red colour, is seen to consist of the villi of the chorion beautifully developed; the maternal portion, which is usually very white, consists of the hypertrophied uterine mucous membrane, and is covered with the openings of the much-enlarged mucous follicles, on squeezing which a considerable quantity of a creamy fluid escapes. I may add that, by exerting gentle pressure, the fœtal can be readily separated from the maternal portion of the cotyledon, without causing the rupture of any vessels.

Before considering in detail the chemical characters of the fluid found in the cotyledons, I shall quote the opinions which have been entertained regarding its origin and nature.

Duverney* considered the maternal portions of the cotyledons to be true glands, whose function it is to separate a juice which the placenta absorbs for the nutrition of the fœtus.

Eschricht described the cotyledonary fluid of ruminants as a white, thickish fluid, secreted by the utricular glands (mucous follicles), and which is absorbed by the bloodvessels of the chorion.

Prevost and Morin† first examined this fluid. They arrived at the conclusion that it contains albumen, fibrin, casein, a gelatinous substance, blood-colouring matter, osmazome, fat, and salts. They made a quantitative analysis of the secretion, which agrees in the main with the results obtained by Schlossberger and myself. This will be found at the end of this paper, below the table of analysis made by Schlossberger and myself.

Professor Schlossberger, of Tübingen, published, in 1855, in No. 95 of the 'Annalen der Chemie und Pharmacie,' a short paper, entitled "Die Uterinmilch der Wiederkäuer." In this paper Schlossberger gave the first correct account of the chemistry of the fluid. He described the result of the examination of four specimens of uterine milk of the cow, and performed a quantitative analysis of two of these specimens, of which he gave the results, and I may here state that I shall frequently make use of the name which Schlossberger first gave to the fluid. The results of Schlossberger's researches were the following: Uterine milk is a fluid of creamy consistence, exhibiting, when examined microscopically, many free nuclei, fat-globules, and epithelial cells. It has a decidedly acid reaction, and contains albumen, fat, and salts, but no sugar. Schlossberger did not direct his investigations particularly to determine whether the fluid contained casein or not, nor did he in any way determine the amount which can be obtained from the cotyledons. I may incidentally state that my investigations were carried on quite independently of anything that had been written by the German chemist, as when I undertook the examination of the fluid I was not aware of it having been examined by any one. Schlossberger's paper only became known to me after I had completed my second quantitative analysis.

Colin‡ considers the cotyledonary fluid to be simply the product of decomposition, and that it therefore does not exist during life. He says that it can only be obtained from the cotyledons a considerable time after death, the period varying with the temperature and other concomitant circumstances. To this assertion of Colin I think myself entitled to give an unqualified denial. I repeatedly obtained the uterine milk which I examined from uteri which had been so recently removed from slaughtered animals that they were still warm, though my experiments were conducted in the middle of winter. Thus, my first quantitative analysis of uterine milk was of a specimen obtained from the cotyledons within six hours of the death of the cow. The quantity obtained from 1000 grains of the maternal portions of the cotyledons was 149·8 grains. Surely this could not be the product of decomposition, especially when we take into consideration the fact that the temperature happened to be below 32° Fah.

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* Œuvres Anatomiques, tom. i. p. 538.

† Mem. de la Société Phys. d'Hist. Nat. de Genève, tom. ix.

‡ Traité de Physiologie comparée des Animaux Domestiques, p. 600.

On the Process adopted to obtain the Cotyledonary Fluid, used in my Experiments.—I obtained the fluid by squeezing firmly the maternal portions of the cotyledons, enveloped in calico. I employed my fingers to express the fluid, as I found that by this means alone I could regulate the pressure to be exerted. I found that if the cotyledons were not fresh, or if too much force was used in squeezing, the tissue became reduced to a pulp, which became mixed with the uterine milk. I never succeeded in obtaining all the uterine milk present in the cotyledon, for, firstly, a considerable quantity was lost on the calico in which the cotyledons were enveloped; secondly, a force sufficient to express all the fluid could not be exerted without reducing the tissue of the cotyledon to a pulp, which, becoming mixed with the uterine milk, would have vitiated the results of the experiments; thirdly, some of the uterine milk remains adherent to the fœtal portion of the cotyledons, from which it cannot be removed, unless mixed with blood in considerable quantity.

With the means at our disposal for obtaining uterine milk, it must be evident that some of the cellular structures of the cotyledons must be inevitably mixed with it. The results obtained by Schlossberger and myself show it to have, however, a tolerably uniform composition.

On the Physical and Chemical Properties of the Fluid found in the Cotyledons.—The fluid obtained by expression from the maternal portion of the cotyledons is of a white or rosy-white colour, and of creamy consistence.

The *specific gravity*, as ascertained by means of the bottle at 60° Fah., of two specimens obtained from the cow, was 1033 and 1040. I found the specific gravity of two specimens of the fluid from the cotyledons of the cow to be 1033 and 1031.

When examined *microscopically*, the fluid exhibits an abundant molecular basis, in which float numerous fat-globules and epithelial cells. Many of these cells possess a highly granular and fatty appearance, and contain nuclei which are brought out by the action of acetic acid. The majority of the cells are *evidently* the spheroïdal cells of the mucous follicles, which have undergone a fatty change.

Reaction.—With only one exception, I found the uterine milk of the cow and of the ewe to have an alkaline reaction, when fresh, becoming very decidedly acid as soon as putrefaction set in. In the one exceptional case to which I have referred, the reaction was neutral. In this respect my results differ from those of Schlossberger, who, speaking of the reaction, says: "Das secret war geruchloss und reagirte in allen vier Fallen deutlich wenn auch schwach sauer. Ich wurde dadurch an die haüfig saure Reaction der Kuhmilch erinnert," &c.

When the fluid obtained from the cotyledons is heated, we notice the formation of a pellicle over its surface, as when milk is boiled. (Such a pellicle was formerly always supposed to indicate the presence of casein in the fluid in which it occurred.) The liquid then solidifies, from the coagulation of the large quantity of albumen which it contains, and which is readily detected by all the tests for that substance. When evaporated to dryness, uterine milk leaves a brownish brittle residue, the colour of which depends very greatly upon the amount of heat which has been employed in the process of evaporation. If the dried residue be pulverized and treated with boiling ether, this dissolves out a certain amount of fat.

The amount of solid matter present in the uterine milk of the cow appears to vary from 9·63 to 11·65 per cent. In two analyses of the uterine milk of the ewe, I found the solids to amount to 11·70 and 8·12 in 100 parts.

The quantity of fatty matter in one specimen from the cow was 1·23, in another 1·40 per cent. In two specimens of the uterine milk of the ewe, I found the quantity to be 1·05 and 1·20 in 100 parts.

If the dried residue, which has been treated with ether, be burned in a crucible at a red heat, a very small amount of ash will be left. This I found to amount to ·37 and ·48 in 100 parts of the uterine milk of the cow, to ·47 and ·82 in that of the ewe. The quantity of the ash is so small, and my time has been so fully occupied, as not to enable me to examine it carefully.

Different portions of uterine milk were repeatedly tested for sugar, but none was found.

In order to determine the presence or absence of casein and albumen, I followed the following processes:

I diluted some uterine milk with an equal bulk of water, and boiled it; an abundant coagulum was formed. On attempting to filter the fluid so as to separate it completely from the coagulum, the process was soon arrested by the clogging of the filter. I therefore added a few drops of acetic acid to another specimen of diluted uterine milk, when, after boiling, I found that the precipitate separated quite readily, and admitted of rapid filtration. Other specimens of uterine milk were precipitated by nitric acid and the other reagents which indicate the presence of albumen. Having diluted a considerable quantity of uterine milk, I added a solution of chloride of ammonium, in amount more than sufficient to render the fluid neutral, I then boiled it for a few minutes, and filtered. The filtrate, which was quite clear, was divided into several portions. To one I added acetic acid; it caused a curdy precipitate. To another I added a solution of chloride of calcium; no precipitate occurred. To another portion I added a solution of sulphate of magnesia; the fluid remained clear. A precipitate was, however, formed when the fluid was boiled.

The above reactions would formerly have been considered ample proofs of the presence of casein, especially when taken in connexion with the film which formed on heating the fluid. We are now aware, however, that albumen, if held in solution by potash and soda (the so-called albuminates of potash and soda), reacts in all these particulars like casein. I therefore added to another portion of the clear filtrate some rennet, and placed it over a fireplace, where the temperature was about 100° Fah. No coagulum formed. I repeated all the above experiments several times, with precisely the same results. They prove that uterine milk contains no casein, but that some of its albumen is held in solution by alkalies. In my quantitative analysis, I estimated separately the albumen precipitated by heat and that precipitated by acetic acid.

On the Quantitative Analysis of Uterine Milk.—1. A quantity of the fluid, varying from 200 to 600 grs., was evaporated to dryness in a porcelain capsule. The evaporation was carried on first in the hot-water oven, secondly in the hot-air oven, at a temperature of 220° Fah.; and lastly, the residue was allowed to cool in a bell-jar over sulphuric acid, and then weighed. By this means the amount of *water* and *solids* was ascertained.

2. The dry residue of (1) was pulverized and treated with successive portions of boiling ether. The ethereal solution was allowed to evaporate spontaneously, then heated gently in the water-oven, and weighed. Thus was found the amount of *fatty matters.*

3. The residue of (2) was incinerated until the organic matter was quite burned away, leaving a white ash, which was weighed. Thus was obtained the quantity of the *inorganic salts.*

4. About 300 grs. of uterine milk were accurately weighed and diluted with an equal bulk of water, and a considerable quantity of a saturated solution of chloride of ammonium was added. The liquid was then boiled for some time. The coagulum was then collected in a counterpoised filter, and dried in the water-oven. It was then cut into very small pieces with a knife, and these were further dried in the hot-air oven, and lastly weighed. The weight, minus that of the filter-paper, gave the amount of *albumen,* which carried down with it the cellular structures present in the fluid.

5. To the filtrate obtained in operation 4, acetic acid was added; the curdy precipitate was collected on a counterpoised filter, and the drying and weighing carried on as described above.

This plan of analysis, it will be observed, is not an absolutely accurate one. In the first place, the albumen cannot be obtained free from the cellular elements present in the fluid obtained from the cotyledons. Secondly, in order to save time, the precipitate of albumen was not incinerated after being dried, so that there must be a slight error in excess in the amount of albumen.

Analysis of Uterine Milk of the Cow, No. 1.—The amount which could be obtained from the cotyledons was not ascertained. The reaction of the fluid was slightly alkaline. The density at 60° Fah. was 1033.

Water in 1000 parts	879·10
Solids	120·90
Albumen with cellular structures	104·00
Alkaline albuminates	1·60
Fat	12·33
Inorganic salts	3·74

Note.—The amount of solids ascertained separately exceed by ·77 the aggregate solids determined by evaporating the fluid.

Analysis of Uterine Milk of the Cow, No. 2.—This specimen was obtained from the cotyledons of a cow at an advanced period of gestation. The following observations were made upon the number and weight of the cotyledons, and upon the quantity of the fluid yielded by 1000 grs. of the maternal portion of the cotyledons. The cotyledons were 90 in number.

	Grains.
The aggregate weight of the cotyledons was	44,780
„ maternal portions of the cotyledons weighed	20,090
„ fœtal „ „ „	24,690
„ largest cotyledon	1,315
„ smallest	105·5
„ quantity of uterine milk which could be squeezed from 1000 grains of the maternal portion of the cotyledon was	149·8
Quantity of cotyledonary fluid, which could at this rate be expressed from the whole of the maternal portions of the cotyledons, estimated at	3009·48

The following are the results of the analysis of this specimen:

Specific gravity at 60° Fah.	1040
Reaction	neutral
Water in 1000 parts	861·2
Solids	138·8
Albumen and cellular structures	116·5
Alkaline albuminates	3·3
Fat	14·0
Salts	4·0
Loss	1·0

Having estimated the amount of the fluid which was yielded by a given weight of the cotyledons, and having calculated from this the amount that could have been expressed from the whole of the maternal portions of the cotyledons, we can calculate how much of the separate constituents would be found in the whole quantity of the uterine milk.

	Grains.
Water in 3009·48 grains of uterine milk	2591·74
Solids	417·74
Albumen and cellular structures	350·60
Alkaline albuminates	9·93
Fat	42·13
Salts	12·00

Analysis of Uterine Milk of Ewe, No. 1—Reaction Alkaline.

Specific gravity	1031
Water in 100 parts	88·30
Solids	11·70
Fat	1·20
Albumen with cellular structures	9·50
Alkaline albuminates	·47
Salts	·45

The uterus in this case contained a lamb weighing 9½ oz.

Analysis of Uterine Milk of Ewe No. 2.

Weight of uterus and membranes	2 lbs. 14 ozs.
„ of lamb contained	1 „ 3 „
Uterine milk expressed from the maternal portions of all the cotyledons	544·5 grs.
Specific gravity of uterine milk	1033
Water in 100 parts	91·88
Solids	8·12
Fat	1·05
Albumen, with cellular structures and alkaline albuminates	6·12
Salts	·82

In this case a considerable quantity of mucus existed between the chorion and mucous membrane of the uterus, external to the cotyledons.

Having given the results of my researches on the composition of the so-called uterine milk of ruminants, I would direct attention to the fact that in other orders of animals a fluid is found between the chorion and uterine mucous membrane, which in physical characters appears to resemble it. In the sow I have found such a fluid. In one case, I collected a considerable quantity of a milky fluid (737·5 grs), which was situated between the mucous membrane of the uterus and the chorion. It had an alkaline reaction; its density was 1017; it was highly coagulable, and contained fat. As it was beginning to decompose, I did not make a quantitative analysis of it. My brother has also informed me that in his dissections of the uterus of pregnant mares he has noticed a considerable quantity of a creamy fluid between the chorion and the uterine mucous membrane.

Without wishing to draw rash conclusions from the few facts which I have adduced, I would remark that they appear to be of some importance; that they seem to bear out the views of those who believe that the placenta contains arrangements which separate those constituents of the blood which are requisite for the nutrition of the fœtus, and that they give support to those anatomists who attribute such a function to the external and internal cells of the villi of the human placenta.* It is a subject in which, however, further researches are much wanted, and to which I hope to revert, when I shall have had opportunities and leisure to study more fully the chemical composition, and especially the microscopic character of the interesting fluid which forms the subject of this paper. For the convenience of reference, I have annexed a table in which the results of analysis of uterine milk, made by Schlossberger and myself, may be seen and compared. To be more complete, I have added below the results of Prevost and Morin's analysis.

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* See Professor Goodsir's Paper On the Structure of the Human Placenta, in his Anatomical and Pathological Observations. Edinburgh, 1845.

Tabular View of Analyses of Uterine Milk.

	Schlossberger.		Arthur Gamgee.			
	I.	II.	III.	IV.	V.	VI.
Reaction	Acid	Acid	Alkaline	Neutral	Alkaline	Alkaline
Specific gravity	—	—	1033	1040	1031	1033
Water	88·07	88·03	87·91	86·12	88·30	91·88
Solids	11·93	11·97	12·09	13·88	11·70	8·12
Fat	1·59	1·52	1·23	1·40	1·20	1·05
Albumen with cellular structures	9·63	9·57	10·40	11·65	9·50	6·12
Alkaline albuminates	—	—	0·16	·33	·47	—
Salts	0·71	0·70	0·37	·40	·45	·82
	Of the Cow.				Of the Ewe.	

Analysis of the Uterine Milk of the Cow by Prevost and Morin.

Water in 100 parts	86·837
Solids	13·163
Albumen, fibrin, &c.	11·028
Gelatiniform matter	0·546
Osmazome	0·714
Fat	0·750
Salts undetermined.	

(*To be concluded.*)

ART. II.

Remarks on some Cases of Vascular Tumour seated in Muscle. By CAMPBELL DE MORGAN, F.R.S., Surgeon to the Middlesex Hospital.

THE records of surgery and pathology relating to vascular tumours seated in the muscles are, so far as I have discovered, but few and meagre. The disease, unless as an extension from the cutaneous or subcutaneous vascular growths, is either very uncommon or has not attracted the attention of surgeons, for the cases mentioned in this communication are all that I have seen myself or found described by others.

M. Demarquay, in a paper published in 'L'Union Médicale,' maintains that these tumours are not rare, and expresses surprise that surgeons have not oftener met with them. The reason why they have not been met with undoubtedly is, that they are amongst the rarest of tumours, especially that form of them in which they are surrounded by a fibrous sheath. M. Demarquay himself only brings forward six cases which he has been able to collect. Mr. Teevan, in his article on tumours in muscle, published in the last number of the 'Medico-

Chirurgical Review,' mentions only five, one of which is Demarquay's case, and two of them are amongst those collected by him, while two others are examples of the extension of the disease from subcutaneous nævus. With the exception of Liston's case, and one brought to the Pathological Society by Dr. Buchanan, both of which will be presently alluded to, I know of none hitherto described in this country, and there is not a single specimen of the disease in the Museum of the College of Surgeons. In the Museum of St. Bartholomew's Hospital is one preparation marked erectile tumour in muscle. Mr. Callender was good enough to have it examined for me, and it proved to be one of erectile growth spreading out into the muscular tissue, not separated by an areolar capsule. Elsewhere I am not aware of any preparations illustrating this form of tumour. Surely, then, these growths must be extremely rare: it is very little likely that they should have been often found, but neither described nor preserved.

The following observations may then, perhaps, attract attention to a class of tumour which presents many points of interest in itself, as well as in relation to the general question of the development of abnormal growths. Into this latter question, however, I do not propose to enter.

Ruth Edwards, a healthy girl of ten years of age, was sent to the hospital by Mr. Yate, of Godalming, to be treated for a tumour in the left leg. It was situated in the calf, its upper border lying just below the popliteal space, and was about the size and form of a small hen's egg. It was not movable upon the surrounding parts, but seemed bound down beneath the fascia. After standing or walking for a short time she suffered pain in the part, and pain was brought on by handling the tumour, but there was no tenderness. When she lay down, the swelling became much less prominent, and its boundaries could not be readily defined; it became still more indistinct if while she was lying down the leg was raised or the tumour kneaded and pressed upon, or if the leg was flexed; indeed, in this position it was not easy to make out the tumour at all. When she was standing up, the tumour was very prominent and tense, and its limits were clearly marked. The leg then measured 10⅛ inches in circumference, and was ⅝ of an inch more than the other limb. On her lying down it was reduced to 9⅞ inches, and after pressing on the tumour or raising the leg, to 9⅜ inches. No pulsation could be detected under any circumstances, nor was there any thrill or bruit distinguishable on auscultation. The anterior and posterior tibial arteries pulsated normally; there was nothing unusual in the condition of the skin, and the temperature of the part was not raised.

The only history that could be got was that her mother told her she had had a lump in the leg from the time of her birth; that she had run about and gone to school with little or no inconvenience until within the last few months, when the swelling became larger and more painful; and that she was in consequence unable to go to school or do any work.

Taking all the circumstances into account, the only conclusion seemed to be, that this was a vascular or erectile tumour, unconnected with the skin or subcutaneous tissue, but lying beneath the fascia, and perhaps in the muscular tissue. Its clearly-defined borders indicated that it was not a mass of erectile tissue imbedded in and merging into the muscular structure, but that it had its own proper boundary. That it was not connected with any large arterial trunk was shown by the entire absence of sound or thrill, and by the natural beat of the arteries below it.

She was admitted on the 10th of February, 1863, and the tumour was removed on the 18th of February. A vertical incision three inches long was made directly over the long axis of the tumour. On slitting up the fascia to the same extent, the tumour was seen covered by an extremely thin layer of the fibres of the gastrocnemius. This was dissected off, and the body of the tumour was then readily enucleated. At its upper end it tapered off into a fibrous cord, which ran up to the popliteal space. This cord was traced up for some little distance, and then cut across. A somewhat similar mode of termination existed at the lower end. There was no hæmorrhage during any part of the operation. The wound was closed by metallic sutures, and united for the most part by first intention. The patient was discharged on the 3rd of March.

My colleague, Mr. Hulke, was kind enough to make a careful examination of the tumour, and has given me the following account of it: "The tumour is of a flattened oval shape, two inches by one in diameter; the outer, less convex than the inner surface, has some shreds of muscular fibre adherent to it. A section in the long axis of the tumour shows patches of cavernous, interspersed with yellowish-fatty and greyish-fibrous, tissue, amongst which are some elastic fibres. In accordance with this difference of composition, the density of the tumour varies from sponginess to great firmness. The tougher greyish parts, which to the unaided eye appear to be fibrous, are found to contain much fatty and a small quantity of muscular tissue, particularly near the outer surface of the tumour. The primitive muscular fibres are scattered and separated by adipose tissue, and from the indistinctness of their markings and granular opacity are evidently undergoing a process of atrophy. The cavernous portions consist of cavities of various sizes, the largest about the size of a No. 4 shot, separated by partitions of fibrous tissue, with elongated nuclei. Some of the cavities contain coagula of various ages; others are empty, and some receive buds from the fibrous partitions or meshwork; these have rounded ends, and appear to be outgrowths. The cavities or sinuses have no proper walls or coats, but are interspaces in the partition or framework. I could not recognise any structures like those proper to arteries or veins of the same dimensions. The cavernous tissue is not, then, a simple hypertrophy of the vessels of the part in which the tumour is situated, but is a new formation."

It may be mentioned that numerous phleboliths about the size of small pins' heads were found imbedded in different parts of the tumour. This is noteworthy, as Cruveilhier states that, except in one case, which also involved the muscular tissue, he never saw phleboliths in accidental erectile tissue, though they are frequently found in true varices.

The diagnosis in this case was not difficult. When the parts were relaxed, the characters were those of a soft fatty tumour. When the patient stood up and the parts were tense, the tumour might have been taken for a tense thick-walled cyst; but the fact of its being congenital, the varying size in different positions of the limb, and the readiness with which it was partially emptied when pressed upon, and refilled when the pressure was taken away, while no enlargement took place elsewhere when its contents were forced out, were conclusive indications enough of the real nature of the tumour. The absence of all pulsation or thrill showed that it was not directly connected with the large arteries. The preparation is in the museum of the hospital.

Some years ago I removed a similar tumour from the thigh of a middle-aged woman. For many years she had had, at times, inconvenience and dull pain. The pain had latterly become so wearing that she applied for relief at the hospital. There was found to be a swelling lying very deep amongst the muscles of the thigh, so deep that it was impossible to make out its characters, especially as the thigh was fleshy and well covered with fat. It did not appear larger than a small walnut. It was situated about the middle of the front of the thigh. She was very anxious to have it removed, and although its nature was obscure the operation was performed. In order to reach it, it was necessary to turn aside the sartorius, and to cut through the fibres of the rectus, in the posterior part of which it was imbedded. There was no hæmorrhage. It proved to be a small tumour of mixed erectile and fibrous tissue, with a fibrous investment separating it from the muscle in which it lay. The specimen is in the museum of the Middlesex Hospital. It has been recently examined, and as in the other tumour, muscular fibres, more or less degenerated, have been discovered in its tissue.

These are instances of a class of cases which has not, it would seem, been often met with. The first case of which I can find a record is that described by Mr. Liston, in the twenty-sixth volume of the 'Medico-Chirurgical Transactions,' and in almost every respect it resembles the first-mentioned one, the only point of difference being that in Mr. Liston's case, distinct pulsation was stated to have existed at one period, before it came under his care. Mr. Liston remarks, that "erectile tissue is not often met with in parts deeply placed," and that "he is not aware of its having been previously found in muscular substance."

Demarquay, in his paper in 'L'Union Médicale,' describes an erectile tumour in the supinator longus, which was removed from a woman twenty-eight years of age. The tumour had been only noticed for nine years, but it was then as large as at the time of operation. It was removed in consequence of the severe pain which the patient suffered. At first this was slight, and merely shot occasionally down the arm; but as time went on it became more severe and frequent, and radiated up to the shoulder and neck.

He refers to other cases in which similar tumours had been found. One developed in the posterior muscles of the neck was removed by Maisonneuve; one in the rectus of the thigh by Denonvilliers. He cites, also, Berard, as having seen a woman who had an erectile tumour in a muscle of the inferior and external part of the thigh.

Dupuytren says that erectile tumours are found in the thickness of the muscles, but he gives no example of the disease limited to that situation. The cases mentioned by him are those in which the structure has extended from the skin or subcutaneous tissue to the deeper parts. This is a condition which is by no means infrequent. In the remarkable case figured and described by Cruveilhier (25 Livraison, Planche 5), the relations of the tissue to the neighbouring parts were altogether different from those which existed in the cases now mentioned. The limb was a paralysed one, and it was the seat of numerous varices, extending from the shoulder to the hand. After death, it was found that, besides these varicose tumours, which abounded in and below the skin, and which in a few places were converted into a sort of erectile tissue, the biceps and the muscles of the forearm were almost entirely replaced by this tissue, with here and there small varices. The situations of the separate flexor muscles of the forearm, indeed, were no longer recognisable. Nothing was found but one mass of erectile tissue.

This form of degeneration, although in Cruveilhier's case carried to a perhaps unparalleled extent, is less uncommon than that in which the erectile tissue forms an isolated tumour imbedded in the muscle. The previous history of this case is unrecorded, and there is no evidence that all this amount of disease had a congenital origin.

A very good example of this form of degeneration of the muscular tissue occurred recently in the practice of my colleague, Mr. Shaw, who has kindly furnished me with notes of the case:

"A healthy girl, aged seventeen, was admitted into the Middlesex Hospital, on the 14th April, 1863, for a tumour situated in the infra-axillary region of the right side. Its form is irregularly circular, the greatest diameter being four inches. The average height near the centre is an inch and a half. At the margins, it is gradually lost in the adjacent structures. The skin is healthy and quite movable. It is indistinctly lobular and elastic, and much resembles a fatty tumour. The consistence of the mass is distinctly denser in the centre than at the periphery; and it appears fixed at its base. On alternately compressing the tumour and ceasing the pressure, it could not be satisfactorily ascertained whether the size varied.

"The patient's mother observed the growth eight days after birth. It was then of the size of a hazel-nut. The increase of its dimensions has been slow. When she has had a hard day's work, the tumour and surrounding parts have become temporarily enlarged. She has never had any pain in it.

"The tumour was removed on the 29th April. On reflecting the skin, its surface came into view, covered with an imperfect envelope of fibrous tissue. At numerous points, what appeared to be thin cysts, containing, some of them, fluid, and others coagulated blood of a dark colour, protruded through the investing membrane. These were considered to be insulated loops of tortuous veins. From studding the face of the tumour throughout, these black-currant-like cysts served, rather than the ill-defined envelope, to mark the extent to which the morbid structure reached. In freeing the tumour around, its substance was found blended with the edge of the latissimus dorsi and fibres of the serratus magnus. For the length of about an inch, the under surface adhered to the seventh rib; and over an equal area it was fused into the intercostal muscle, between that and the sixth rib. Hence the separation could be effected only by cutting through the morbid mass. While doing so, blood flowed profusely from a cellular cavernous structure, resembling the corpus cavernosum penis; and it continued to ooze afterwards for several minutes, rather freely, from the numerous pores of the remains of the tumour left in the rib and the intercostal muscle. The bleeding subsided, however, without the use of styptic, so as to admit of the wound being stitched and the further flow restrained by a thick compress."

The following memorandum of the minute structure of the tumour was furnished me by Mr. Hulke:

"A flattened oblong mass, 2½ inches, by 1¾ inch, and ¾ of an inch thick. The outer surface is overlaid by a coat of adipose tissue. The deep surface is ragged, presenting shreds of muscle, with patches of spongy vascular tissue. Beneath the fatty coat, the outer surface exhibits blue-black globular beads, 1 to 1½ lines in diameter, which are blood-clots in dilated sinuses. The walls of the sinuses are *lined* by an *epithelium* (?), and formed of connective tissue. In the thicker partitions between neighbouring sinuses, yellow elastic tissue occurs plentifully. Where the muscular blends with the cavernous tissue, its transverse markings are obscure, or altogether lost; and the bundles of muscular fibres are separated by the cavernous tissue. Amongst the cavernous, are patches of tough fibrous tissue, where the former are become obliterated. These contain both connective and elastic tissues."

The case mentioned by Mr. Coote, in a lecture on nævus, in the forty-fifth volume of the 'Medical Gazette,' appears to be one of varicose veins or blood cysts imbedded in muscle. The tumour was supposed to be of a fatty nature, overlying the deltoïd, and its removal determined on with that view. But, on cutting down to the muscle, no tumour was found; and on continuing the dissection through the deltoïd, "a vascular mass was discovered imbedded in its substance; the bleeding was inconsiderable and for the most part venous; and the morbid structure was readily removed." The patient was a little girl, and the swelling was congenital. This tumour would appear, from the description, to be like the varicose tumours seen in Cruveilhier's case, studding the skin and muscle.

There are, then, three forms under which vascular tumours are found in muscle:—1. Tumours having the appearance of a varicose state of the veins, unconnected with the muscular tissue in which they lie, but yet not isolated by any well-defined covering of connective tissue.

2. Erectile tissue, forming in, and encroaching on, the muscular tissue, from which it is not separated by any defined boundary. 3. Erectile tissue, forming a defined tumour, having an investment of connective tissue, not continuous with the muscular tissue in which it is imbedded.

It is very probable that these different forms have a common mode of origin. They have nothing in common with the ordinary varicose veins of the leg, which are produced by obstruction to the circulation or weakness of the veins. On the contrary, they are of the nature of morbid growths, with characters as marked as are those of cancer. This is sufficiently evident from what is observed in ordinary cutaneous nævus. It may remain quiescent, then start into new growth, attacking the surrounding tissue; may spread amongst the deeper structures, so as to disorganize them through the whole thickness of the region involved; or it may be arrested again in its growth, and ultimately become condensed into a harmless mass of connective tissue. So long as a single piece of the original disease remains, there is risk of its spreading. Nothing is known as to the causes which determine the seat of the disease in one or other of the vascular tissues. Sometimes, it is said, the arterial portion of the vascular tissue is principally affected; sometimes, and perhaps more usually in tumours, the venous; while, probably, in all cases there is, as well, some affection of the capillaries.

The disease is most frequently congenital; but many cases are on record, such as those related by John Bell, Warren, and others, in which, so far as could be ascertained, it came on in later life, after injury; and some in which, as in a case to be presently referred to, no cause could be assigned. John Bell, indeed, to whom we are indebted for the first recognition, and for the most graphic account of these diseases, considers that they frequently occur from mere external irritation—the pressure of the hat, for example. Seeing, however, that a small spot, so insignificant as to be quite disregarded, will often prove the focus of a formidable vascular tumour, it is not unreasonable to conclude that in many of the cases on which he formed his opinion there had existed some such little mass of abnormal tissue, ready, when irritated, to start into active growth. In the cases of defined, deep-seated tumours of this nature, it is probable that there is always a congenital origin, but they are far too few to justify any positive conclusion on the subject. The situations in which these vascular tumours imbedded in muscle are found deserve consideration. The cutaneous and subcutaneous forms of nævus are, as is well known, developed most frequently in the upper parts of the body—on the face and neck especially—very rarely on the lower extremities. On the other hand, out of the seven cases here mentioned of isolated tumour in the muscle, five were seated in the lower extremity.

The early condition of vascular tumours has not yet been satisfactorily investigated, and very little has hitherto been done towards determining their mode of growth or their true structure. The fullest account of the structure of the cutaneous and subcutaneous forms of the disease is to be found in Mr. Paget's Lectures. Of the deep-seated tumours involving the muscle or lying imbedded in it, scarcely anything has been made out; and the investigations of Mr. Hulke into the structure of two of these tumours are of great value. They tend to show that the origin of both forms—those which spread in the muscular tissue and those which grow in it as isolated humours—is the same. The presence in both of the same elements, the elastic and the muscular tissue, leads to this conclusion. The difference which they present may be the result possibly of a greater or less rapidity of growth; in the one case the growth may be very slow and thickening, and new deposition of connective tissue will take place to a greater degree than the extension of the disease of the vascular element of the tumour, so as gradually to isolate it more and more from the surrounding muscular structure; in the other, the vascular growth will outstrip that of the fibrous tissue, and will consequently encroach more and more on the muscle. The presence of the muscular fibre within the isolated tumour becomes then an important and significant fact. It is true that in Mr. Liston's case no mention is made of the presence or absence of muscular fibre, but its existence might very easily be overlooked unless every part of the tumour were examined; and there is no doubt but that although present in the early stage, it might become entirely degenerated. In Dr. Buchanan's case muscular fibre was discovered in the walls of the tumour; but the exact nature of the tumour was not determined, though I think that now there can be little doubt that it was a modified erectile tumour seated in muscle. Since his case was published, the presence of muscular fibre in such tumours has remained as an isolated fact.

I should conclude, then, that the disease first involved the vascular elements connected with the muscle-tissue, just as the cutaneous nævus involves those of the skin. It may be that there is from the first some abnormal condition of the vessels, or perhaps there is an altered mode of growth in parts hitherto properly developed. The fact, if such it be, that true erectile and other vascular tumours may come on from injury, irritation, or even without assignable cause, in parts previously healthy, would render the latter view as probable as the former.

Once present, the disease may spread indefinitely, or it may remain stationary; it may, as before said, continue to involve the neighbouring parts, or it may become separated from them by a fibrous investment. In either case the changes in the diseased part are of the same nature, though they may differ in degree. The muscular fibre, compressed by the hypertrophied vascular and connective tissue, and its nutrition otherwise impaired by the altered and thickened state of the capillaries, degenerates in whole or in part, leaving here and there traces of its presence, but becoming for the most part converted into fat. The connective-tissue elements at the same time increase in amount, and in some places condense themselves into masses or bands, which, if they form at the circumference of the diseased mass, constitute a fibrous sheath, which shuts it off from the healthy parts around. The abundant amount of the yellow elastic tissue seems to indicate that a new formation of this element takes place. What the change may be which occurs in the vascular tissue itself is not clear, nor can it be made out so long as we are in ignorance of the original condition of the vessels. Is there at first simple increase in the amount and size of the proper vessels? or are there lacunæ left in the growing tissue with which the vessels communicate? Mr. Paget inclines to the former view, and supposes that the walls of the mutually apposed vessels ultimately thin and give way, forming blood-spaces. This, probably, is the case in the active arterial vascular growths. In the tissue of the tumours now described there is no evidence of such a transition from simple increase of the vessels to the formation of blood-spaces, no enlarged or tortuous vessels, and no lining to the cavities. The blood is not driven into the tumour through one or two large and active vessels creating pressure on its contents and walls such as is seen in an aneurysm; on the contrary, the most remarkable point in the operation of removal of these tumours is the entire absence of hæmorrhage from any vessel feeding them. If cut into, no doubt the bleeding from them would be very abundant, but the supply of blood is evidently derived from the general vascularity in the neighbourhood, and no single vessel is large enough to give rise to hæmorrhage. It is possible still that the presence of blood-spaces may be due to pressure and absorption, but the mere physical cause of their formation is of secondary importance. The essence of the disease is the new condition of growth, which, having its starting-point perhaps in some abnormally developed tissue, determines a similar condition in all parts within its reach. Hence, the term tumour may fairly be applied to all these growths, whether they become in the end isolated by the formation of an investing capsule of cellular tissue, or spread without limitation in the plain of a muscle or other tissue which they involve. They are not mere hypertrophies—their whole history is opposed to that view—they are of the nature of new growths, possessing their own peculiar and distinctive character. In using this term it is not of course implied that the structure possesses any new or peculiar elementary tissue which could not be discovered in some form or under some arrangement in the normal structure; so limited, perhaps, there is in reality no such thing as new growth, except in the case of parasites. Nor is it implied that there is such a wide deviation from the normal structure as is found in true cancer; but, at any rate, there is as good reason for classing these diseases amongst new growths as there is for placing adenoïd or cystic tumours in that category. The structures may be histologically the same, but their mode of development and the relative amounts and general arrangement of the elements which enter into them differ so greatly from what is seen in the normal condition of the parts involved that they may fairly be considered as tumours. As in Cruveilhier's case, already mentioned, there is at times a disposition to the development of the disease in a number of different places, reminding one of the tendencies of cancer, and this is more remarkably seen in another instance figured by him (Livraison 27, Planches 3 and 4), where the hand and arm were studded with innumerable small tumours, varying in size from that of a small shot to that of a large bean, lying, some in the skin, some under it, and some below the fasciæ and in the midst of the muscles. They all consisted of erectile tissue. Cruveilhier seems, indeed, to consider that there is a relation between erectile tumour and cancer, and that in their stroma the two are much alike, their differences being that in one case the spaces are filled with blood, and in the other with cells. Though one may be indisposed to go this length, yet it is a fact which has been long recognised, that of all forms of abnormal growth none are more prone to degenerate into cancer than those which fall generally under the name of vascular tumours. I may here mention another remarkable case of this kind, in which there appeared little doubt as to the tumours, numerous as they were, having a congenital origin. A man, aged sixty-three, was admitted into the medical wards of the Middlesex Hospital, in 1854, for hæmatemesis. He presented over many parts of the body venous nævi—some in the skin, some subcutaneous. They existed in large numbers on the forehead, face, and neck; in the latter situation they formed a large mass on each side. Similar tumours were found studding the shoulders, arms, hands, and legs. Some of these appeared to be dilated veins simply—some had merely the characters of mother's marks. None of them were much affected by steady pressure. On the left side of the tongue was a large vascular tumour. He said that he had had them all for as long as he could remember. This patient was admitted again in 1855 for hæmorrhage from the urethra, caused by the abrasion of a small vascular tumour in that situation. His general health had always been very good.

The diagnosis of these vascular tumours is not always so easy as is generally supposed. In the first case, that of Ruth Edwards, there was no difficulty; the emptying and filling of the tumour under the circumstances described, left no room for doubt as to its true nature, but in the second case such a diagnosis was impossible; the tumour was so small and so deeply seated that the changes of bulk which might have taken place in it could not be recognised. In Mr. Shaw's case, again, there were no characters in the tumour itself by which its nature could be determined; the fact of its having existed from the time of birth was the great ground for regarding it as possibly a deep-seated nævus.

A somewhat curious case of blood-tumour came under my care a short time since, in which, though it was subcutaneous, none of the diagnostic signs of such tumours were present. A man, aged forty-one, came to the Middlesex Hospital to have an operation done for the removal of some necrosed bone in the radius. He pointed out a small tumour on the other forearm, which had never given him any trouble, but which he thought might as well be removed at the same time. The tumour was about the size of a large marble; it rolled freely under the skin, which presented a network of fine veins on its surface. It was suspected to be a vascular tumour; but neither pressure upon it, nor upon the veins above it, nor change in the position of the arm, made any alteration whatever in its size and consistence, which was that of a somewhat condensed fatty tumour. Hence it was believed to be really a simple fatty tumour, and the more so, as the man was positive in his statement that it had not existed for more than ten years, and that when first seen it was not so large as a very small pea. I therefore cut down directly upon it and into it, when its true nature was at once seen. The texture looked like that of spleen, and the blood oozed from it as if squeezed from a sponge. It was very readily turned out, however, and there was a good deal of venous bleeding, which was easily stopped by a compress. The wound healed by the first intention.

For an examination of the structure of the tumour I am again indebted to Mr. Hulke, who reports that "it is composed of cavernous tissue, and does not contain any muscular element, neither did I meet with any yellow elastic tissue. The cavernous structure was enclosed in a capsule of connective tissue, and its connexion with the veins outside was not obvious." The specimen is in the museum of the hospital.

The term erectile is not then universally applicable to vascular tumours. Here, for example, we find a growth which was not undergoing any process of degeneration, and was as active probably as at any period of its existence, yet was totally devoid of any recognisable erectile character.

Neither probably could the term be applied to the first form of deep-seated blood-tumour which I have mentioned, in which, to all appearance, there is a simple varicose state of the veins. Still less is known of the real nature of these than of the other forms. Probably they are not in reality mere dilatations of the veins, but are true blood-cysts, such as exist sometimes as secondary formations in the erectile tumours, and sometimes are found as isolated tumours, unconnected with any venous trunks. Of such a nature was the blood-cyst before alluded to, a report of which, by Dr. Buchanan, appears in the eighth volume of the Pathological Society's Transactions, p. 363. It was situated in the gastrocnemius muscle, which, in fact, "was converted into a cyst that contained about ten ounces of viscid fluid, homogeneous, and of a brownish-red colour, not unlike bile." There were also a few loose shreds, the remains apparently of old coagulum. There was very little of the muscular tissue of the gastrocnemius left. The walls of the cyst were formed of tough membrane, which appeared composed of condensed and altered muscular tissue. On microscopical examination, the walls presented little beyond fat, though in some parts muscular fibres, with the transverse striæ hardly recognisable, were to be seen. The tumour was only discovered after death, and no previous history could be obtained.

I cannot help suspecting that the following case had its origin in some form of cutaneous nævus:

Some years ago I removed a tumour about the size of a large orange from the back of a gentleman where it had been growing for eight years, and who wanted to get rid of it merely because it got chafed at times. It was situated over the left scapula. It was somewhat dark-coloured and rather flaccid: it could not be diminished in size by pressure; no vessels could be seen running upon or near it.

It was clearly a cyst with thin walls, and I removed it by elliptical incisions round its base. I was certainly astonished at the active hæmorrhage which took place from five or six arteries—the blood spirted out round the tumour as if from the rose of a watering-pot. As soon as the tumour was removed all the bleeding stopped, and we could not find any vessel to take up. On cutting through the tumour it was found to contain nothing but blood, with smooth, thin, old fibrous layers lining the walls, which were otherwise not thicker than ordinary cardboard. The base was more solid, but nothing could be found in it save condensed fibrous tissue. No vessel could be discovered entering into the tumour. The wound made for its removal healed almost entirely by the first intention, and there has not been any return of it. It may be questioned, of course, whether this was truly a blood-cyst, and not an ordinary cyst which, from friction or injury, had had blood effused into it; but the remarkable vascular activity in the skin around it, which ceased so rapidly on its removal, indicates that its original connexion was with the bloodvessels. The case is reported by Mr. Flower, and a drawing of it given in the eleventh volume of the Pathological Society's Transactions, p. 237. The preparation is in the Museum of the Middlesex Hospital.

In the same Museum is also a preparation presented to it by Mr. Hulke, of a vascular tumour which he removed after death from the neck of an infant a few months old. It had been considered during life to be a deep-seated venous nævus. It was covered by the trapezius, which adhered intimately to it, and by the sterno-mastoïdeus. Reaching from the occiput to the upper border of the scapula, and passing forwards under the sterno-mastoïdeus, it communicated by a short wide trunk with the internal jugular vein. It has an irregular tuberous form, and consists of freely communicating blood-spaces varying from the size of a large filbert downwards. These are lined by a smooth, continuous membrane, which structurally resembles the inner coat of a large vein, and is distinctly separable from the septa, which imperfectly divide the spaces, and which consist of connective and elastic tissues. No muscular tissue exists in these septa. This latter point, which Mr. Hulke has clearly made out, and the fact of the free communication with the internal jugular, indicate that the tumour was not primarily seated in muscle, though its close investment by the muscular fibre seemed to point to such an origin. It is, however, a remarkably fine specimen of the cystic vascular tumour.

In Cruveilhier's plates, and especially in the one before referred to, these blood-cysts are seen in various situations and states. In some places they appear to be undergoing a change towards the erectile form; in some they lie on, and are connected with, large venous trunks; in others they are unconnected with any large vessel, and lie as isolated masses in the midst of the cutaneous and muscular tissues. Here and there are small single cysts; at other parts they form clusters like berries.

The "black-currant-like cysts" observed in Mr. Shaw's case were probably of the same nature.

These formations, however, require investigation as to their mode of origin and growth, and their connexion with the other forms of blood-tumour.

The terms erectile tumour and blood or vascular tumour have been used in preference to the more lofty-sounding one of "telangeiectasis," inasmuch as they are far more easy to read and write, and as they convey quite as correct a notion of the character of the disease. The term erectile simply points to a fact that these tumours under certain circumstances become turgid, and again empty themselves: it does not imply that they possess the peculiar nervous endowments and vascular arrangement of the normal erectile *tissue*.

ART. III.

On the Phytopathology of the Skin, and Nosophytodermata, the so-called Parasitic Affections of the Skin. By ERASMUS WILSON, F.R.S.

SHORTLY after the first discovery by Remak, in 1836, of the phytiform structure of the yellow matter composing the cups of Favus, and their further illustration by Schoenlein of Zurich in 1839, Gruby of Vienna proposed that the diseases in which this structure had been observed, which were then two in number—namely, Favus and Aptha—should be placed in a group, to be termed Nosophyta, with the distinctive titles, Porrigophyton and Apthophyton. And, subsequently, having discovered a similar organism in mentagra, Gruby further proposed to add that disease to the same group under the name of Mentagrophyton. No better term than Nosophyta has been suggested by any of the phytopathologists who have succeeded Gruby; it expresses the bare scientific fact of the association of a plant-like organism with certain diseases; it commits those who make use of it to no theory; and therefore we adopt it as the title of our present essay.

The Nosophytodermata are five in number, namely:—Favus, *vel* Tinea favosa; Trichosis, *vel* Porrigo tonsurans; Mentagra, *vel* Sycosis; Alopecia areata, *vel* Porrigo decalvans; Chloasma, *vel* Pityriasis versicolor. These diseases form a natural group, which is distinguished by a morbid alteration of the epidermis and hairs, rendering them friable and brittle, changing their appearance and colour, and causing their exfoliation and fall. The epidermis loses its smoothness, the hairs lose their polish; the former becomes spongy and furfuraceous, the latter become faded, discoloured, and shrivelled, and break off close to the skin, suggesting the idea of being cropped by artificial means; hence the terms, tonsurans, tondens, tonsoria, &c., applied to one of these diseases.

Closer observation detects certain differences between them—for example, favus and mentagra affect chiefly the hair-follicles; trichosis and area chiefly the hairs; and chloasma chiefly the epidermis. Favus has the appearance of a yellow flat pustule, often of considerable breadth (two or three lines), surrounding the mouth of the hair-follicle and hair; trichosis is known by the broken state of the hair, which gives an aspect of bareness or seeming baldness to the diseased parts;

mentagra is a prominent pustule with a hard and often elevated base surrounding the mouth of the hair-follicle and hair, and situated, as its name implies, on the chin, or on the face; porrigo decalvans or area is a positive baldness of the affected part, often associated with loss of pigment and atrophy of the skin; while chloasma is known by its situation on the trunk of the body, by an alteration of the pigment of the skin—which has gained for it its name—and by a loose and spongy state of the epidermis, which exfoliates from time to time in the form of a branny or furfuraceous desquamation, pityriasis versicolor, or, as Hardy very appropriately suggests, pityriasis lutea.

The general characters of these affections, therefore, so far as *resemblance* is concerned, are, the seat of the disease—namely, the hair-follicles, the hairs, and the epidermis,—and the consequences of the disease—namely, the disintegration and destruction of the tissues involved, the epithelium of the follicles, the epidermis, and the hairs. The *differences* of a general nature are the limitation in chief of the disease to the follicles, to the hairs, to the epidermis; the intensity of the special affection, which is greatest in favus and least in chloasma; and the complication of the special affection by common inflammation, as occurs in mentagra.

We have next to consider: In what these diseases essentially consist; in other words, their nature and their cause. More than twenty years since,* after a careful microscopic investigation of the pathological elements of which they are composed, we came to the conclusion that they resulted from an aberration of cell-formation; that this aberration consisted in the growth and proliferation of the primary granules of which epidermic cells and hair-cells are normally constituted; that this growth and proliferation had the effect of arresting the granules at their embryonic stage, and in the performance of their embryonic function, and, as a consequence, that the cell-tissue (epidermis and hair) composed of these embryonic granules and imperfectly-elaborated cells, was, upon desiccation by the air, dry, spongy, friable, and brittle; that in trichosis, this change alone existed in the hair and epidermis; whereas, in favus, the granules composing the yellow disk around the mouth of the follicles passed through a pustular stage, the consequence of a higher degree of inflammation, and, in their pustular transition, obtained their peculiar yellow colour. We called this morbid alteration of the primary granules of the epidermic and hair-tissues, "granular degeneration," and subsequent experience has not altered our opinion.

If a portion of the whitish, friable, epidermic, scaly exfoliation of trichosis, chloasma, or favus, or the exuviæ of the diseased follicles, be placed in the field of the microscope, it is found to consist of globular granules mixed up with epidermic scales more or less imperfect and broken. The granules are nucleated, separate or in groups, or, adherent in moniliform strings of two to four or five in length. The greater number of the granules are uninuclear; some, however, are binuclear

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* Diseases of the Skin. 1842. On Ringworm, its Causes, Pathology, and Treatment. 1847.

or trinuclear. The binuclear granule is in process of proliferation, the original nucleus has split into two, each moiety has become a separate nucleus, the space between them has increased, a septum has arisen in the interval, and the cell has become oblong—an oblong cell with two nuclei, or rather, two independent but adherent cells; cells that may subsequently divide and become separate unities. This is the mode of cell-proliferation. At an early stage and in contact with the vital tissue from which they derive their nourishment, and where the nutritive pabulum is abundant, they pass rapidly through the proliferous process, their action is to beget separate granules; but as they become removed from the source of nutrition, proliferation weakens, the divided cells remain adherent, and they either retain their embryonic and globular shape and give rise to moniliform filaments, or they grow in length at the expense of their contents and form cylindrical and transparent filaments divided by septa, each internodial segment representing a single original globular granule or cell. It would seem as if—released from the law which restricted their functions to the construction of a cell, afterwards, by its chemical control over the elements of nutrition, to become a corneous scale—they had lapsed into an irregular and filamentary proliferation, giving rise to an inferior form of organization neither so complete nor so permanent, and which converted the higher animal organization into a lower and phytiform organization—an animal tissue into one which might be likened to a vegetable tissue.

If, now, we take a portion of the yellow paste which constitutes the pus-like disk around the mouth of a hair-follicle in favus, we shall find that it is composed of the granules already described in great abundance; and we shall have an opportunity of observing the proliferation of these granules from their simplest, globular, uninucleated form, through the binuclear and trinuclear forms, to the moniliform and opaque, the cylindrical and transparent, and the branched and ramified filament, until we are led to the conclusion that we have before us no longer an animal organism but a vegetable organism of the fungoïd or mucedinous type. We have described the manner in which the cell with a double nucleus becomes two cells; and if it be imagined that in a string of five this proliferous process take place in all, there would result immediately a filament consisting of ten cells; and if the developmental process be active, these ten cells will become so many cylinders adherent at their ends, and so constituting a filament. In the case of the trinuclear cell a bifurcation is effected, and the proliferation of each part of the trifid cell lays the foundation of a branched and phytiform growth. Favous matter is thus found to be composed of separate granules—granules adherent in moniliform strings; of cylindrical filaments, and of branching filaments; a combination which, out of the body and in the free air, would be regarded as a fungoïd plant.

The precise seat of the pathological alteration here described is the same in all the phytodermic diseases—namely, the deep and growing stratum of the epidermis, that portion which is known as the rete mucosum, and lies in contact with the basement-membrane of the derma;* the position is the same, whether it relate to the follicular or to the epidermic surface of the derma. There is no superficial or deep as respects the seat of the disease; any difference that there may be in depth has reference simply to thickness of the layer of the morbid product. In favus it is thick, because the follicle is highly vascular, and the inflammatory process more energetic, or, more correctly, the constitution of the patient weaker, his vitality less, for favus is usually an accompaniment of the scrofulous diathesis. In trichosis it is thin, because there is an absence of all purulent complication; it is merely the normal thickness of the epithelial lining of the follicle, or of the interfollicular stratum of the epidermis, which is concerned, and the morbid cells exfoliate almost as fast as they are produced. While in chloasma the interfollicular portion of the epidermis, although thin, is thicker and more important than the follicular epithelium, and the morbid exfoliation apparently superficial.

We have remarked that the chief difference between favus and the other forms of phytodermic disease is due to a pustular complication. In fact, we have observed that the layer of the favous cup, which lies in contact with the basement-membrane of the derma, is composed of pus-globules; that these pus-globules contain from four to seven or eight well-formed nuclei; and these nuclei, on their escape from the cell-membrane of the pus-globule, become the nucleated granules which are the chief constituent of the pathological product. That, commonly on their escape from the cell-membrane, the nuclei of the pus-globule are adherent to each other, so as to constitute the moniliform string that we have previously described; and that already, on the first birth of the granules, they are impressed with the type which subsequent development converts into a phytiform tissue. We perceive in this phenomenon an example of the primary elements of the morbid epidermis passing through a transitional change of which the pus-globule is a stage, and at the same time one which raises a doubt as to whether the pus-globule is really the perfected form of that series of developmental changes which it is known to pass through before it reaches that of mature pus. That modification of the elements of composition of the body which gives rise to the special colour of pus is also in operation, and bestows its colour on the peculiar substance of favus.

The peculiar yellow substance of favus is situated in the rete mucosum beneath the horny layer of the epidermis and in contact with the derma; it is first formed around the orifice of the hair-follicle, and as it increases in quantity by means of fresh formations beneath, it sinks more deeply into the follicle, and also extends its circumference around the hair, until it forms a cup-shaped mass, convex below, flat or slightly depressed above, and raised at the circumference, so as to constitute a rounded border. This appearance of the cup-shaped or, rather, saucershaped crust, no doubt suggested the name favus or honey-comb which has been given to it, and the idea of a honey-comb would be more fully realized when a number of these cups were assembled, as sometimes happens, in a cluster. Thus, it will be perceived that there is no lesion of continuity of the epidermis or of the epithelium of the follicle; these cover its epidermal and follicular surface throughout, while the deep surface is in contact with the basement-membrane of the derma. But the hair which pierces the centre of the crust of favus is frequently disorganized, and so also is the interfollicular epidermis which immediately surrounds the crust. When this is the case, the granules already described may be found entering more or less abundantly into the structure of the diseased hairs and into that also of the diseased epidermal cells; but they are not yellow like those of the favous substance, they are white like those which are found in trichosis and chloasma. This yellow substance of favus differs from the morbid element of the other phytodermata with which it is associated, not only in its colour, which is remarkable, but also in possessing a fluid or semi-fluid state when first formed, and hardening into a kind of paste, which subsequently becomes dry and pulverulent; and, thirdly, in its tendency to accumulate, and by its inward pressure, to produce absorption of the derma on which it lies.

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* Bazin is in error in stating that the morbid process takes place between the corneous and the mucous layer of the epidermis, and that, when the favous crust is removed, a layer of rete mucosum is seen beneath it; the layer beneath it is the basement-membrane of the derma, as we have frequently verified.

The action of the granules which are found in trichosis, mentagra, area, and chloasma, and also in the hairs and neighbouring epidermis of favus itself, is altogether different from this. They do not accumulate, although found, as we have stated, in the same sub-epidermic position, and also in the very substance of the hairs; they simply give rise to the production of an epidermal cell-tissue, which is spongy and friable, and breaks away in scales as quickly as it is formed; or which, taking the place of the normally constituted fibres of the hairs, weakens and discolours them, and on drying leaves them so brittle that they break off on the application of the slightest force.

One other fact is interesting in connexion with these diseases, and especially in connexion with two of them—namely, chloasma and trichosis. There can be no question as to the seat of the morbid action being the rete mucosum; if there were, it might be proved by the association of an increase of pigment with the two diseases above named. Indeed, this discoloration is the chief feature of chloasma, and has caused it to be classed with diseases of chromato-genesis, to which it is much more closely allied than to pityriasis; for, in some instances, the discoloration may be present without exfoliation of the epidermis; and however freely epidermic exfoliation may occur, it never approaches in nature or in appearance the furfuraceous desquamation of true pityriasis. In trichosis tonsurans also, as has been remarked by Bazin, there exists a brown discoloration of the skin, the clearing away of which he takes to be an important sign of the cure of the disease.

Such is the view which we take of the peculiar disease now described; a disease which we think properly defined by the term "granular degeneration," this term being intended to convey the idea of an arrest of development of the cell-tissue of the epidermis at its embryonic stage, and the production of a tissue constructed of crude and imperfect materials, which represents an earlier period of cell-generation than that which Nature intended, and which in consequence is truly in a state of degeneration from the perfect type. And we find a corroboration of our views in the Lumleian Lectures of Dr. Thomas K. Chambers, delivered before the College of Physicians in 1863.

In these Lectures "on mucus and pus," Dr. Chambers illustrates the operations of the powers of life, as evinced in the attraction of organic matter towards a centre, that centre becoming the nucleus of further action, the parent of new centres or nuclei, and the nuclei the agents of building up and constructing the cell and the tissues. The nucleus is a focus of life, and, as a focus of life, a separate individual, absorbing nourishment, augmenting in size, altering in shape, and increasing in number. Dr. Lionel Beale has demonstrated in a mucus-globule so produced, consisting of nuclei and granular matter enclosed in a cell-membrane, the power of formative life to be still active. He has shown that the granular matter of a part of the cell may be drawn into a mass; that the mass may cause a protrusion of the membrane of the globule at some one point; that it may become a bud, an embryo-cell; that it may separate from the parent-cell; and that a nucleus is already present in its interior. This, says Dr. Chambers, is organic growth; this will explain the rapid accumulation of mucus-cells on inflamed membranes. The first, the parent-globule, may have been an aborted epithelium-cell; and the subsequent marvellous increase is the natural consequence of the rapidity of generation which is characteristic of low organic life. Therefore, mucus may be viewed as a *parasite*,* receiving nutriment from the body, but not shape nor claim to vitality. Pus, in process of formation, presents similar phenomena to mucus; it is seen to be composed of globules of all sizes and of every variety of figure, oval, bulging, budding, without nuclei, and with nuclei. The lower the organization the more rapid and copious is reproduction; the less the demand for function, force, and intensity of existence, the more prominent is reproduction as the main object of creation. Whereas, when matter is destined to form part of an animal or of a consistent whole, it ceases to multiply itself; the "highest development of vitality is, ceasing to exist as growing matter." The intention of reproductive force is an expression of the lower, and exclusion from the higher, functions of life. The granular masses of the under surface of the rete mucosum are identical with mucus-globules. Mucus-globules are "young epithelium arrested in its growth and prematurely moulted;" a result of arrest of development. According to our view, favous matter and the mucedinales of the phytodermata are organic matter arrested in development at the lowest degree of life, the function of reproduction; the sporules are growing organic substance, aborted epidermic granules; the filamentary portion fully-formed organic substance, beyond which there is no further growth, the highest and perfected form of development.

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* In this sense we are willing to accord to the nosophytodermata the title which has been assumed for them of parasitic diseases, but not as organisms originating from without, and intruding upon the tissues of man, as the phytopathologists claim.

But another, and, it must be admitted, a very captivating theory of the pathological nature of the Nosophytodermata, has become popular in the schools of France and Germany, and has already made some way in Britain; a theory which has received a powerful impulse at the hands of a very able and zealous advocate of this doctrine, Dr. Bazin, of Paris. The theory in question is called the "vegetable theory;" and the pathological forms already detailed are regarded as component parts of a fungoïd plant. In the eyes of the promoters of the vegetable theory of the Phytodermata, the granules are spores or sporules, the seeds of the plant; the multinuclear granules are sporidia or seed-vessels; the moniliform strings are rows of sporules, and with the separate sporules and sporidia represent the fructification of the plant; while the transparent and branched filaments are the stems and ramifications of the plant. Then the theory of colonization of the plant is as follows:—A sporule is brought in contact with the skin; if the soil be favourable it strikes root into the epidermis; it perforates the epidermis, and it reaches the rete mucosum, where it grows and undergoes development and produces spores by which its further increase is effected. The more common seat of penetration of the spore is the mouth of a follicle; it no doubt perforates the soft epithelial lining of the follicle with more ease than it does the horny epidermis; but nothing is impossible to the growing spore. Having established its roots in the epithelium of the follicle, it spreads outwards in the rete mucosum around the mouth of the follicle; inwards through the epithelial lining of the follicle to the bulb of the hair which it pierces and invades in its interior texture; and upwards by the side of the hair, so as to become visible externally to form a kind of sheath for the hair, and to admit of being gathered in quantity sufficient to be submitted to examination with the microscope. It is the vegetation and accumulation in mass of this plant that causes the yellow cups of favus. It is this plant that robs the epidermis and hairs of their nourishment, and causes them to wither and be destroyed.

The presence of a fungoïd plant in the epidermic tissues of the skin was first announced, nearly thirty years ago (1836), by Remak; and has since been made the subject of investigation by numerous eminent physiologists in Germany and France—for example, Schoenlein, Link, Fuchs, Langenbeck, Gruby, Malmsten, Lebert, Robin, &c. Professor Schoenlein, of Zurich, appears to have been the first to distinguish the plant by a botanical name. His description related to favus, and he called the vegetable parasite of favus, Oidium; subsequently it has been named Achorion Schoenleinii. The plant found in the hair has been termed Trichophyton tonsurans; and that of the furfuraceous scales of chloasma, Microsporon furfur. These three—namely, Achorion, Trichophyton, and Microsporon—are the especial and essential plants of the phytodermata; other names have been suggested by different authors—for example, Porrigophyton, Mentagrophyton, Puccinia, Microsporon mentagrophytes, Microsporon Audouini—but none deserve to be retained, except the three already named. Bazin makes a hard fight for Microsporon Audouini, as distinguishing porrigo decalvans from the other forms of tinea, but fails, in our opinion, to establish it as an independent species. Indeed, the three organisms which we are willing to admit might, we believe, be with advantage reduced to two* only—namely, the pus-generated fungus of favus, Achorion; and the fungus of the epidermis and hair, which at present is recognised by the two names, Trichophyton and Microsporon; the difference between these latter being due, as we opine, not to any specific diversity of nature, but simply to the varied structure and density of the organ in which they are developed—namely, the epidermis and the hair. Or, instead of the term Microsporon, which is utterly unmeaning, we might adopt, as has been suggested by Bazin, the word Epidermophyton; and then we should have a separate term for each of the tissues involved—namely, achorion for the favous disease of the follicles; trichophyton for the disease of the hairs; and epidermophyton for the disease of the epidermis.

According to Charles Robin, the dermophyta or parasitic fungoïd plants of the human skin belong to the class Arthrospora, of which there are two groups, the Torulaceæ and the Oidiæ; the Torulaceæ are divided into two genera, Trichophyton and Microsporon: the Oidiæ, also, into two, Achorion and Oidium. The species are, as we have already stated, Trichophyton tonsurans, Miscrosporon furfur, and Achorion Schoenleinii. The Torulaceæ are distinguished by their great simplicity of structure, consisting of spores only, or of spores with a small proportion of mycelium. Of this simple structure are the trichophyton and the microsporon or epidermophyton. The Oidiæ are more complex in their structure, being composed of spores, sporidia, and mycelium; of this more complex formation is the vegetable parasite of favus, the achorion.

The Achorion Schoenleinii is the sole constituent of the crusts of favus, and is found only in favus. According to Bazin, when a fragment of the crust, moistened with water or acetic acid, is viewed in the microscope with a magnifying power of 200 to 300 diameters, it is seen to be composed of sporules, empty tubes or mycelium, and tubes filled with sporules or sporidia. The sporules vary in size, the smallest looking like small black granules, the larger, when magnified 800 diameters, seeming to have a double envelope. In shape they are not uniformly spherical. The greater number are oval, some are triangular, some constricted in the middle, and not unfrequently they are seen united end to end like a string of beads. The tubes are flexuous, uniform, or branched, empty, or filled with spores or granules; united together, they form stems of greater or lesser dimensions, and have the appearance of being articulated. Besides in the crusts, the achorion is found in the hairs, which have the appearance of being twisted, split, and woolly, which break, and are loosened at the roots, so as to admit of being pulled out with the slightest force. And, moreover, it is sometimes met with beneath the nail (onychophyton), which it breaks up and destroys.

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* Hebra, of Vienna, on the authority of Dr. McCall Anderson, reduces the number of cutaneous fungi to one; and he attributes the diversity of character of the fungus to the difference of structure of the part of the skin attacked—e. g., hair-follicles, hairs, epidermis, nail.

The Trichophyton tonsurans is the parasitic fungus of trichosis and mentagra. According to Bazin, it is composed exclusively, or almost exclusively, of spores, and in this respect differs from the Achorion; nevertheless, at the beginning of its development, before it has acquired sufficient reproductive power, and towards the end, when that power is on the wane, traces of mycelium may be discovered here and there. Like the achorion, it lives at the expense of the hairs, the epidermis, and the nails. When a portion of hair is examined in the microscope with a magnifying power of 200 to 300 diameters, it is found to be unevenly broken at the ends; the texture of the hair has the appearance of being disorganized, its longitudinal fibres are separated, and the intervals occupied with sporules. The stump of the broken hair is surrounded by a white sheath composed wholly of spores. The spores are innumerable; they are extremely regular in size, and they are found everywhere, both in the sheath and in the hair. In the epidermis surrounding the diseased hairs the fungus forms white, fleecy rolls and nacreous, snow-white lamellæ that seem to crop out from the surface in the midst of the yellowish and greyish *débris* of the desquamating cuticle. Beneath the nails, as occurs in the same situation in favus, it accumulates in mass, until it breaks up the texture of the nail and destroys it completely. The mentagrophyton of Gruby, Bazin shows pretty clearly to be a trichophyton.

The Microsporon furfur, discovered by Eichstedt, has, with good reason, been named by Bazin, Epidermophyton. It is essentially the parasitic fungus of the epidermis, and is found chiefly in chloasma, in which disease it is associated with a concentration of the pigment of the rete mucosum; and from its habit of disintegrating the epidermis, and causing its exfoliation in small, thin, and spongy scales, it is confounded with pityriasis, and is commonly spoken of as pityriasis versicolor. Besides chloasma, Bazin enumerates pityriasis nigra, maculæ gravidarum, maculæ hepaticæ and ephelis lenticularis,* as other chromatogenous affections in which the epidermophyton is found, and of which it is the active cause. The epidermophyta, according to Bazin, occupies in structure an intermediate position between the parasitic fungi of tinea and those of mucous membrane, the epitheliophyta, consisting of a rich network of tubes or filaments,† mingled with spores. The tubes or filaments are straight or twisted, simple or ramified, without septa, and furnished with terminal spores. The spores are spherical, bilinear in outline, and contain no granules; they are never met with in the structure of the hair, nor on the roots of full-grown hairs, but vegetate on the surface of the hair. In the parasitic furfuræ, in which it is found, the epidermophyton is mingled with epidermal scales and downy hairs; and by its colour, which resembles that of coffee diluted with milk, it gives rise to the especial tint of the eruption. By the aid of ammonia, the epidermic element of the furfuræ may be dissolved, and the fungus may be examined alone.

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* Hardy very properly objects to the admission of ephelis into this category, an affection involving a simple alteration of pigment without other symptom.

† Vide supra, the torulaceæ of which microsporon is a genus, are characterized by extreme simplicity, by their *small* proportion of mycelium.

To render the subject complete, and in justice to M. Bazin, we may just glance at the Microsporon Audouini, which we have rejected in a previous paragraph, and hear what can be said in favour of retaining it. The mycodermis, called Microsporon Audouini, was first described by Gruby, in 1843, as a parasitic fungus appertaining to area or porrigo decalvans; it appears on the bald patches of porrigo decalvans in the form of a greyish down; and, in the field of the microscope, is found to be composed of sporules and mycelium. The spores are smaller and less numerous than those of trichophyton, and are found on the shafts of the hair in isolated and racemiform groups. They are also met with in certain swellings or nodosities which are sometimes seen in the hairs in this affection; and there they are packed, as in trichosis, between the longitudinal fibres of the hair, which at these points are more or less parted asunder; but they differ from the trichophyton in being partial and not extending the entire length of the hair. Bazin also lays some stress upon the fact of the hairs in porrigo decalvans admitting of being extracted by the root, and being without a sheath; whilst in trichosis they break off, or if, by any possibility, they can be got out, they are found to be enveloped in a white sheath composed wholly of sporules. Such is the description of the Microsporon Audouini; and we must repeat, that we cannot see sufficient difference between it and the Trichophyton to recognise it as a separate species. Again, it is quite true that a moment exists in the history of area when the hairs break and fall off; but the change is so sudden that we rarely have an opportunity of examining the hair at this stage, although the opportunity may arise of discovering a few morbid hairs in the circumference of the bald patch.

If we pass in review the three parasitic mucedinous plants just described, with the view of comparing, and at the same time of contrasting them, we shall find between them certain very palpable differences; for example, the yellow colour of the achorion when seen in mass; its tendency to accumulate in mass, and the position of that mass around the mouths of the follicles; the white, pearly, and even snow-white colour of the trichophyton; its situation around the hairs in the form of a sheath; in the midst of broken particles of epidermis; and in the substance of the hair, which it utterly destroys; and the association of the microsporon furfur or epidermophyton with an increase of the normal pigment of the skin, supposed by Bazin to be the colour of the fungus itself; its seat upon the body and limbs, and not upon the scalp and face, like the two preceding, and its commingling with, and disintegration of, the epidermis, which it causes to exfoliate in minute furfuræ and scales. If, now, we compare their resemblances, we shall find that all the three affect the hair-follicles and hairs; that all are generated in the deep stratum of the rete muscosum;

that all cause disintegration and desquamation of the epidermis; and, finally, that all present the same pathological elements, though in different proportions—namely, spores and mycelium.

We will next follow Bazin in his mode of describing the diseases to which the dermophyta give rise, and trace whatever other analogies may exist between them. In order to give these diseases an uniformity of name, and unite them as closely as possible in a single natural family, he calls them *tinea* in the sense in which that term was formerly in use in England, and which was changed to Porrigo by Willan and Bateman. For example, his Tineæ are three in number, namely—T. favosa, T. tonsurans, and T. pelada (teigne pelade). Tinea favosa is the same in signification as porrigo lupinosa or favus, and obeys, as its pathological cause, the Achorion Schoenleinii. Tinea tonsurans, the equivalent of our Trichosis tonsurans, or furfuracea, he makes to include mentagra, which he formerly called Tinea mentagrophytes; its cause being the Trichophyton tonsurans. Tinea pelada (pelade) is the equivalent of porrigo decalvans or area, and is due to the Microsporon Audouini. The remaining mycodermis, microsporon furfur or epidermophyton is removed from the Tineæ, and is the dominant cause of chloasma or pityriasis versicolor, pityriasis nigra and maculæ gravidarum et hepaticæ, or, as Bazin sums them in two words, furfuræ parasiticæ; which might be rendered less objectionably by the words furfuræ dermophyticæ; although it is more than doubtful whether the term furfuræ is strictly applicable to every member of the list.

To constitute a natural group, or family of diseases, it is necessary, writes Bazin, that there should be an analogy of cause, symptoms, and treatment. Analogy of cause is perfect in the five diseases at present under consideration—namely, favus, trichosis, mentagra, area, and chloasma; whether we choose to consider these diseases as the manifestation of cell-degeneration in the epidermis and hair, or whether we prefer the vegetable theory, and suppose that the existing pathological changes are the consequence of the growth of a plant in the rete mucosum and hair acting the part of a parasite, feeding upon the epidermis and hair, and arresting in its course the nourishment intended for the healthy formation of these tissues. Let us now turn our attention to the next condition of analogy—namely, the symptoms—and see how far a correspondence in the signs of the disease may be considered to be complete. According to Bazin, the parasite is at once the cause, the symptom, and the lesion.

The symptoms of the Phytodermata, according to Bazin, are:—*firstly*, the parasite; yellow, and constituting cups of a peculiar shape in favus, beautifully white and existing in the form of short filaments in trichosis or tinea tonsurans, a greyish down in area, and stains of a light coffee colour in chloasma or pityriasis versicolor. *Secondly*, altered appearance of the skin—for example, such as results from pressure of the crusts in favus—or change of colour, as in chloasma and trichosis, in which there is an augmentation of pigment, or in area, in which there is a loss of pigment to a greater or less degree. *Thirdly*, eruptions symptomatic of the disease—for example, erythema, in disks or in rings; papulæ, vesiculæ, or pustulæ, occupying the area, and more commonly the borders of these disks and rings; or, in a more advanced stage, a furfuraceous desquamation; and *fourthly*, constitutional symptoms, or, as they are termed by Bazin, symptomatic phenomena—for example, disorder of nervous sensation or pruritus, which may precede or accompany the symptomatic eruption; and at a more advanced period of the disorder, sleeplessness, lassitude, and loss of appetite, the precursors of parasitic cachexia.

The course of the disease, according to Bazin, is divisible into three periods: the first being that of germination of the parasitic fungus; the second, its appearance and growth on or in the skin; the third, its destructive phenomena and termination. In the first period, there is pruritus in all and a commencing alteration of the hair; and in favus, trichosis, and mentagra, an inflammatory erythema. In the second period, the fungus makes its appearance, there is still pruritus; the hair undergoes a further stage of deterioration, and in chloasma, exfoliation of the epidermis begins. In the third period, there is increased destruction of the hair, obliteration and suppuration of the follicles in favus, trichosis, and mentagra, disappearance of the fungus, and baldness of a more or less permanent character.

Let us follow the course of these diseases more closely, beginning with favus. In the *first* period of favus there is pruritus; next, a dull erythematous redness, sometimes circular and circumscribed, sometimes diffused; next, a copious formation of scurf; then the yellow flat pustules which precede the cups; the hair loses its polish, looks dry and discoloured, and comes out by the root with less force than healthy hair. The *second* period is occupied with the formation and accumulation of the peculiar yellow favous matter—in other words, the growth of the achorion, which is now visible to the eye; pruritus continues, the hair is more changed in appearance, is slate-coloured, rusty, dull, falls out, and is reproduced; and secondary, or symptomatic eruptions, are apt to be developed. In the *third* period, the inflammatory action set up in the hair-follicles occasions a permanent fall of the hair, followed by closure of the hair-follicles and a cicatrix-like baldness; the remains of the crusts disappear, and the achorion, deprived of its nutrition, dies.

FAVUS has been commonly described as presenting two varieties—*favus dispersus* and *favus confertus;* the former title conveying the idea of disease of separate follicles giving rise to independent cups; the latter an aggregation of cups pressing one against the other, so as to produce a compound crust. In the latter case the cups may be simply coherent, or they may blend with each other and form a layer which is even with the scalp in the centre of the mass, and only presents the characteristic cup-like forms around the circumference. Very commonly the two forms are blended on the same head. When favus attacks the body, it generally assumes the dispersed, or isolated, form, because the hair-follicles are further apart; it has been met with on most parts of the body, and may occur wherever follicles exist; it has been seen on the pubes, on the glans penis, and also in another situation where no follicular origin can be traced—namely, beneath the finger-nail; so that, according to Bazin, the habitat of the achorion may be the epithelium of the hair-follicle, the substance of the hair, the epidermis, and the nail.

The term favus is derived from the resemblance of the cup, and particularly of the coherent cups to a honey-comb. Its synonyms are numerous—for example, tinea favosa, tinea lupinosa, tinea vera, tinea ficosa, porrigo lupinosa, &c. Bazin, Devergie, and Hardy call it tinea favosa; and the first of these authors distinguishes three varieties—namely, tinea favosa urceolaris, or favus urceolaris; tinea favosa scutiformis, or favus scutiformis; and tinea favosa squarrosa, or favus squarrosus.

Favus urceolaris (disseminatus, isolatus, lupinosus) corresponds with our favus dispersus—favus with separate, or separately defined, cups. When the cups are grouped closely together, Bazin names the eruption favus urceolaris coherens, which seems to correspond with our favus confertus. The term favus alveolaris has also been applied to this form of the disorder.

Favus scutiformis (favus en ecu, favus nummularis, favus en groupe, en anneaux, en cercles), according to the description of Bazin, is a very peculiar and uncharacteristic form of the disease, and seems to rest less upon its own merits than upon the fact of favus urceolaris being at the same time developed upon other parts of the body, and occurring on the scalp when the present form is in process of cure. It appears in circular patches, or scuta, a little raised above the level of the surface and furfuraceous, while under the layer of squamous epidermic crust there is produced a thin plate of yellow favous matter, which is perforated by the hairs. This yellow plate is uneven, embossed, sometimes elevated around the border, but it does not descend into the follicles. It would seem that in this variety, "the parasite loses in depth what it gains in surface." We are not surprised that it should be confounded by the English, as Bazin tells us is the case, with Tinea tonsurans; and that it should be equally misunderstood by his own countrymen. Hardy is of opinion that favus scutiformis has no claim to be considered a species; that it is in fact nothing more than a favus confertus which has lost its normal forms in consequence of age; that the crusts have been in part detached, and that the yellow irregular laminæ found on the patches are merely the remains of the original disease.

The favus squarrosus is even less well defined than the favus scutiformis; it is limited to the scalp; the favous matter is in contact with the shaft of the hair lying between it and the sheath of the follicle, the follicular sheath detached from its proper place has been thrust upwards upon the hair; several of these sheaths become united, and a fasciculus of the implicated hairs results which forms a rugged prominence upon the head. The pathognomonic cup of favus is absent; the disease neither attacks single hairs like favus dispersus, nor does it occur in a definite circular patch like favus scutiformis; it makes its appearance in elongated streaks of irregular form. Favus squarrosus is not generally admitted by authors, and is commonly described under the name of impetigo granulata (Tinea favosa granulata, Devergie), which, as Bazin remarks, it closely resembles. Hardy appears to take the proper view of favus squarrosus, when he regards it as the last term of favus confertus, when in fact the disease is worn out, and all that remains of it is an assemblage of whitish irregular masses that resemble old mortar, many of which are threaded upon the hairs.

TRICHOSIS TONSURANS, *vel* furfuracea, distinguished by the various synonyms, Tinea tonsurans, Tinea tondens, Porrigo tonsurans, Herpes tonsurans, Trichophytie tonsurante (Hardy), like the other forms of tinea, has its three stages or periods. The *first* period, which corresponds with the germination of the parasitic fungus, the Trichophyton tonsurans, has, for its symptoms, pruritus, an erythematous inflammation of the skin, and a change in the colour and texture of the hair, which looks dry, faded, rusty, fawn-coloured or yellowish, and breaks off close to the skin, leaving a bare patch of circular form and greater or less dimensions. The erythema is dull, and may occur in spots or in circular and circumscribed disks of various magnitude; sometimes uniform with the rest of the skin, sometimes raised at the border, and sometimes elevated over their entire extent; at other times, as commonly occurs on the body and limbs, the erythema may assume the form of an open ring. Moreover, in the latter situation, the ring is generally papulated (Lichen annulatus solitarius *vel* circinatus). Occasionally, small vesicles or pustules, may be developed upon these erythematous disks, or rings, in clusters or in rows, or they may throw up a furfuraceous desquamation (Herpes tonsurans, Herpes circinatus, Herpes circinatus pustulosus). The disks and rings enlarge by their circumference, and seem to creep (erpein, hence the word herpes, which must be distinguished from the vesicular eruption of that name) over the skin, often attaining a considerable size, taking in, for example, the whole scalp with the exception of its extreme border, or, in the annular form, occupying the entire extent of the neck. Not unfrequently, a ring may be seen to throw out a second ring from its circumference, and the second ring a third, as occurs in a concentric form of Lichen annulatus serpiginosus.

We must, in this place, trespass on the attention of the reader for a few moments in explanation of several of the terms used in preceding paragraphs, and especially in reference to the word "herpes." Tinea, the *teigne* of the French school, is represented in our own by the term Tinea capitis; a disease in which the hair is cropped close down to the skin, as we see in fur exposed to the ravages of the grub of the Tinea pellionella of Linnæus. The word is highly expressive, and Tinea tonsurans carries with it a vivid word-picture of the appearance of the disease. Not so, however, Tinea favosa and Tinea decalvans, in which there is no cropping of the hair; these are simply courtesy titles, and not titles of right; they creep into the group by virtue of a licence of classification, without which classification would, in fact, cease to exist. Tinea presumed too much upon its privileges, however; and Willan thought well to drop it altogether, and adopt the word Porrigo in its place.

Porrigo is derived, not from porrigere, to stretch out or spread, as stated by Mason Good, but simply "a porro, quia ut porrum in tunicæ involucra ita cutis velut in squamas resolvitur;" and therefore means, squamous. Celsus observes:—"Porrigo is a disease in which small scales crop up among the hair, and are thrown off by the skin. The disease commonly occurs on the head; sometimes, but rarely, in the beard, and sometimes even in the eyebrow." Hippocrates uses the word "pityrousthai," which is rendered by the Romans "Porrigine imfestari;" and Paulus Ægineta describes porrigo under the name of pityriasis; it is, in fact, the Latin equivalent for the latter term. It is clear, therefore, that the term porrigo is better suited as a distinguishing title for diseases of the scalp than tinea, for a squamous condition of the epidermis is common to all but area; whereas the tineal element—that is, the cropping or breaking off of the hair—exists only in one.

Herpes is a word that has been strangely abused, more so, in fact, than either tinea or porrigo. Herpes, derived from erpein, to creep, seems to have been originally employed as a distinctive term for diseases having a disposition to *creep* or spread upon the skin; hence Herpes esthiomenos, the eating herpes or lupus, &c. Hardy remarks that it was the synonym of Dartre or Tetter, a state of disease characterized by spreading and relapsing. Willan, however, saw reason for confining the term to the signification which is now recognised universally in the British school—namely, a vesicle intermediate in size between the small vesicle of eczema and the bulla of pemphigus, or, as Celsus expresses it, "bullula." In the modern French school these two ideas are perpetually obstructing and confusing each other. Hardy cannot find a single example of Willan's genus Herpes worthy of being retained, excepting Herpes labialis, which he reserves to fill a niche in his own classification; and confesses to an entire want of knowledge of a peculiar affection not very uncommon in England, but apparently unknown in the Hospital St. Louis—namely, Herpes iris, a disease excellently described by Marshall Hall, and illustrated in our 'Portraits of Diseases of the Skin.' The eruption which we name Lichen annulatus solitarius, and serpiginosus, or Tinea annularis, occurring on the body in concentric rings, without vesicles, and associated with Trichosis tonsurans, Bazin and Hardy term Herpes iris, which is clearly an error. By associating the two very different ideas, excentric annular development and vesicles, the French fall into the mistake of calling an erythema or a lichen, herpes, on account of its annular form; and as no vesicles are apparent, of appealing to the supposition that the vesicles were no doubt very minute and very transient; or, still led on by the same mistake, announcing as a presumed discovery that furfuraceous exfoliation may take place from an erythematous or papulous ring without a foregoing Willanean necessity, as they conceive it, an eruption of vesicles. The difficulty is easily removed; the school of Willan mean by the word herpes a large vesicle, a bullula; with the modern French school the word herpes simply represents an idea. Hence the use of the terms Herpes circinatus, Herpes tonsurans, &c., can only lead to confusion. Herpes circinatus is an erythematous or a papular ring without bullulæ, and Herpes tonsurans is a patch of Porrigo tonsurans also without bullulæ; moreover, in both the presence of vesicles is exceptional; they are, in fact, a herpes sine herpete, which is absurd.

To resume: the *second* period of Trichosis tonsurans is ushered in by the appearance of the parasitic fungus on the surface of the skin; this event is coincident with the fracture of the hair, the brittleness of the hair being due to the growth of the fungus in its substance. The patches are now deprived of hair, as though rudely shaved (tonsus), and an opportunity is given for examining the fungus, which appears in the form of white micaceous sheaths around the stumps of the broken hair, and conceals their ends from view, and also crops up amidst the epidermic furfuræ over the surface of the patch in white films or wreaths. Bazin compares the appearance of the patches at this period, when the stumps of the hairs are enclosed in their parasitic sheaths, to a surface coated over with white jelly. During this period also the patches throw up a thick layer of furfuræ, a hyper-secretion of epidermis (Bazin), and in the midst of these cuticular squamæ, the white wreaths of the trichophyton may be distinguished; while, under the furfuræ, the surface of the skin is papillated from congestion of the vessels of the follicles according to our view; but, according to that of Bazin,* from mechanical distension of the follicles with the parasitic fungus (turgescence or erection of the follicles). The same cause no doubt explains, in part at least, the papular form of the serpiginous (herpetic) rings of trichosis when they appear upon the hairless skin (Lichen annulatus solitarius et serpiginosus). Another symptom which is apparent at this period is a gradual deepening of the colour of the skin from augmentation of pigment; in persons of fair complexion it assumes a reddish or yellowish-grey tint; in dark persons it is bluish or slate-coloured. Bazin regards this discoloration as pathognomonic of the second period, and as evincing in its disappearance the recovered health of the skin. The hairs also become more and more disorganized and destroyed, and are more and more covered with fungi.

The *third* period of Trichosis tonsurans is marked by the disappearance of the fungus on the surface and its greater ravages in the follicles; the hairs are destroyed down to their bulbs, and suppurative inflammation is set up, which dislodges the remains of the hairs and obliterates the follicles, so that the part is left permanently bald. The pus at the same time annihilates the fungus, and a new series of phenomena is established consisting of inflammation, suppuration, exudation, incrustation, and thickening of the tissues of the skin. It is this latter series of morbid symptoms, with the addition of pustules, suppurating pimples, tubercles, deep indurations, and granulating excrescences, which especially characterize mentagra or sycosis; that disease, according to Bazin, being the third period of Trichosis tonsurans.

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* The MM. Mahon have compared this appearance to the skin of a plucked fowl: the papillæ they term asperities.

Suppuration of the follicles performs the part of a natural cure of the disease. Bazin emphatically observes that pus destroys the parasitic fungus, and therefore in seeking to find it when suppuration has commenced we must avoid those follicles and those hairs which are bathed with pus. In other words, that kind of inflammation which is productive of normal pus, is not the form of inflammation which causes granular degeneration of the cell-tissues of the skin, but on the contrary, is opposed to it; and in the treatment of the phytodermata we have only to supplant specific inflammation by common inflammation to effect a speedy cure.

The varieties of Trichosis tonsurans are not, as Bazin remarks, so much varieties in form, as are the varieties of Favus; but varieties which depend upon the seat of the eruption. The varieties which he admits are three in number—namely, circinatus, punctatus and gyratus. Trichosis *circinatus* (Trichophytie circinée, Hardy), appears in the form of rings, the rings being surmounted with small vesicles (Herpes circinatus), or simply papulated (Lichen annulatus solitarius *vel* circinatus). This form is common on the face and neck, the trunk of the body and limbs (ringworm of the body, Tinea annularis), and is sometimes met with on the scalp; where, as we have seen, circular erythematous patches covered with small vesicles (Herpes en groupes) or furfuræ (Herpes squamosus) are the usual form of eruption. Trichosis *punctatus* is also a common form of the eruption on the scalp, where it resembles a crop of pimples; and is also found in the area of the rings of Lichen annulatus of the body. Trichosis *gyratus* is met with on the scalp, and sometimes on the body, resulting from the development of a succession of excentric circles of any of the circinate forms, or from the interruption of one series of circles by another.

Bazin remarks, that on the *face* and *neck* Trichosis tonsurans begins in the form of Herpes circinatus or erythematous disks; and that sometimes a segment of a circle extends across the front of the neck from one mastoid process to the other; while the area is sprinkled over with disks and small rings. During the second period, the area of the rings becomes furfuraceous; and beneath the furfuræ the follicles are papillated. It is in the third period, and in the region of the beard and whiskers, that those more serious alterations are met with which constitute mentagra. On the trunk and limbs, erythema circinatum, erythematous disks, and lichen circinatus are almost the only forms of eruption; and in consequence of the insignificant size of the hairs and the consequent absence of nourishment for the trichophyte, the disease generally ends with the first period. In the hairy regions of the sexual organs and axillæ, the three periods of the disease are found to be combined—namely, the erythematous disks and papulous rings, the furfuraceous accumulations, broken and sheathed hair, and the pustules and tubercles of the third period.

Mentagra or Sycosis (Trichophytie sycosique, Hardy) is, as we have already seen, included in the description of Trichosis tonsurans; it has its three periods, but the chief and characteristic symptoms of the disease belong to the third period. In the first period there is pruritus and erythema and a commencing alteration of the hair; in the second period there is a continuation of pruritus, furfuraceous desquamation and a further change in the hair; but it is not until the third period has commenced that the disease comes to be really serious; it is then that we observe the pustules pierced by hairs, some springing directly from the derma, others surmounting hard papules; then we have boils and incrustations; tubercles, some superficial and some deep, for the most part hard; then follow thickening and infiltration of the skin with tubercles in a state of ulceration or surmounted with large fleshy granulations resembling a growing fungus (*inde*, sycosis), or fleshy vegetations. Afterwards, when the hairs are destroyed, and the trichophyte exhausted, there follow, obliteration of the hair-follicles, permanent baldness, and permanent cicatrices.

PORRIGO DECALVANS, *vel* ALOPECIA AREATA.—Having discussed two out of the three forms of tinea admitted by Bazin—namely, Tinea favosa and Tinea tonsurans—we may now proceed to the third, Tinea pelada (Teigne pelade). Tinea pelada comprehends two varieties of the same disease; these Bazin formerly considered as separate species under the names of Tinea achromatosa and Tinea decalvans; but subsequent considerations have induced him to unite them under the ancient term "Teigne pelade." The leading feature of Tinea pelada is loss of hair, hence the terms, Tinea decalvans, Porrigo decalvans, Alopecia areata, Area, &c.; but the loss of hair is not unfrequently associated with loss of colour of the skin and absence of colour in the hair which is subsequently reproduced on the denuded part, hence the second term—Tinea achromatosa. These diversities of character are the basis of a subdivision of Tinea pelada into Tinea pelada simplex and Tinea pelada achromatosa; the achromatous variety presenting two sub-varieties, in one of which there is no alteration of thickness in the affected skin; while in the other the skin is thinned and depressed.

Bazin associates with the Tinea pelada the Microsporon Audouini, first described by Gruby, in 1843; and, like the other forms of tinea, he traces the course of the disease through three successive periods. During the *first* period, the period of germination of the microsporon, there is pruritus in a moderate degree, and alteration of appearance and colour of the hair. It loses its brightness and polish, and looks dull and faded. The *second* period, the period of growth of the fungus, is still accompanied with pruritus; the microsporon appears on the surface in the shape of a greyish down, in small quantity, and easily overlooked. The affected portion of the skin is thickened, and a more decided alteration is apparent in the hair; it falls out, but grows again feebly; and, finally, ceases to grow, or assumes the quality of down. Then follows the *third* period: the pruritus continues, the hair falls for the last time; no effort at restoration takes place; the affected part of the scalp shrinks, the greyish or whitish down of fungus disappears, and the patch remains permanently bald.

Bazin is not so happy in his attempt to limit the course of Alopecia areata to three periods, as he is in the cases of favus and trichosis. The three periods might, in fact, be regarded as one, consisting briefly of pruritus, alteration of appearance of the hair, thickening of the skin, fall of the hair, and baldness. And then, with regard to these various signs, he is constrained to admit that they sometimes take place so rapidly that the fall of the hair and immediate baldness is the first symptom observable; this is his Pelada decalvans, the Porrigo decalvans of Willan. Then, with loss of hair, which falls out by the roots, there is in some instances loss of pigment; this is his Pelada achromatosa. Again, the disease of the skin may be comparatively trivial and produce no serious alteration in its tissues; this is his *non-depressed* variety of Area achromatosa; while at other times, with arrest of hair-nutrition, hair-formation, and pigment-secretion, there may be arrest of tissue-nutrition—in other words, an atrophy of the substance of the skin, the foundation of his *depressed* variety.

We have next to consider the nature of the part which the parasitic fungus, the Microsporon Audouini is supposed to play in Area alopeciata, of which it is to be remembered that Area ophiasis is only a serpiginous variety. And here our evidence is of a negative quality, it does not disorganize the hair from end to end, as does the trichophyton in trichosis; it does not distend the follicles so as to render them papillated or mamillated; it does not creep up the shaft of the hair and cover its broken end with a sheath; it does not penetrate into the fundus of the follicle, and burrow into the bulb of the hair; it does not disintegrate and break up the epidermis so as to make it look like a freshly harrowed corn-field. Then, what, it may be asked, does it do? It *appears* upon the skin! for Gruby saw it there; Robin denied that it was a Microsporon; but Bazin retains it for the convenience of his third form of tinea. Favus has its achorion, trichosis has its trichophyton, even the superficial chloasma has its microsporon; then pelada shall have its microsporon also. Bazin is a generous parent; he divides his little fungi equitably among his children.

But the hair is sometimes found broken in area, and especially around the circumference of its circular patches, and then comes the triumph of the Microsporon Audouini. In this case, as we have already noted, the hair is not diseased from end to end, as occurs in trichosis; but near the skin, and on some of the hairs, swellings or nodosities are observed, spherical or oval in figure. These nodosities are occasioned by a separation of the longitudinal fibres of the hair, and in the midst of the separated fibres, and the cause of their separation and the subsequent breakage of the shaft, there are found granules. These granules, according to Bazin, are the sporules of the Microsporon Audouini; but why of the microsporon? Why not of the trichophyton? The answer which Bazin would no doubt give, is as follows:—Because Gruby discovered a greyish down on the ill-kept head of a person affected with Alopecia areata, and because in trichosis where the trichophyton is found there is a greater degree of destruction of the hair. But it must be remembered, as a commentary on this explanation, that the seat of the dermophyte is the rete mucosum or the substance of the hair; whereas it is not pretended that the Microsporon Audouini is found in the rete mucosum. Again, the amount of destruction of the hair is simply a matter of degree; we can conceive no reason why the trichophyton might not destroy partially as well as wholly, if in truth it be the destructive agent which the advocates of the vegetable theory of tinea pretend.

The loss of pigment in the skin, in association with Alopecia areata, is attributed to the microsporon; it feeds upon the pigment, just as, by exerting its parasitic qualities, it absorbs the nutrition of the skin and causes the fall of the hair and atrophy of the cutaneous tissues which ensues. But by the side of this phenomenon we have the apparently contradictory fact of the accumulation of pigment in another of the parasitic tineæ, the Trichosis tonsurans. The supporters of the vegetable fungus doctrine have overlooked this very important feature of diagnosis between trichophyton and Microsporon Audouini: the latter feeds on pigment, the former rejects it. But eagerness for pigment as an article of nutrition is not a family propensity on the part of the microsporon, for another member of the same family, the Microsporon furfur, would appear to reject it entirely, as, in association with the latter—*e. g.*, chloasma—there is so great an excess of pigment that the affection has been commonly considered with the group of dyschromatogenous diseases.

CHLOASMA, *vel* PITYRIASIS VERSICOLOR.—Under the name of Furfuræ parasiticæ (crasses parasitaires), Bazin includes chloasma or Pityriasis versicolor, Pityriasis nigra, Maculæ gravidarum, which he terms Chloasma, Maculæ hepaticæ, and Ephelis lenticularis, or common freckles. The fungus associated with this series of affections is the Microsporon furfur first described by Eichstedt, or, as Bazin very aptly names it, epidermophyton; for its ravages are chiefly confined to the epidermis, although there cannot be a doubt, and we long since drew attention to the fact, that the follicles of the skin are also, and, as we believe, primarily affected. In this respect, chloasma comes very naturally into the group of Tineæ, and deserves to occupy a place among "diseases of the hair-follicles and hairs;" the natural group into which we propose to assemble all these affections, and where we would suggest that they should be allowed to remain until the vexed question of their parasitic nature be settled beyond further dispute. We have hitherto classed chloasma among the "disorders of the chromatogenous function of the skin;" but we have long felt that that position ill accorded with the decided furfuraceous desquamation which accompanies it; and we readily admit the analogy of cell-degeneration which subsists between favus, trichosis, and chloasma. As to the maculæ of the skin and ephelis lenticularis we are not yet certain, and, as it seems to us, it is only in exceptional cases that they present the furfuration which is common to the other affections. That chloasma or pityriasis versicolor is not a pityriasis—that is, that it does not belong to the group of squamous affections—must be admitted by every dermopathologist.

Chloasma has received its very appropriate name from its yellowish-brown colour; and this peculiarity of colour, in addition to its furfuraceous desquamation, has gained for it the title of Pityriasis versicolor; while a deeper tint of colour, approaching to black, distinguishes Pityriasis nigra. According to our views, the colour of chloasma is due to a concentration of tint of the pigment of the skin; but in the belief of the phytopathologists, the colour is present in the plant itself. Bazin declares that the fungus feeds on the epidermis, and is more superficial in its operations than the trichophytes and the onychophytes; that it is sometimes seen on the short hairs of the skin, but never on the lanugo; and he is indignant with those authors who have stated that it destroys the pigment, for that, he says, is the attribute of the Microsporon Audouini and not of the Microsporon furfur.

Chloasma is not always accompanied with pruritus. Bazin says that pruritus sometimes precedes the eruption, and is usually present when it is developed. Generally the pruritus is trifling, but we have seen cases in which it has been severe and harassing. He very properly draws a broad line of distinction between this disease and Pityriasis simplex et rubra.

EPITHELIOPHYTÆ.—As certain portions of the mucous membrane fall under the observation of the dermopathologist, Bazin thinks it desirable to draw attention to the fact that fungous plants are occasionally met with, occupying the same position with reference to the epithelium that the epidermophyte does in regard to the epidermis. They occur also on the surface of recent blisters, and not unfrequently on the surface of ulcers. The apthophyte, the Oïdium albicans, is a common associate of aptha, and yields, with the utmost facility, to the treatment which is destructive of the epidermophyte.

CAUSE.—The cause of the phytodermata, according to the supporters of the vegetable theory, is the fungus-plant or mycodermis. The fungus, according to Bazin, is at once the cause, the symptom, and the pathological lesion. Another consequence follows from this theory—namely, that these diseases are all highly contagious. That they are not so is matter of practical experience; that they may be contagious to a limited degree is possible; that they are much less contagious than is generally acknowledged, we firmly believe. But a plant, producing seeds or sporules in myriads must needs be a highly contagious affection, for all that is necessary to the communicability of the disease is, that one of these myriads of spores should fall into the mouth of a cutaneous follicle; and there, finding a favourable nidus for growth, should pierce the horny layer of the epidermis, and so reach its mucous layer, where pabulum in abundance exists for its development and further increase. It is true that Bazin and his disciples admit the necessity of a further element of propagation, namely, *aptitude;* the plant may take root; it may grow even, but it will not flourish in the absence of aptitude; or, as we should say, speaking with humoralistic views, without a favourable state of the humours or juices of the body; in other words, an unhealthy condition of the fluids. Bazin draws some curious and not particularly clear distinctions between the different stages of the phytodermic diseases; for example, an affection of the skin produced directly by the fungus, such as the erythematous disks and rings, and the papulous, vesiculous, and pustulous rings of Trichosis tonsurans, he calls a parasitic *affection;* whereas the particular state of the organism which maintains and combines these symptoms, lesions, and affections, he terms a parasitic *disease.* The fungus he considers to be the agent of producing a particular condition of the organism of the part, which he calls aptitude; and this particular condition of the organization, this aptitude, is a necessary element in the maintenance of the disease. Here, he observes, "as in every disease, we find two causes whose concurrence is necessary—one external, and one internal. It is this latter which I have designated *aptitude;* it must not be mistaken for a favouring state of the soil. The disease may be considered as the result of these two forces, internal and external. The parasite can effect nothing without the aptitude of organism, and without the parasite the aptitude is sterile." The first portion of his argument goes to show that the aptitude is the consequence of the parasite; the latter, that the aptitude may be in existence before the appearance of the parasite; thus corroborating the humoralistic view of the case, that a peccant state of the fluids of the body must be in existence before the implantation of the fungus can take place.

With regard to predisposing causes, it may be generally stated that favus and Trichosis tonsurans commonly occur in children and adolescents; that both result from a state of cachexia induced by defective nutrition or reduced vital power, and that favus is commonly associated with the strumous diathesis. Trichosis decalvans may occur at any period of life, and is as common in adults as it is in children and young persons. It also is a consequence of deteriorated vital power, general or local. Trichosis maxillaris and versicolor, mentagra and chloasma, occur in the adult, and are independent of any marked lowering of power of the constitution. We have, however, never seen an instance of either in a person of perfectly sound health.

According to Hardy, the persons the most susceptible of the germination of parasitic fungi are such as are exhausted by want or disease—strumous subjects, for example; and such persons have with good reason been compared to "those ancient trunks of trees the favourite habitat of parasitic vegetation."

Diagnosis.—The diagnosis of the phytodermata is twofold: firstly, as distinguishing them from other cutaneous diseases; and, secondly, as distinguishing them the one from the other. The general characteristics of these diseases are:—The seat of the disease in relation to the hair-follicles; a furfuraceous desquamation of the skin, resulting from a peculiar state of disintegration of the [epidermis, and the disorganization and probably destruction of the hair. Their special characters are:—The accumulation of a peculiar yellow substance around the mouths of the follicles, assuming the figure of cups, each pierced in the centre by a hair in Trichosis favosa or favus; the breaking off of the hair close to the head, and leaving patches that have the appearance of being shorn, in Trichosis tonsurans; the total baldness of the patches, from actual falling out of the hair and exhaustion of growth, in Trichosis decalvans or area; the disks and rings, one while bounded by a raised border, another while crested around the circumference with papulæ or vesiculæ, in Trichosis corporis or Lichen annulatus; the assemblage of patches, pustules, and tubercles, in Trichosis maxillaris or mentagra; and the brownish and greenish yellow stain and furfuraceous desquamation of Trichosis or Pityriasis versicolor.

The cutaneous diseases with which they are apt to be confounded, are impetigo and eczema capitis, and pityriasis: in impetigo and eczema there are moist exudations and muco-purulent discharges, which form crusts of considerable thickness. In the phytodermata there is no moisture and no exudation in the first and second periods, although there may be a pustular eruption in an advanced period of the disease, a secondary eruption, the consequence of inflammation of the tissues of the skin. The furfuraceous desquamation of the skin differs from that of pityriasis in being limited to the diseased patches wherein other morbid phenomena are in progress, as well as in being different in intimate structure; and the union of increased pigment with a furfuraceous desquamation is pathognomonic of chloasma.

The phytopathologists very wisely recommend their disciples to trust to general indications for the diagnosis of these affections, and to avoid recourse to the microscope, excepting in cases of extreme difficulty. Hardy, however, has the courage to declare that in a case of doubtful diagnosis the microscope will clear away all misgivings. We can only say that we should attach very little weight to the opinion of any man who had the temerity to pretend to establish a diagnosis between favus and Trichosis tonsurans, on the more than doubtful differential characters of the achorion and trichophyton as viewed by the microscope.

PROGNOSIS.—The duration and termination of these diseases have both been considerably lightened by the improved methods of treatment of modern times. Favus is serious chiefly on account of the morbid diathesis with which it is associated. But for the cure of the local disease Bazin states the period to be not more than two months. Trichosis tonsurans, although less grave than favus, so far as its constitutional influence is concerned, is more chronic in its nature. In hospital, Bazin states the period necessary for its cure at two to four months; but left to itself it may continue in its first stage for fifteen or eighteen months, and mentagra has been known to be prolonged to fifteen or twenty years. When spontaneous cure does take place, it is accompanied with permanent loss of hair and indelible cicatrices. Trichosis decalvans is still more enduring than Trichosis tonsurans, and extremely difficult to cure. Pityriasis versicolor, however, soon yields to treatment, but is liable to relapse.

Hardy remarks that favus when neglected has a tendency to perpetuate itself indefinitely; it is no uncommon thing, he says, to meet with adults who have suffered from the disease from infancy. The only natural cure of the disease being the total destruction of the hair and atrophy of the scalp.

TREATMENT.—The treatment of the phytodermata offers three indications:—The *first*, to destroy the parasitic fungus; the *second*, to subdue the inflammation which accompanies the growth of the fungus; the *third*, to relieve the secondary or constitutional eruptions which complicate the disease in its advanced stage. The remedies suited to the first of these indications are termed by the French phytopathologists "parasiticides," and with especial reference to the vegetable fungus "phyticides." Bazin enumerates as the chief of the phyticides, the oleum picis juniperi (oleum juniperi pyrolignicum, huile de cade), the bichloride of mercury, and turpeth mineral (hydrargyri subsulphas flavus). But another, and the most important of the phyticides, according to him, is depilation or avulsion of the diseased hairs. The second indication is met by antiphlogistic remedies of the emollient or moderately stimulant class, such as the cold starch poultice, fomentations, cold ablutions, saturnine lotions, the benzoated oxide of zinc ointment, and the ointments of nitric oxide of mercury, or the ammonio-chloride of mercury considerably diluted. While the third indication, the secondary complications of the disease, is to be treated by similar means.

Bazin describes his method of treatment of the tineæ as follows:—The head is to be thoroughly cleansed; crusts are to be removed, and the hair cropped close to the scalp; then the surface is to be painted over with the oleum picis juniperi, which, as he states, soothes the skin, renders it less sensitive, loosens the bulbs of the hair, and at the same time shrivels and destroys the fungus. The next day, the head is to be washed with soap, and depilation is commenced; that is to say, every hair is to be removed over the whole extent of the diseased surface. The accomplishment of depilation may possibly require four or six sittings, for the process is too painful to be borne for a long period at a time, and may require to be repeated twice or three times, or even more frequently, before the cure is effected. The operator seats himself in a chair, and places the patient on a cushion at his feet; he rests the patient's head on his knees, and then, with a pair of tweezers, patiently removes the hairs, one by one, or in bundles of two or three, when they can be fairly seized by his instrument. Having pulled out the hairs over a small extent of surface, he brushes it over with a solution of the bichloride of mercury to destroy the fungus, both on the surface and in the patent follicles; and then goes to work again in the same manner, from time to time resting, to moisten the skin with the solution, until he has proceeded as far as the patient can bear.

Four or five hours after this operation, the head is to be thoroughly anointed with a parasiticidal ointment, of which the two following are the best, and Bazin gives a preference to the first:

℞ Hydrargyri subsulphatis flavi, ʒss.
Olei amygdalæ dulcis,
Glycerinæ destillatæ, āā ʒij.
Adipis purificati, ℥ij.
M. Fiat unguentum phyticidum.

℞ Olei juniperi pyrolignici, ʒij.
Adipis purificati, ℥iiss.
M. Fiat unguentum phyticidum.

On the following day, the washing, the depilation, the painting with the bichloride of mercury lotion, and the anointing with the phyticide pomatum are to be repeated as before, and continued daily, until every hair growing on the diseased portion of the skin, and for a short distance beyond, has been removed. This process is painful, but not, according to Bazin, so painful as the avulsion of healthy hairs; and the pain may be much reduced by drawing gently but forcibly on the hairs in the direction of their implantation in the skin.

When depilation is complete, the head is to be painted over, night and morning, with the solution of the bichloride of mercury, for three days; and after the third day, is to be anointed, night and morning, with the yellow subsulphate of mercury ointment. Bazin does not mention the strength of the sublimate solution which he employs, but the solution used by Hardy contains one grain of the bichloride to an ounce, and we apprehend that if it were stronger it would produce inflammation and more pain than the patient could bear. The success of the process cannot be ascertained until the hair begins to grow again; and then, if it be unhealthy in its aspect, the operation must be repeated, if necessary, to the third or fourth time.

Hardy's treatment of tinea differs from that of Bazin only in this—that he finds no advantage in the huile de cade, and therefore omits its use; he prescribes the sublimate lotion after depilation, for eight days instead of three; and in the latter part of the treatment, sulphur ointment, containing half a drachm of sulphur to the ounce, instead of the turpeth ointment. We may in this place, also remind our readers of the sulphurous acid lotion of Dr. Jenner, and the pentasulphide of lime lotion, recommended by Mr. Astley Price, for the destruction of the oïdium of the vine; the latter being an equally successful phyticide, or, at least, remedy, when used to the human skin in these diseases.

Treatment of Favus.—In favus the first step is to cleanse the head, to crop the hair down to the crusts, to paint with the oleum juniperi pyrolignicum; then to apply a starch poultice to soften the crusts. When the crusts are sufficiently softened, they are to be gently lifted with a comb and removed, and the oleum juniperi is to be repeated as before. On the following day depilation may be performed, as already described; and in four or five days the operation will be complete.

When the body is the seat of the eruption, and the latter is extensive, baths containing sulphur, or the bichloride of mercury, will be necessary; the patient should have them repeated several times; the crusts are to be removed after the bath, and depilation may then be commenced. The process of depilation will occupy from fifteen to twenty days, and, night and morning, the turpeth ointment should be well rubbed into the skin. In favus of the nail, the horny portion of the nail must be scraped until the fungus is laid bare, and then it should be saturated with the sublimate lotion.

After the operation of depilation, the redness of the diseased skin gradually diminishes for about a month; then there is some reappearance of hyperæmia, a few pustules are seen, and the favus-cups begin to be developed; but they are smaller and more scattered than at first. Depilation is now to be repeated in the same manner and to the same extent as before. The patient is then left for another period of five or six weeks; and if any favus-cups reappear, they must be removed and depilation of the parts where they were produced is to be performed. This is only a partial depilation; and after two complete, and one partial depilation, the cure may be expected to be complete, and no further danger of relapse anticipated. Nevertheless, it is always necessary to watch the patient for a few weeks longer.

Bazin remarks, that if the eruption exist to the extent of five-sixths of the scalp and body, the removal of every hair the patient possesses, whether unhealthy or healthy, will be necessary for the cure; that the operation must be repeated in four or six weeks; and that at the end of another month a partial depilation must be effected. Where there are not more than four or five patches situated on the scalp, depilation of the diseased part and of the sound skin for a short space around will be all that may be necessary. And where there are not more than two or three cups, they may be cured at once, and in the course of a few minutes, by the removal of the crusts and the avulsion of a few hairs.

During convalescence, extreme cleanliness is of the first importance, any formation of scurf or accumulation of sebaceous matter should be carefully washed away, and the yellow subsulphate of mercury ointment applied every four or five days.

Treatment of Trichosis Tonsurans.—The treatment of this form of disease, like the preceding, consists in the performance of depilation and the use of the phyticide applications. Trichosis of the body or limbs in women and children rarely progresses beyond the first period, and the bichloride of mercury lotion, together with the phyticide ointments, are all the treatment necessary. When, however, as in man, the body is hairy, and the disease is circumscribed, depilation may be practised during the first period, and followed by the plan of treament laid down for favus; but until the disease is fairly defined, the bichloride of mercury lotion and the phyticide ointments are the only means to be employed.

After the first period is passed—whether the disease be seated on the body or on the scalp—depilation is to be performed in the manner already described for favus. But the operation is more difficult than in favus, on account of the brittleness of the hair, and may require to be repeated five or six times before the whole of the diseased hairs are removed.

The restoration of the skin to its healthy condition is shown by the growth of natural hair; the papillation of the patches subsides, the patches return to the level of the surrounding parts, and the scalp recovers its normal colour and appearance. The healthy growth of the hair is the best evidence of the destruction of the fungus.

Treatment of Mentagra.—On the face, and especially on those parts of the face where mentagra commonly makes it appearance, avulsion of the hairs may be practised in every stage of the disease. The oleum picis juniperi should be applied in the first instance as directed for favus, and then the avulsion may be commenced at once. Whenever, says Bazin, "Tinea tonsurans has already, for some time, reached its papulo-pustular period, depilation must be performed, even if we be sure that the parasite no longer exists; it is in these cases that the disease yields as if by enchantment to our treatment; a single depilation may suffice; and on this account it is that the cure of mentagra may be said to be easy in proportion to its chronicity." The same author remarks, that much time is often lost in vain attempts to subdue the inflammation and tumefaction of the skin in mentagra before resorting to avulsion of the hairs, under the impression that the inflammatory action would be increased by the operation; but the real agent of irritation is the hairs, which act the part of a foreign body and stimulate the inflammatory process; and the best method of dissipating it, would be to remove the foreign body, which is the cause of all the mischief. After avulsion our remedies perform wonders.

Treatment of Alopecia Areata.—The treatment of area is to be conducted on the same principle as that of the other forms of tinea—namely, the application of phyticides and depilation. Bazin remarks that the puny hairs which are found on the bald patches of area are difficult to remove on account of their weakness, and that repeated depilations are therefore necessary; he also reminds us of the necessity of carrying the depilation to a short distance beyond the limits of the patch into the region of the sound hair.

Treatment of Phytodermatous Furfuræ.—The treatment of these affections, including chloasma, is, according to Bazin, extremely simple: no internal treatment is called for; the parasitic fungi are situated superficially in the epidermis, and the phyticidic remedies already enumerated are all the treatment required; they may be used in the form of lotion, forcible friction, or baths, according to their situation or extent. The treatment commonly pursued by this author is to apply the bichloride of mercury lotion and use sulphurous baths, but he gives the preference to baths containing the hydrargyri bichloridum*.

Epitheliophyta.—Carrying out his views of the parasiticidic operation of the bichloride of mercury, Bazin recommends it as a wash for the mouth in aptha, to destroy the Oidium albicans, in preference to the biborate of soda. And upon the same principle he favours the use of the pyroligneous oil of juniper in chronic ulcers.

Observations.—The chief point of novelty in the treatment of Tinea, as advocated by Bazin, is depilation; but this recommendation is far from being new; it was practised by Plumbe, by Rayer; it is the basis of the treatment of the Mahons, and the principle of the notorious "calotte," so long, and we fear at present, in use in France. But the objects of the treatment are different; Bazin removes the hairs because they are the harbour of a parasitic vegetable fungus, which developes seeds, and consequently, as long as it remains, keeps up and extends the disease. Plumbe, Rayer, the Mahons, and the promoters of the calotte, because the hairs are supposed to act the part of foreign bodies and irritants to the inflamed skin. "In old standing cases of favus," says Rayer, "every method of treatment into which the avulsion or removal of the hair does not enter as an element, is incomplete, and unworthy of being entitled curative."

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* The strength of the mercurial bath ranges from one drachm to three, and may be carried as high as one ounce. It will be recollected that this salt of mercury is soluble in water only to a limited extent. The bichloride lotion varies from one-eighth of a grain to one grain to the ounce.

The depilation recommended by Plumbe differed from that of Bazin in being partial; the same objection may be made to that of the Mahons, which operates only upon the loosened hairs; while the calotte is an instrument of the grossest barbarity, tearing out alike sound and diseased hairs, and in a manner the most painful that could be contrived. Bazin's method is painful enough, and to many persons would be insupportable, but every care is taken to make it as gentle as possible; the skin is rendered less sensitive by the previous application of the juniper tar, and the hairs are withdrawn artistically, no doubt in the direction of the *set* of the shaft, with moderate and steady traction, and to a limited extent at each repetition of the operation. With reference to the rest of the treatment, it differs in nothing from that which has been in common use in this country for a number of years—namely, washings with soft soap, combing and brushing the hair, and the use of mercurial pomatums, of which the nitric oxide and ammonio-chloride of mercury are examples, and tar and sulphur ointments, which in public institutions are highly popular.

With the exception of mentagra, we believe depilation to be wholly unnecessary in these diseases; we believe that they may be successfully cured, as they have been heretofore, without depilation. We never find any overwhelming difficulties in the treatment of these complaints; they are tedious, but not remarkably so when properly managed, and we decidedly declare against depilation. Depilation may be, and no doubt is, a great boon in the treatment of tinea in France, but it is worthless in England, where more care is bestowed upon the constitutional treatment of cutaneous diseases.

And what, it may be asked, is the treatment on which we rely for the cure of these diseases? We will state it briefly:—We remove crusts and furfuræ by thorough washing with the juniper tar or petroleum soap, or with a liniment consisting of equal parts of soft-soap, juniper tar, and alcohol. We repeat the washing daily with tepid or cold water, with the double object of cleansing the diseased parts and giving a moderate stimulus to the skin of the head; we comb the head well, brush it if not too sensitive, and night and morning we rub into the entire scalp, but most into the patches, an ointment consisting of one part of the nitric oxide of mercury ointment, diluted with three parts of fresh lard. In milder cases we wash the head less frequently, but we continue the combing and brushing and anointing steadily. A failure of cure is an event that we cannot anticipate and rarely see.

But we do something more: we administer mild tonics; we prescribe a generous diet; we treat symptoms; and, in a case of favus—a disease of the scrofulous diathesis—we have recourse to cod-liver oil and the iodide of iron. Moreover, in certain cases, where no indications of general debility exist, where the nutritive functions seem principally at fault, we administer arsenic, with the most brilliant success. Favus and Trichosis capitis are diseases of children and adolescents, and this method of treatment answers admirably with them. In Trichosis corporis (Lichen annulatus solitarius et serpiginosus) we have for many years been in the habit of prescribing a lotion, or ointment, of the bichloride of mercury; and in chloasma, or pityriasis versicolor, we prescribe ablutions with the juniper-tar soap, sponging with a spirituous solution of the bichloride of mercury in almond emulsion; or frictions at night with one of the diluted mercurial ointments already mentioned, and ablution in the morning with the juniper-tar soap. In Trichosis barbæ, or mentagra, we are ready to admit the value and importance of avulsion of the hairs; not, however, as a means of removing a supposed parasite, nor of an irritant foreign body, but as a healthful stimulant to a part in a state of chronic and irritable inflammation.

And this brings us to the question of the use of depilation: that it disposes to the cure of the local disease there can be no doubt. We cannot disbelieve the statements of Bazin that he succeeds in curing these diseases through the agency of depilation; but we do and must disbelieve that the removal of the fungus is the proper explanation of the cure. We see in depilation a stimulant, and a valuable stimulant—a stimulant that we ourselves commonly use when we seek to make a black hair grow up in place of one that is white—a treatment that for this purpose we have been in the habit of using for years, and with remarkable success; so that we have been led to regard avulsion as one of our best, indeed our only reliable remedy for producing *deep stimulation of the cutaneous tissues*, for setting up a new action in the papillæ of the hair; as the only remedy, in fact, by which we can reach successfully the fundus of the hair-follicle. And this, in our belief, is the "methodus medendi" of depilation and avulsion in the phytodermic diseases. It sets up a new action, a healthful inflammatory process in place of a morbid inflammatory process—a process whose natural course is to end in resolution, instead of one whose nature is perpetuity. For morbid cell-genesis it establishes healthy and plastic inflammation and sometimes healthy suppuration, when, as Bazin declares, the fungus is starved or drowned. Even Bazin has recourse to avulsion as a stimulant in old cases of mentagra, wherein the hair acts the part of a foreign body, of a thorn, in the skin, and so keeps up irritation.

Hardy makes the following very practical observation in reference to the treatment of the Phytodermata:—"It will not be sufficient to destroy the parasitic fungi only; we must modify the soil so as to render it inapt to the reception of new germs, and unfavourable to their nutrition. We must give tone to the constitution by means of tonics and bitters, prescribe good food and closer attention to cleanliness, and cut the hair short to admit of the action of the air upon the scalp."

PART FOURTH.

Chronicle of Medical Science

(CHIEFLY FOREIGN AND CONTEMPORARY).

HALF-YEARLY REPORT ON PHYSIOLOGY.

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I. Circulation and Respiration.

1. J. Sachs: *An Inquiry into the Seat of the Formation of Carbonic Acid in the Animal Body.* (Reichert and Dubois-Reymond's Archiv, 1863, pp. 345.)
2. Sczelkow: *On the Interchange of Gases which takes place in the different Organs of the Body.* (Sitzungsberichte der Wien Akad., Band xlv. 1862, p. 171.)
3. M. Edenhuizen: *Essay on the Physiology of the Skin.* (Henle and Pfeuffer, Zeitschrift, iii. Reihe, Band xvii. p. 35–105.)

1. Sachs observes that three opinions may be held upon the seat of the formation of carbonic acid in the animal body: 1. That it is formed in the tissues, from whence it passes into the blood; 2. That it is exclusively formed in the blood; and 3. That it is generated in both places. At first sight the objection made by Ludwig against the formation of carbonic acid in the tissues appears to be a strong one—viz., that if this be the case, the pressure or tension of the carbonic acid in the tissues should be greater than it is in the blood; but direct observations prove the contrary. Thus Schumowsky has shown that even the muscular tissue does not contain more than about 14·4 per cent. of carbonic acid; and as regards the fluids of the body, Schöffer states the amount of carbonic acid in the urine to be 4·4 per cent., whilst its proportion in the milk varies, according to Setschenow, from 5·01 to 6·72 per cent. In all these instances, therefore, it is present in a proportion far below that in which it is contained in the blood. But the force of the objection vanishes if it be remembered that a large portion of the carbonic acid naturally present in the blood is in combination with the phosphate of soda, and that only a small part is really free. In order to ascertain whether carbonic acid is generated in the blood, the method pursued by Sachs was to divide a freshly drawn portion of blood into two parts—to examine the gases in one immediately, and to set the other aside for some hours before obtaining the gases from it. He draws the following conclusions from six experiments, made with great care:—1. The gases contained in the blood after its removal from the body, at ordinary temperatures undergo marked changes in their relative proportions, the oxygen gradually disappearing, and carbonic acid taking its place; the quantity of carbonic acid generated being, however, always greater than the volume of oxygen which has disappeared. 2. The quantity of carbonic acid that is chemically combined in the blood increases with the interval at which it is examined after removal from the body; and he agrees with Schöffer and Sczelkow in consider-

ing the quantity of chemically combined carbonic acid to be always greater in venous than in arterial blood. 3. Though the number of experiments was too small to found positive statements upon, it appears that after forty-eight hours the quantity of oxygen remaining in the blood is reduced to a mere trace, and after that period (for some time at least) the gases suffer no further change. 4. He was unable to draw any definite conclusions in reference to alterations in the proportion of nitrogen. 5. The presence of fibrin is a necessary condition for the formation of carbonic acid in the blood, but the exact *rôle* which it plays cannot as yet be definitely stated. From these results he believes that he is justified in the general conclusion, that carbonic acid is constantly being generated in the blood at the expense of the oxygen present in it, and that consequently carbonic acid is not formed exclusively in the tissues.

2. In Sczelkow's experiments three kinds of blood were compared:—1. That obtained from the carotid artery. 2. That obtained from the femoral vein when the muscles of the leg were quiescent. 3. That obtained from the same vein when the muscles of the leg were tetanized. In the following table the first is indicated by A, the second by V R, and the third by V B:

Experiment		Colour.	O	N	Free CO_2	Combined CO_2	Total CO_2
I.	A	...	16·289	0·931	27·216	1·173	28·389
	V R	...	8·217	0·951	32·159	2·102	34·260
II.	A	...	12·083	1·108	25·726	1·377	27·103
	V R	...	4·389	1·080	32·872	1·532	34·400
	V B	brighter	4·680	1·318	30·078	1·452	39·530
III.	A	...	16·058	...	28·685	0·572	29·257
	V R	...	3·744	1·200	37·130	1·293	38·423
	V B	brighter	1·514	2·208	38·903	1·615	40·518
IV.	A	...	13·178	...	32·635	1·018	33·653
	V R	...	5·971	1·222	36·687	1·150	37·837
	V B	...	2·154	...	41·153	1·419	42·572
V.	A	...	17·334	1·636	24·204	0·341	24·545
	V R	...	7·500	1·364	31·036	0·550	31·586
	V B	darker	1·265	0·923	34·443	0·438	34·881

It thus appears that 100 vol. of venous blood returning from muscles at rest contains on the average 6·71 vol. more carbonic acid than is contained in 100 vol. of arterial blood, whilst 100 vol. of venous blood returning from contracting muscles contain 10·79 vol. more CO_2 than 100 vol. of arterial blood. On the contrary, the venous blood of quiescent muscles contains 9 per cent. less oxygen than arterial, whilst in the venous blood of contracting muscle the quantity of oxygen may not exceed 1 or 2 per cent., arterial blood containing from 12 to 17·3 per cent. by volume of that gas.

3. Edenhuizen has investigated anew the effects of suppressing the action of the skin in various animals by covering them with different kinds of varnish. The experiments were made upon sheep and rabbits, a dog, a weasel, a mouse, a pigeon, and two frogs. On completely covering the animal, the duration of life was usually longer in proportion to its size, providing it was strong and healthy. If more than from one-eighth to one-sixth of the body were covered in rabbits, they quickly died. Immediately after the operation, a temporary increase in the frequency of the respiratory acts, of the pulsations of the heart, and of the temperature of the body was usually noticed; but in a short time these all fell below the normal standard—at first rapidly, and subsequently more slowly, though uninterruptedly, till death supervened. Other symptoms which appeared when a considerable portion of the skin was varnished were, restlessness, strong rigors, dyspnœa, paralysis, tonic and clonic convulsions, languor, and the secretion of a considerable quantity of albuminous urine. On keeping a

small part of the skin permanently varnished, it became red, and suppuration occurred in the whole thickness of the corium, whilst in the interstices of the areolar tissue numerous crystals of the triple phosphate of ammonia and magnesia were contained. Similar crystals were found after death in the peritoneal cavity of animals that had died from the application of an entire coating of varnish. Hence it seems probable that ammonia is eliminated by the skin, and experiments made with hæmatoxylon-paper showed that, under normal conditions, there is an escape of volatile alkali. It is the prevention of the excretion of the ammonia that occasions, according to Edenhuizen, the morbid appearances seen in animals thus killed—viz., congestion of the brain, lungs, liver, spleen, and kidneys; effusion into the pleura, peritoneum, pericardium, and the subcutaneous areolar tissue; and lastly, the ecchymoses so frequently seen in the mucous membrane of the stomach.

II. LYMPH, CHYLE, AND BLOOD.

1. C. LUDWIG: *On the Origin of the Lymph.* (Medizin Jahrbüch., v. A. Duchek and A. Schauenstein, Wien. 1863. Heft iv. pp. 35-72).
2. H. SCOUTETTEN: *Recent Experiments made with the view of establishing the Electricity of the Blood, and of Measuring its Electromotor Force.* (Gazette Hebdomadaire, tom. ix. 1863, p. 769.)

1. In this memoir the essential results of the investigations undertaken by C. Ludwig, and his friends Herren Noll, Krause, Schwanda, and Tomsa, are given. Ludwig remarks that the considerable pressure under which the lymph is constantly flowing, together with the sudden variations which are observed in the rapidity of the current, are circumstances opposed to the ordinarily-received notion, that it proceeds from the osmosis of the surplus material which has been effused from the bloodvessels for the nutrition of the tissues; and that although it has been customary to attribute the movement in part, also, to the pressure of the blood, yet that it is difficult to understand how this force can act if we admit with most writers on the subject three sets of spaces or cavities—namely, one belonging to the bloodvessels, a second formed by the lacunar spaces intervening between the fibres of the connective tissue in the different organs, and a third represented by the cavity of the lymphatic vessels—for it is inexplicable that the fluid effused from the blood should enter the closed system of the lymphatic system rather than percolate through the splits and fissures of the areolar tissue. The researches of Noll, based on these considerations, led him to give a different interpretation of the mode of origin of the lympathic vessels to that usually accepted; and he believes that he can show by injections forced through those vessels in a retrograde direction, that they actually commence, not by closed free extremities, nor by loops, nor by a plexus, but in the interspaces of the connective tissue itself, and that, consequently, the bloodvessels may be regarded as in reality distributed in the interior of the lympathic system, and the fluids effused from the blood, as entering directly the rootlets of the absorbent vessels. In order to render the finest lymphatics of any part visible, Ludwig recommends that the chief lymph-vessels should be exposed and tied during life. On killing the animal a few hours later, the most minute branches, now turgid with fluid, are discoverable, and may readily be injected, though great care and gentleness in the operation are required. The testicles are well adapted for experiments of this nature, since there is no intervening material between the tubuli seminiferi and the bloodvessels besides the fibrillar connective tissue; and it can there be readily shown that the lacunar spaces, splits, and fissures of that tissue open into more definite channels lying be-

tween the several layers of membrane which form the septa of adjacent lobuli; these again, by their union, begin to form sinuous vessels, whose walls are, however, still formed by a condensation of the connective tissue alone. Ultimately these become perfectly defined and cylindrical, with proper walls, and, on reaching the surface of the organ, are provided with valves, and attach themselves to the bloodvessels which they accompany in their farther course. He compares the arrangement of the channels in the tunica albuginea and septa to that produced by making a series of openings through a quire of paper, and then shifting some of the leaves, by which means passages are formed that, after running in a straight course through two or three pages, terminate, as it were, in the split between two adjoining sheets, which again open at numerous points into other passages running in the former direction. On injecting fluids into the lymphatics of the testis it may clearly be seen to penetrate between the bloodvessels, the tubuli seminiferi, and the fibrils of the connective tissues, so that these constituents of the gland are on all sides surrounded by the fluid. This account, therefore, closely coincides with that given by His and Brücke of the origin of the lymphatics in the intestinal canal, substituting only the tubuli seminiferi for the Lieberkühnian follicles. Similar investigations upon the kidney and on the cornea led to the same conclusions. In reference to the movement of the fluid in the lymphatics, he remarks that if any hypothesis on the subject be admissible, we must probably attribute it to the difference in the pressure of the blood contained in the bloodvessels as compared with the fluid (lymph) contained in the lymph spaces. The testicle is again, on account of the simplicity of its vascular and lymphatic systems and the slight operation required to expose the vessels, admirably adapted to show the effects of artificial variation of pressure in the veins on the amount of lymph produced. Tomsa's experiments on this point showed that the application of moderate pressure to the veins of the plexus pampiniformis was followed by an increased discharge of lymph; in three instances, amounting on the average to 59 per cent., on increasing the pressure, it rose in five animals from 30—1000 per cent., and on the average 370 per cent.—that is, that 3·7 times more lymph flowed than before. If, however, the pressure was increased till the current of blood was altogether stopped in the veins, the lymph first became bloody, and the flow then altogether ceased. The effects of active congestion, produced by section of the sympathetic nerve, were observed in the neck; and it appeared that though, in some instances, the amount of lymph discharged by the large lymphatic vessels was considerably increased, in others but little effect was perceptible. Other modes of inducing congestion of various parts, as by applying cantharides, produced no remarkable increase in the flow of lymph. It was found that, on diminishing the pressure of the fluids in the lacunar spaces of the connective tissue, as by firm but intermittent pressure of the head and face of a dog with the hands, and stroking the tissues so as to evacuate the lymph, much more fluid was discharged; whilst by adopting means that increased the pressure in those spaces less was obtained.

As regards the influence of the nerves, it has been shown by Krause that irritation of the portio dura caused an increased flow of lymph from the lymphatics of the face; and the same result was obtained by Schwanda on irritating the interior of the mouth till pain was produced. In similar experiments, to determine the influence exerted by the nerves upon the flow of lymph through the testicles, it was found by Tomsa that irritation of these nerves produced no increase. The difference between these results and those obtained by Krause, are, no doubt, attributable to the double action of the muscles and the valves in the experiments of the latter observer upon the lymphatics of the face.

Various facts tend to show that the lymph may be regarded as essentially a *filtrate* from the blood: for, in the first place, the proportion of albumen in it is considerably less than in the blood, which is in accordance with the known difficulty of albuminous fluids to traverse animal membranes; and again, the proportion of albumen diminishes with increasing temperature, and increases with increasing pressure; and it is remarkable that the effused fluid more closely approximates to the circulating when the pressure is steady and continuous, than when variable and intermittent. Moreover, Tomsa's experiments showed that, by injections of serum at the ordinary pressure of the blood after death, through the bloodvessels, a filtrate is discharged from the lymphatics with about the same rapidity as it flows naturally. The serum used in Tomsa's experiments contained from 6·77 to 6·26 of solid residue, that obtained from the lymph-vessels 6·12 to 4·86 parts in 100, therefore, from 1·90 to 0·65 per cent. less; and this is about the percentage residue of ordinary lymph. The rapidity with which the pressure of the blood in the bloodvessels reattains its ordinary height after venesection, is due, according to Ludwig, to the ready communication which exists between the bloodvessels and lymphatics, the fluids from the latter immediately re-entering the cavity of the vascular system when the pressure in the latter falls below that of the fluids contained in the lymphatic system.

2. In Scoutetten's experiments, a large, wide-mouthed porcelain vessel was half filled with venous blood; into this a porous vessel was introduced, containing arterial blood. Another small vessel, containing a saturated and neutral solution of sulphate of zinc, was immersed in each kind of blood, and into these solutions two electrodes, made of amalgamated zinc, and connected with a galvanometer, were plunged. The needle of the galvanometer, on the completion of the current, was powerfully deflected, and remained for nearly an hour at the 66th degree. In another experiment, it was shown that arterial blood is positive in relation to venous. And in a third, it was ascertained that the electromotive force generated by the contact of the two kinds of blood might be represented by 1·82, if 58 represented that of a Daniell's cell—i.e., 100 being the electromotive power of pure zinc.

III. Nervous System.

1. Gluge and Thiernesse, MM.: *Experiments on the Reunion of Sensory with Motor Nerve-fibres.* (Bulletin de l'Academie Royale de Belgique, vol. xvi. ser. ii. No. 7, 1863, p. 65.)
2. Setschenow, J.: *Physiological Investigations upon the Mechanism in the Brain of the Frog for exerting an Inhibitory Influence upon the Reflex Activity of the Spinal Cord.* (Pamphlet, Berlin, 1863, pp. 49.)
3. Valentin, G.: *Upon the Development of Heat during Nervous Action.* (Virchow's Archiv, Band xxviii. 1863, p. 1.)
4. R. Vigouroux: *On the Influence of Respiratory or other Violent Movements of the Body on the Motions of the Iris.* (Gaz. Medicale, 1863, p. 664.)
5. V. Kempen and Thiernesse: *Upon the Functions of the Roots of the Nervi Vagi and Accessorii.* (Bulletin de l'Academie Royale de Med. de Belgique, 1863, tom. vi. p. 184.)

1. The experiments of MM. Gluge and Thiernesse were made to determine whether the functions of nerves are inherent in themselves, or whether the effects produced depend exclusively upon the nerve-centres from which they spring, and the structures to which they are distributed. Their former experiments led them to admit the first alternative—viz., that a sensory fibre could only convey impressions centripetally, and a motor one centrifugally, and that consequently it would be impossible to convert a sensory into a motor fibre. MM. Phillipeaux and Vulpian[*] found, on the contrary, in their experiments,

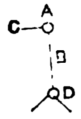
[*] C. Rend., January, 1863.

that when the cut centric extremity of the lingual nerve was made to unite with the cut peripheric extremity of the hypoglossal nerve, on repeating the section of the lingual after an interval of some months at a higher point, contractions occurred on the corresponding side of the tongue, when the peripheric extremity of the lingual (now united to the hypoglossal) was mechanically irritated, the fibres of a sensory nerve thus conveying a motor impulse. In consequence of this statement, Gluge and Thiernesse took two dogs on the 29th of January and 6th of February, 1863, and, after division, applied the opposite ends of the hypoglossal and lingual nerves to one another. The animals recovered from the operation without other effects being perceptible than that the tongue, when projected, was pushed to the side on which the operation had been performed. Both animals were killed on the 3rd of June by section of the spinal cord. In dog No. 1 the nerves were perfectly united, and the lingual, being now divided above the cicatrix, the peripheric extremity which had united to the hypoglossal, was pinched; but no contractions of the muscles of the tongue were observed. The application of an electrical current, however, immediately induced contractions. On the opposite sides contractions were readily obtained by mechanical irritation of the hypoglossal nerve. In dog No. 2 the reunion was found to have also been perfected. On pinching the lingual nerve three minutes after death, strong contraction of the muscles of the tongue occurred; but when the lingual nerve was divided, and the peripheric extremity irritated, no movements whatever occurred. Compression of the cicatrix, and consequently of the peripheric extremity of the hypoglossal, occasioned vigorous contractions. The conclusion is therefore obvious, that the former movements were due to the reflex action of the sensory fibres of the lingual. MM. Gluge and Thiernesse, therefore, still maintain that the sensory fibres can never be rendered capable of conveying motor impulses, and *vice versâ*.

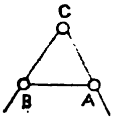
2. In order to render the following investigations of Setschenow intelligible, it may here be premised that he proceeds on the assumption that in all *vertebrated* animals, at least, a certain inhibitory power is possessed over the reflex action of the spinal cord, whereby the movements which would naturally occur as the result of that reflex action are restrained or checked. That some such influence is ordinarily exerted seems to be proved by the much greater readiness with which the lower limbs of frogs, for instance, respond to direct irritation *after* division of the spinal cord in the dorsal region than in the uninjured animal. Setschenow endeavours to prove that this inhibitory influence has a local habitation, and that there is, in fact, an inhibitory centrum or mechanism in the brain. He considers that this is established for the frog, 1st, by the results of section through various parts of the brain and medulla oblongata; 2ndly, by the effects of chemical or electrical excitation of different parts of the brain; and 3rdly, by the results of the physiological excitation of the brain. The agent employed for exciting the reflex actions was, in all instances, a dilute solution of sulphuric acid, applied to the skin of the hind feet; and the rapidity with which the limb responded to the irritation was determined by a metronome. In order to establish a standard of comparison for the results of sections of the cerebral mass, he invariably, in the first instance, divided the hemispheres transversely, apparently with the object of abrogating the influence of the will, and reducing all the animals experimented on to a common level; and he considers the rapidity and energy of the reflex actions which can then be excited as *relatively normal*. On comparing the results of sections made, 1st, through the thalami optici, 2ndly, just behind the corpora quadrigemina, and, 3rdly, through the lower part of the medulla oblongata, and further, on comparing the results of chemical and electrical excitation of the cut surfaces, he arrives at the conclusion that the inhibitory influence possessed by the cerebral masses of the frog on the reflex movements of its limbs has its seat in the optic thalami, corpora quadrigemina, and perhaps also in the medulla oblongata, and is to be regarded in the light of a true centrum, and that no

inhibitory influence is exerted by the spinal cord itself. The activity of this nervous centre is called into play by sensory fibres (alone?).

He then proceeds to consider the nature of the inhibitory influence, and remarks that there are three modes in which it may be conceived to act—1. By depressing the sensibility of the sensory nerves; 2. By rendering the transference of the impression from the sensory to the motor fibre more difficult; and 3. By depressing the conductivity of the motor nerve. In order to determine which of these modes actually occurs, he endeavours to ascertain what influence was exerted upon the conductivity of the motor fibres, and upon the irritability of the sensory fibres of the skin when the inhibitory centrum was called into action, by the following ingenious method. He selected an eminently ticklish man, and having previously ascertained the exact period which elapsed before the hand dipped in acid perceived any sensation, he directed him to restrain (or exert an inhibitory influence upon) the usual reflex movements which followed on the act of tickling. The results which he uniformly obtained were, that during the powerful exertion of the inhibitory influence, the time at which the sensation was first experienced was retarded—that is to say, the sensibility of the skin was depressed; the objection which might naturally be raised to this experiment—viz., that the weaker sensory impression of the acid is lost in the stronger one produced by the tickling—he meets by stating, that on repeating the experiment himself, and imitating the movements made by the ticklish man in restraining the natural reflex movements, such as compression of the teeth, powerful contractions of the muscles of the chest and belly, he obtained exactly the same results, and from this he was led to consider that the violent reflex movements which occur in all animals from pain, by calling on the inhibitory centres to act may be a merciful provision for the diminution of suffering. The mode in which he believes the brain acts in diminishing the reflex activity of the spinal cord is applicable to other cases—as the heart, intestines, &c.—and may be explained by the following simple diagram, where A is a ganglion cell in the medulla oblongata, from which a fibre of the pneumogastric nerve proceeds, B, and in which a sensory fibre, C, terminates. A is an inhibitory centre, and B an inhibitory nerve-fibre; the latter terminates in a nerve-cell, D, from which two other nerves proceed, and D is one of the nervous ganglia through which the ordinary automatic movements of the heart are effected. Here irritation of the nerve B at any height is followed by an inhibitory influence exerted upon the heart. The only differences between this case and the inhibitory influence exerted by the brain on the spinal cord are that the fibres corresponding to B in the above diagram run in the nervous centres themselves instead of being detached, and that their excitation, instead of being followed by a visible effect (as the stoppage of the heart), is productive of no obvious, or merely negative, results.



The cause of the greater rapidity and energy of the reflex movements of the spinal cord which occurs when the animal is beheaded, or after the removal of its cerebral masses, may also be readily explained. Thus, if A B be a connecting fibre passing from a sensory ganglion cell, A, to a motor cell, B, both contained in the spinal cord, the reflex action will take place through the straight line, A B, where it is limited to the spinal cord, as in the beheaded animal; but it is conceivable that, under ordinary circumstances, the sensory impression arriving at A, may be broken into an influence passing as before in a straight line from A to B, and into another influence or impulse, proceeding to the inhibitory ganglion cell, C, situated in the medulla oblongata, which may again react upon the motor cell, B, and materially modify the nature of the reflex movement performed by the animal. A section carried through the cord in such a manner as to cut off the influence of the ganglion cell, C, would have as its effect the discharge of the force generated in A,



through the straight path, A B, and would therefore render the reflex action more energetic and sudden than before.

3. Valentin commences his essay by remarking that Helmholz concluded, from his experiments, that the amount of heat generated during nervous action, if indeed there were any, was almost imperceptible as compared with that produced by the muscles, and certainly did not exceed a few thousandths of a degree Centigrade. But since the nerves when at rest absorb oxygen and eliminate carbonic acid, and show also many analogies to muscle in their electromotory properties, it is only natural to expect that they would, like the muscles, develop heat when their activity was called into play. This mode of reasoning might, however, be easily invalidated by two circumstances, for whilst in the case of the muscles we know that during contraction they change their form, though with scarcely any concurrent change of volume, and that the nature of the gases then evolved differs to a considerable extent from those given off when the muscle is at rest, there is no evidence except the negative variation of the magnetic needle to determine whether any analogous changes occur in the nerve. But it is probable that the change of form, and more rapid generation of carbonic acid, are the causes of the increased heat of muscle during action, and as these conditions may not be present in nerves when called into action, the above is clearly not a logical conclusion. In order to determine whether any appreciable amount of heat is generated, Valentin employed an extremely delicate magnetic needle, and made various experiments on frogs with a sensitive thermo-electric apparatus, enabling a variation of $\frac{1}{2222}$ of a degree Cent. to be recognised. The nerves (sciatic plexuses) were usually excited to action by galvanism applied to the spinal cord for the space of thirty seconds. Immediately after the application of the stimulus there was evidence of the generation of heat. On cessation of the stimulus the needle vibrated for some time, and then came to rest; but could again be made to deviate from its quiescent position by a fresh application of the stimulus, and this could be repeated three or even four times, though more heat was always generated on the first and second application of the stimulus than subsequently. The following table shows the results he obtained in a good experiment upon a frog:

Galvanization.	Duration of the same in seconds.	Warmth at the end of galvanization in C. degrees.	Difference from the last observations.	Time after the end of galvanisation in minutes.	Heat in Cent. degrees.	Difference from the last observation.
1st	90	0·021	+0·021	—	—	—
				1	0·025	+0·004
				6	0·054	+0·029
2nd	30	0·064	+0·010	—	—	—
				4	0·068	+0·004
				6	0·071	+0·003
				8	0·080	+0·009
				12	0·084	+0·004
3rd	30	0·087	0·003	—	—	—
				2	0·088	+0·001
				3	0·089	+0·001
				5½	0·090	+0·001
4th	30	0·092	0·002	—	—	—
				1	0·092	0·000
5th	30	0·094	0·002	—	—	—
				1	0·094	0·000
				5	0·096	0·002
				7	0·098	0·002
6th	30	0·099	0·001	—	—	—

From this table it appears that the greatest elevation of temperature occurred after the first application of the stimulus, which lasted for ninety seconds. Each of the subsequent applications produced less and less effect, until, on the sixth occasion, the elevation did not exceed 1–1000° C., which was a clear proof that the elevation originally noted was not due to any internal changes of the mechanical apparatus itself. It is further remarkable that the increase in temperature did not attain its maximum immediately on the cessation of the irritation (see col. vi.), but continued to rise gradually, and in the first instance considerably for some time. Similar experiments were made with similar results on other frogs, substituting mechanical for chemical irritation. Hence it appears that either electrical or mechanical irritation of the spinal cord effects a marked elevation in the temperature of the sciatic plexus. The subsequent rise of temperature is attributable to radiation, and also to conduction of heat from the spinal cord itself, and from those parts of the sciatic plexus which are not in direct contact with the point of junction of the antimony and bismuth in the thermo-electric apparatus, and also from the muscles thrown into contraction by the irritation of the nerves.

4. R. Vigouroux has ascertained that every energetic muscular movement either of the body generally, of inspiration, or of expiration, coincides with a dilatation of the pupil, varying in amount from one-fifth to one-third of its previous diameter; and he conceives the explanation to be this—that whenever a nervous centrifugal current traverses the spinal cord at the level of the origin of the first two pairs of dorsal nerves, a portion of the current is derived towards the pupillary filaments which separate from these nervous trunks, and effect the contraction of the radiate fibres of the iris.

5. Three series of experiments were undertaken by M. v. Kempen. In the first he cut successively the roots of the pneumogastric and those of the spinal accessory nerves at their origin from the medulla oblongata. The irritation produced by the section of the former caused contraction of the œsophagus and of the constrictor muscles of the pharynx, whilst the section of the latter made immediately afterwards only caused contraction of the sterno-mastoïd and trapezius muscles. The same results were obtained on pinching or otherwise irritating the distal cut surface of the roots of these two nerves. In a second series of experiments, the intrinsic muscles of the larynx were exposed, and the roots of the accessory were first irritated, and then those of the pneumogastric nerve. In both cases contractions were observed to take place in the œsophagus, in the constrictor muscles of the pharynx, and in the intrinsic muscles of the larynx, and in addition, when the spinal accessory roots were irritated, in the trapezius and sterno-mastoïd muscles. These results at first sight appear to be in contradiction to those obtained in the former series of experiments in which the section of the roots of the spinal accessory at their origin from the medulla oblongata called forth no movements in the muscles of the pharynx or larynx, but it must be remembered that this section was practised in the former instance after the roots of the pneumogastric had been divided, whilst in the second series of experiments the roots of neither of these nerves were cut. These experiments consequently lead to the conclusion that those contractions of the intrinsic muscles of the larynx, of the pharynx, and of the œsophagus, which occur on irritation of the spinal accessory nerve alone, are due to a reflex action on the pneumogastrics; the spinal accessory thus containing centripetal or sensory fibres, and the communication between this nerve and the pneumogastric being effected by a chain of ganglionic nerve-cells situated in the medulla oblongata. This view has been fully corroborated by M. v. Kempen in a third series of experiments, in which the supposed communication in the medulla between the two nerves was divided by a transverse section carried between the origins of the pneumogastric and spinal accessory nerves. It was then found that on irritation of

the posterior cut surface of the medulla oblongata, and on pinching the roots of the spinal accessory, no movements occurred except in the sterno-mastoïd and trapezius, whilst, when the same irritation was applied to the anterior extremity of the medulla oblongata or to the roots of the pneumogastric, contractions occurred in the other above-mentioned muscles. V. Kempen's experiments, therefore, show that both the vagus and the spinal accessory are mixed nerves, possessing motor and sensory fibres, but Longet long ago, also from very carefully-performed experiments, arrived at the conclusion that the vagus nerve is purely sensory, the stimulus applied being electricity. To reconcile this difference, Thiernesse repeated the experiments of M. v. Kempen, and obtained similar results to those already recorded, and he therefore agrees with him in regarding both the pneumogastric and spinal accessory as nerves of mixed endowments.

IV. MUSCULAR SYSTEM.

1. F. COHN: *On the Contractile Filaments of the Cynareæ (Thistle tribe.)* (Quarterly Journal of Microscopical Science, 1863, p. 186, and Annals of Natural History, 1863, p. 188: translated from Siebold and Kölliker's Zeitschrift für wissens. Zoologie, Band xii. p. 366.)
2. Dr. SOLGER: *Upon the Production of Heat during Muscular Action.* (Studien des Physiolog. Instituts zu Breslau, 1863, p. 125.)
3. Dr. MEYERSTEIN and Dr. L. THIRY: *Upon the Relation between the Amount of Heat generated in Muscle and the Amount of Work done by it.* (Henle und Pfeuffer's Zeitschrift f. rat. Med., Band xx. Reihe iii. 1863, p. 45.)
4. H. F. BAXTER: *On Muscular Power.* (Edinburgh New Philosoph. Journal, vol. xviii. p. 194.)

1. At an early period of flowering in the thistle tribe, Cohn remarks that when the anther tube, consisting of five cohering anthers, rises about four millimetres above the summit of the corolla, and for a period of about twenty-four hours, if touched, pollen masses extrude from the apex, and at the same time the tube exhibits a peculiar twisting movement. Microscopical examination of the filaments shows them to consist of a central vascular bundle containing annular and closely wound spiral vessels, surrounded by rows of elongated cylindrical cells, and covered by an epidermis and a thick cuticle. The elongated cells appear to be longitudinally striated, but in the state of contraction they present close transverse striæ, due to the cells, in the act of shortening, becoming very regularly and closely wrinkled. It would seem, therefore, that these contractile cells closely resemble the involuntary muscular fibre-cells of animals; and we may now be said to be acquainted with plants in reality furnished with muscles. Cohn comes to the conclusion that these cells are normally in a state of active extension, and that the shortening depends upon a relaxation of muscular effort, so that the elasticity which had acted as an opponent to the elongating force is called into play and effects the contraction—a view which supports Dr. Radcliffe's theory of muscular action.

2. In this paper Dr. Solger describes briefly a thermo-electric instrument which he believes to be peculiarly adapted to investigations of this nature, and gives a table of experiments performed on a frog, from which he draws the conclusion that a muscle in contracting is not, in the first instance, warmer, but colder than before—a phenomenon which he terms the "negative calorific variation." If, however, it be made to contract persistently (tetanus), heat is generated, so that ultimately the original temperature of the muscle is surpassed; and there is this further peculiarity of the effects of tetanus, that the development of heat does not cease when the muscle has, in consequence of the cessation of irritation, regained its original length. On the contrary, heat

continues to be developed; and it is to be remarked that the amount of this secondary or "subsequent development" of heat diminishes with increased exhaustion of the muscle, and also that it is much less in the muscles of frogs experimented on in winter than in autumn. The experiments are in course of repetition by Heidenhain himself, with apparatus of a still more delicate construction.

3. Meyerstein and Thiry's researches were undertaken with the view of determining whether the amount of heat given out when a muscle contracts continuously against a heavy weight, and when therefore it remains elongated, is the same as that which is generated when the weight is light, and when consequently the muscle can readily contract, the former being called static, the latter dynamic exertion. The experiments were made uniformly on the gastrocnemius of the frog, and the relative increase of temperature was ascertained by a very sensitive thermo-electric apparatus, of the construction of which full details are given. They also observed that peculiar cooling of the muscle described by Solger, which takes place during the first three or four seconds after the commencement of contraction, and which is constantly succeeded by an elevation of temperature. They noticed that this phenomenon was usually more remarkable in weak than in strong muscles, and in the latter the depression of temperature was greater the smaller the weight that was lifted by the muscle; indeed, in many instances it attained its maximum when no weight at all was appended. In some cases the muscle never regained its original temperature during the whole time it was made to contract, which was ten seconds. This primary depression of temperature, they think, may with great probability be attributed to the contracted muscle possessing a less specific heat than when in the state of elongation; and they argue that it is only natural to expect some such change when we recal the alteration of the other physical and chemical properties which the muscle undergoes during contraction. The elevation of temperature which followed the first effect of cooling usually continued for some seconds after the muscle had ceased to contract, and is attributed by these observers to the chemical changes (oxidation) taking place in the substance of the muscle; but they do not mention, what seems to be of importance, the increased flow of blood through the muscle which occurs after contraction. The primary depression of temperature makes it exceedingly difficult to express in figures the actual amount of heat developed by any muscle in the act of contraction. They arrive at the conclusion that the amount of heat generated increases or decreases in direct proportion to the heights to which the weights were raised; successive experiments on the same muscles, agreeing very perfectly in their results, though it was found to be impossible to compare the effects produced by the contraction of different muscles. And thus it may be said that in any case, if double the amount of heat be generated, twice as much work has been performed; and if an equality of heat be produced, the muscle has performed the same amount of work, however it may have been exerted, whether in traction against a heavy weight, or in lifting a light weight.

4. In this paper, Mr. Baxter considers, 1st, the mechanical power exerted by muscles during contraction; and 2ndly, the application of the principle of conservation of force or energy to the explanation of physiological phenomena. For the determination of the first point, the gastrocnemius of a living frog was employed, still attached to the animal, but paralysed by division of the sciatic. The means used to excite the nerve was the current of electricity from a single cell of medium size (Smee's construction). The conclusions at which he arrived were: 1. Out of 60 frogs taken indiscriminately, the average results of the experiments show that 1 grain of muscle is capable of raising a weight of 608 grains through a space of 1-53rd of an inch. 2. That sex has an important influence over the results. In males, it was found that 1 grain = 656 grains,

whilst in females 1 grain = 579 grains. 3. That the weight of the animal previous to the experiment gave no indication as to the real muscular power of the animal that could be relied on. 4. That during the experiment there was a loss of weight in the animal, but an increase of weight in the muscle experimented upon. 5. That an increased nutrition of the muscle was indicated by its weight, its vascularity, increase in the activity of its electrical condition, and by the maintenance of its muscular power. 6. That circumstances influencing the health of the animal, such as absence of food, temperature, and confinement, have an important influence over the results. 7. That the maintenance of the circulation of the blood is of the utmost importance in these experiments.

SUMMARY.

For the analysis of the following works and papers we have not space:

Elements of Human Physiology. By L. Hermann. (Berlin, 1863, pp. 412.)

Lectures on Physiology and Comparative Anatomy, By H. Milne-Edwards. (Vol. viii. part 1. Paris, 1863, pp. 236.)

The Antiquity of the Human Race; three Popular Lectures. By N. T. Schleiden, M.D. (Leipzig, 1863, pp. 62.)

On the Activity of Matter in relation to Organization and Life. By P. Dupuy. (Gazette Médicale, 1863, pp. 465–479.)

Note on the Law of Transmutation of Forces, considered in its Application to Animal Physiology. By M. Leotard, M.D. (Gazette Médicale, Paris, 1863, p. 598.)

Researches on the Development of the Tissues in the Warm-blooded Animals. By C. W. L. Bruche. (Part 1. Frankfort, 1863; with 6 Plates and 28 Woodcuts.)

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On the Effect of Temperature on the Excretion of Urea, as observed on a Voyage to China and at Hong Kong. By E. Becher, M.D. (Proceedings of the Royal Society, vol. xii. p. 440.)

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HALF-YEARLY REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.

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I. *On Glycerine, and its Applications to Medical and Surgical Treatment.* By Dr. Démarquay.

Dr. Démarquay has lately published a book on the therapeutical applications of glycerine. This substance has been used internally as a laxative, but its aperient effects are more evident when employed as an enema, in the proportion of two ounces of glycerine to sixteen of water. Fetid and gangrenous ulcers are modified by glycerine, and rapidly assume a healthy aspect, if the dressings are changed two or three times a day. It forms a good dressing for malignant carbuncle, and in cases of burns it imparts to the injured surfaces a permanent sensation of coolness, due to its hygrometric properties. It is also a useful adjunct in the treatment of scorbutic, scrofulous, and syphilitic ulcers, and a valuable palliative in cancer. It possesses the property of dissolving iodine, and an injection of an ounce of iodine and three ounces and a half of glycerine has been found very efficacious in cases of deep-seated abscess, sinuses, scrofulous wounds, syphilitic bubo, &c. In diseases of the skin, glycerine is often more successful than pommades, as for instance in vulvular hyperæsthesia; in pityriasis capitis a combination of hydrochlorate of ammonia, glycerine, and rose-water, as prescribed by Dr. Gueneau de Mussy, has been found very efficacious; and a sulphur pommade for scabies, made with glycerine instead of lard, possesses the advantages of not staining the linen and being free from offensive odour. Dr. Démarquay gives numerous formulæ, containing glycerine, applicable to the treatment of diseases of the eyes, ear, mouth, fauces, and larynx. In thrush, stomatitis, and ulcerative sore throat, M. Blache prescribes an application consisting of two drachms of biborate of soda and one ounce of glycerine.

II. *On the Treatment of Tendinous Rheumatism by the External Employment of Sulphur.* By Dr. Renard. (L'Union Médicale, April 21st, 1863.)

Tendinous rheumatism, according to Dr. Renard, differs from acute rheumatism by the absence of the general symptoms, and from the chronic by the presence of local inflammatory symptoms. Dr. Renard suffered from this complaint himself after an attack of acute rheumatism, for which he was copiously bled. The parts affected were the tendons of the hamstring muscles, and no improvement resulted after a long course of diaphoretics, camphor, terebinthinate and other liniments, and the administration of the solanaceæ. At last Dr. Renard saw a passage in an English medical journal, stating that persons suffering from rheumatism in the legs had only to dust the inside of their stockings with sulphur. He immediately employed this simple remedy, the sulphur being the commercial flowers of brimstone, which contain some sulphurous acid. The curative effect was very well marked, for Dr. Renard walked in the evening, then renewed the sulphur in the stockings before sleeping in them, found himself very much relieved the next morning, and nearly quite cured on the morning after. A few days later, he left off the brimstone, and the pain reappeared in the soles of the feet, but yielded very soon to the reapplication of sulphur. Since the year 1857, when he was first attacked, the same experiment was repeated every winter when he was suffering from chronic tenodynia, either in the hams, the heels, or the elbows. He felt, under the influence of the contact of the flowers of brimstone, the skin becoming hotter, slightly excited, and more disposed to sweating; and as soon as this effect was produced, the relief of the pain seemed to be immediately marked. Whatever may be the explanation of the manner in which sulphur exerts its curative agency, Dr. Renard affirms that it has a beneficial effect upon the rheumatic pains of the tendons, and that this action is the more rapid and certain in proportion as the tendons are more superficial and the sulphur is kept more closely over the painful parts.

III. *On the Use of Nitrate of Silver in the Paraplegia of Children.* By Dr. Bouchut. (Bull. Gén. de Thérap., Jan. 30th, 1863.)

Dr. Bouchut employed the nitrate of silver internally in the case of a child, aged seven years, in the Hospital of Sainte Eugénie. The patient had had a fall from a height of a few feet, and immediately complained of acute pain in the dorsal region. From this time the child was unable to walk, and when she was placed upright the legs bent and sank down under the weight of the body. The speech became slow, difficult, and indistinct, and the food partly escaped from the mouth during mastication. For nearly a month, only the expectant treatment was adopted; but Dr. Bouchut then conceived the idea of treating the paralysis with nitrate of silver, according to the views of Wunderlich, and Charcot and Vulpian. He therefore prescribed one centigramme of the nitrate, divided into two pills, to be taken every day; and this treatment (occasionally varying the dose) was continued for more than a month with success, for at the end of this time the child left the hospital perfectly cured. Dr. Bouchut remarks that this was a case of paraplegia from direct violence, depending apparently upon a state of commotion of the spinal cord, and that the use of nitrate of silver was attended with manifest advantage. The expectant treatment had been tried without any avail, but as soon as the nitrate was employed the improvement became apparent: in twelve days the child began to walk alone, and at the end of six weeks of the treatment the cure was complete. Although the nitrate of silver was successful in the present case, Dr. Bouchut thinks that it would not be a suitable medicine for cases of paraplegia in which there are symptoms of acute inflammation of the spinal cord or its membranes.

IV. *On the Employment of Tannin in the Affections of the Respiratory Organs, and principally in Pulmonary Phthisis.* By Dr. Woillez. (Bulletin Général de Thérapeutique, January 15th, 1863.)

Dr. Woillez has employed tannin in pulmonary phthisis, and also in some other affections of the respiratory organs, accompanied with hyper-secretion from the bronchial tubes. He believes that in the former case it occasionally exercises an undoubted influence, not only in the diminution of the mucous secretions obstructing the air-passages, but also in the evolution of the local tubercular lesion, as well as in improving the general health. In bronchorrhœa it apparently acts only on one element of the disease—namely, the abnormal hypersecretion.

In selecting his cases of bronchitis, Dr. Woillez has admitted only those which were well marked, and characterized by the principal distinctive sign of a subcrepitant rhonchus, occupying at least the base of both lungs behind. In such instances, the bronchial secretion becomes the prevailing feature as soon as the most acute phenomena are relieved, and the greater or less abundance of this secretion may account for the greater or less dyspnœa, and the frequency of the cough and expectoration. The first indication of treatment, therefore, is to diminish this catarrhal element, and thereby to relieve the functional symptoms, and to restrict the extent of the humid rhonchi heard on auscultation. Dr. Woillez administers tannin in the daily dose of four pills, each containing the ·15 or ·20 of a gramme, two to be taken twice a day at meals.

In reference to the efficacy of tannin in pulmonary consumption, Dr. Woillez considers that this drug has an undoubted influence in improving the general condition of the patient, and in inducing a favourable change in nutrition, and that its tonic action is comparable to that of cinchona. But he admits that this favourable action on the nutrition, and its reparative influence on the pulmonary lesion, are not constant, and he cannot accurately state the proportion of successful and unsuccessful cases which he has observed. Even in the advanced period of pulmonary consumption, Dr. Woillez has known the tannin produce a favourable result by arresting the symptoms in their progress, and in modifying them to such a degree as to produce an apparent cure. When tannin is given in the dose already described, to patients who have cavities in their lungs, not amounting to large excavations, it generally happens that the local signs are sensibly improved at the end of from eight to fifteen days. This amelioration is characterized by a marked diminution of the humid rhonchi. The blowing or cavernous respiration is afterwards clearer, as well as the bronchophony, and the rhonchi, sometimes less numerous, are perceived principally at the end of inspiration, or only at the moment of coughing. The principal conditions in which the treatment by tannin has seemed to fail in phthisis, are the continuance of fever, the rapidity of the course of the disease, and the existence of recent delivery in women: these circumstances seem to Dr. Woillez to make phthisis almost necessarily fatal.

V. *On the Simultaneous Employment of Perchloride of Iron and Ergot of Rye in Albuminuria.* (Gazette Médicale de Lyons, Oct. and Nov. 1862.)

Dr. Socquet, in the first instance, and afterwards Dr. Chatin, both physicians of the Hôtel Dieu of Lyons, have employed the perchloride of iron and ergot of rye for the prevention of the loss of albumen in the urine, and the results they have obtained are deserving of notice. The cases observed were some men of bad constitution, weakened by former unfavourable hygienic conditions, such as insufficient food, and dwelling in damp and badly-ventilated localities. The dropsy, in all the cases, at first confined to the face, had successively attacked the limbs and the peritoneum. The urine was pale and inodorous, and contained large quantities of albumen, and in one case microscopic examination revealed the presence of the remains of renal epithelium. Immediately on their admission into the hospital these men were subjected to diaphoretics, alkaline diuretics, uva ursi, and digitalis, though without any good result; but at last they took the ergot of rye and perchloride of iron. These medicines were given in progressive doses, beginning with 20 drops of tincture of the perchloride and 50 centigrammes of ergot of rye. Every two or three days these doses were methodically increased, and carried successively to 30, 40, 50, 60, 70 drops of tincture of the perchloride, and to 75 centigrammes, 1 gramme (about 15 grains), and 3 grammes of the ergot. Under this treatment the albumen in the urine rapidly began to diminish; in ten days it disappeared completely, and in ten days afterwards the different dropsical effusions disappeared also. In one of the cases, the treatment having been suspended a little too soon, the albumen again appeared in the urine. In order to judge comparatively of the effects of the perchloride and the ergot, the perchloride was administered alone, when the albumen diminished; but this diminution, although rapid at first, was afterwards very slow. The ergot being added to the prescription, accelerated the cure, and four days after its administration there was no more albumen in the urine. M. Perrond, in making some remarks on these cases, observes that the ergot and the perchloride of iron appear to have a beneficial effect on the *albuminuria*, but that their use constituted the treatment of a symptom rather than that of a disease, and that they are not therefore calculated to supersede the use of other measures intended to remove the original malady.

VI. *On the Employment of Large Doses of Digitalis.* By Dr. W. E. Bowman. (Canada Lancet, April 15th, 1863.)

Dr. Bowman doubts the correctness of the generally-received opinion as to the poisonous effects of digitalis, and he attributes many of the sudden deaths said to occur in dropsy to the accompanying disease, and not to the remedy. Dr. Bowman records two cases of delirium tremens in which he gave large doses of digitalis with good effects. The first was a man of fifty years of age, who had been a hard drinker for many years. Morphia was first administered, but it did not produce sleep. Digitalis was given, with the result of reducing the pulse from 100 to 56, of diminishing the tremor, and of calming agitation, and he recovered without any bad symptoms remaining. The second case was that of a man of forty-five years of age, who had frightful visions, and attempted to destroy himself by throwing himself out of a window. The treatment was begun by the administration of half an ounce of tincture of digitalis, which soon brought down the pulse from 80 to 64; and the same dose was repeated at short intervals for eleven hours and a half, and in eight days he recovered. This patient took two and a half fluid ounces of the tincture of digitalis in eleven hours and a half, the tincture being made from fresh and fine leaves. The first dose lowered the pulse eight beats, but it was again raised by the second dose, and the second, third, and fourth doses had no effect on the number of beats, but rendered them occasionally irregular. The 5th dose, however, brought down the pulse at once to 48, at which it remained most of the time for several days, producing no ill effects whatever, and merely rendering the patient languid.

[These cases show, what has long been believed, that persons may take large doses of digitalis without any bad effects; but from the manner in which the cases are related, it is doubtful whether they were cases of true delirium tremens.—Reporter.]

VII. *On the Internal Exhibition of Atropia and Strychnia.* By ALEXANDER FLEMING, M.D., F.R.C.P. Lond. (Edin. Medical Journal, Jan. 1863.)

Dr. Fleming has for several years employed solutions of atropia and strychnia for internal use, and he prefers them to the ordinary preparations of belladonna and nux vomica, on account of their greater safety and efficiency. The solutions of both alkaloïds employed by Dr. Fleming are so proportioned in strength that ten minims is the ordinary commencing dose, which easily admits of increase for the adult, and of diminution for the child. The solution of atropia is prepared from 1 grain of atropia, and 5 drachms of distilled water. The alkaloïd is to be thoroughly dissolved with the aid of a few drops of hydrochloric acid, and sufficient rectified spirit is to be added to make 10 drachms. This solution keeps well and is of uniform strength, and 10 minims of it, containing 1-60th of a grain of atropia, is the commencing dose for an adult. It should be given in a little water, once daily, at bedtime, and on an empty stomach. For children of one year, and all ages under one year, the commencing dose is 1 minim; of two years, 2 minims; of three years, 3 minims, and so on up to ten years, when 10 minims may be given. The diseases in which Dr. Fleming uses atropia are epilepsy, asthma, constipation, and hooping-cough. He uses it *once* a day, because the action of one dose does not subside completely for sixteen or eighteen hours; and if a second is given before the effects of the first have passed away, there is a risk of producing cumulative action. It should be given on an empty stomach, because the dose of atropia requires, for its due action, to be promptly absorbed; and when mixed with the contents of a full stomach it enters the system very gradually, and manifests its usual effects very imperfectly, or not at all. This is one reason why the drug, when taken into the stomach of the rabbit, has no action, for it always meets there a large quantity of food, and mixing with it, enters the system very gradually. Several experiments made by Dr. Fleming have satisfied him that this explanation accounts in some measure for the immunity of grass-feeding brutes from the effects of certain poisons, for their stomachs are always full. Atropia should never be given in pill, which may undergo solution very slowly or not at all, lest when two or three pills accumulate in the stomach or bowels, they may, from some change in the gastro-intestinal fluids, be suddenly dissolved and excite severe atropism.

The solution of strychnia is made with 2 grains of strychnia and 5 drachms of distilled water: the strychnia is to be thoroughly dissolved by means of a little diluted hydrochloric acid, and rectified spirit is to be added to make 10 drachms. This solution, like that of atropia, is uniform in strength, passes readily into the circulation, and the dose can be apportioned with accuracy. The commencing dose is 10 minims, and contains 1-30th of a grain of strychnia. When employed for its *tetanic* action, the solution should be taken in the morning, half an hour before breakfast, and in half an ounce of water, and the dose increased 2 or 4 minims daily until a slight degree of its physiological action, such as stiffness about the jaws or neck, or spasmodic movements in the paralysed muscles, is manifested, when no further increase should be made. It should be given only once daily, to avoid the risk of cumulative action; it should be taken in the morning, so that its action may be over before bedtime and the sleep be not disturbed; and it should be given on an empty stomach and diluted with water, to ensure its prompt and easy absorption. Strychnia should never be given in pill, for it is hard of solution in the weak acids of the stomach, and several pills may remain unchanged and accumulate there, or in the bowels. When the strychnia is employed as a *tonic*, the dose of the solution is 5 minims, and it may then be exhibited twice daily with safety and advantage.

VIII. *On the Treatment of Neuralgia by Common Tincture of Iodine and Morphinized Tincture of Iodine.* By M. Bouchut. (L'Union Médicale, July 21st and 23rd, 1863.)

M. Bouchut relates the particulars of sixteen cases of neuralgia seated in different superficial nerves, all quickly cured by applications of common and morphinized iodine. He employs this substance as a revulsive, and he considers it a milder and more manageable caustic than the greater part of the revulsives employed up to the present time, and it therefore deserves to be classed among the local remedies for neuralgia. He has used it in all kinds of neuralgia, but it is difficult to employ it in cases of neuralgia of the face or in hemicrania, in consequence of the yellow colour which it imparts to the skin. Hence he uses it in such cases only when he is able to conceal it in the eyebrows or the hair. Thus, in frontal neuralgia he paints the eyebrow without allowing the iodine to pass to the skin, and in temporal neuralgia he applies the tincture in front of the ear, concealing it under the hair of the temple. But everywhere else the tincture of iodine may be extensively applied, without any precaution, on large surfaces, and then the revulsion is the more energetic in proportion to the greater size of the cauterized part. Thus, in intercostal neuralgia a large band of tincture of iodine over the whole extent of the painful nerve, and in sciatica over the whole of the sciatic nerve, never produces any inconvenience. These revulsive applications are made with pure tincture of iodine by means of a camel's-hair pencil; they are repeated night and morning, or three times a day, and then the skin is irritated, and the hardened epidermis cracks, and a severe pain is felt. In such cases the application must be altogether discontinued, or used at more distant intervals, or the tincture must be weakened by the addition of water or a saturated solution of iodide of potassium. As long as the epidermis is entire, the iodine laid on the skin is not absorbed; but when the dermis is denuded by the exfoliation of the epidermis, and the application of iodine is painful in itself, absorption takes place, and iodine is detected in the urine by the use of re-agents. M. Bouchut, although convinced of the utility of iodine applied in cases of superficial neuralgia, doubts very much whether it acts as a specific in consequence of absorption, and is disposed to think that it acts only as a caustic, stimulating the skin and acting as a revulsive. It is a succedaneum to all irritating embrocations used in neuralgia, such as mustard, turpentine, camphorated spirit, ammonia, chloroform, &c.; and it may be substituted for blisters and the actual cautery. In fact, it possesses some really beneficial properties, without producing any inconvenience, and in many cases it may be used in the first instance before having recourse to any more energetic remedies.

The morphinized tincture of iodine acts in a different manner: the application is less painful, and does not so rapidly produce epidermic desquamation. It acts also as a revulsive, but in a far less energetic manner than simple tincture of iodine; and it appears to be especially the medium of a combination of morphia with the epidermis, acting upon the painful tissues by its sedative action. In certain cases it acts by inoculation when the cracked epidermis leaves certain parts of the dermis denuded by which absorption takes place.

IX. 1. *On the Action of Expectorant Remedies.* By W. T. Gairdner, M.D., Professor of the Practice of Physic in Glasgow University. (Glasgow Medical Journal, July.)

2. *On the Use of so-called Expectorants in Diseases of the Mucous Membrane of the Lungs; being Remarks suggested by Professor Gairdner's recent Article on the Action of Expectorant Remedies.* By J. A. Easton, M.D., Professor of Materia Medica in Glasgow University. (Glasgow Medical Journal, Oct.)

In the first of these papers, Dr. Gairdner, after examining and criticising the opinions of Cullen and other authors as to the action of expectorants, offers a theory of his own. He regards normal expectoration as an act totally independent of the will or of the consciousness, and analogous to the contractions of the heart and arteries, of the intestines, of the Fallopian tubes, and of the ducts of the secreting glands. He thinks that it is, in fact, a kind of peristaltic action, such as is observed in the long tortuous uterus of some of the lower animals, in the bile-duct, or in the ureters. Just as the fœtus, in the first instance, and the biliary or urinary calculus, in the second and third, so is the bronchial mucus expelled from the bronchial tubes by a kind of peristaltic action, caused by the contraction of the bronchial circular fibres. The remedial agents that are found by experience to facilitate the removal of mucus from the lungs are, according to Dr. Gairdner, chiefly of two kinds: first, aromatic and volatile substances, such as camphor, ammonia, assafœtida, garlic, myrrh, copaiba, and several other balsamic extracts, turpentine, benzoin, &c. The action of these is probably local, and consequent on their direct contact with the pulmonary mucous membrane, either through the secretions or through the expired air. Secondly, substances which are expectorant in small doses, and nauseant and emetic in larger ones, as antimony, ipecacuanha, squills, senega, lobelia, tobacco, &c.; and these last are believed by Dr. Gairdner to be peculiarly and specifically the exciters of bronchial peristalsis. Squill, ipecacuanha, and antimony act upon the stomach in large doses, and produce vomiting, and this vomiting will often relieve the lungs, when they are congested, by producing copious expectoration; and, on the other hand, severe and irritative cough, such as that of hooping-cough, or of some stages of pulmonary phthisis, is apt to be followed by vomiting. This fact can only be explained by the nervous communication which exists between the stomach and the lungs through the medium of the pneumogastric nerve.

In the second paper at the head of the present article, Dr. Easton disapproves altogether Dr. Gairdner's views on expectoration, and he denies that the medicines classed under the head of expectorants excite peristalsis in the bronchial tubes. Indeed, he denies that peristalsis can be said to occur in these tubes, for Reisseisen has seen in them only transverse muscular fibres, the longitudinal fibres being of a doubtful character, and certainly not muscular; hence peristalsis cannot take place, for that operation requires both transverse and longitudinal muscles. The transverse fibres, according to Dr. Easton, are undoubtedly capable of narrowing the calibre of the bronchi, and thus are very probably serviceable in regulating the ingress and egress of air, and perhaps may take a part, together with ciliary motion, in disposing of the normal mucus of the air-tubes. Dr. Easton considers that the medicines called expectorants act in very different modes, and that the object of the practitioner in relieving congestion or inflammation of the air-tubes is not merely to remove the superabundant secretion, but to alter the character of the membrane by which the secretion is poured out.

The condition of the bronchial mucous membrane is very different in the acute and chronic forms of bronchial inflammation, for in the former case the membrane is dry and harsh, assuming a state something like that of the skin during an attack of fever, while in the latter the tubes are filled with an excessive muco-purulent secretion. In the former case, therefore, instead of attempting to cause expectoration where there is nothing to expectorate, the medical treatment should be directed to overcome the congestion, to restore the suspended secretion of the pulmonary membrane, and to establish from it a kind of internal diaphoresis. In the latter case, there should be a double object—namely, to remove the excessive secretion, and to alter and improve the condition of the secreting surface. Squill, being an irritant, does harm in acute inflammation, because it increases the congestion of the membrane which is already too much congested; but it does good in chronic inflammation when the pulmonary membrane is in an atonic state. Dr. Easton proposes to divide the so-called expectorants into what he calls *relaxing broncho-muco alterants*, which are principally inhalation of vapours, tartar-emetic in doses of one-twelfth to one-sixth of a grain, ipecacuanha in one-quarter or one-half grain doses, henbane, hemlock, aconite, green hellebore, hydrocyanic acid, demulcents, alkalies—all of which are suitable to the acute cases; and *stimulating broncho-muco alterants*, as squill, leek, onion, garlick, benzoin, styrax, preparations of tolu, turpentine, copaiba, the fetid gums, myrrh, senega, lobelia, sesquicarbonate of ammonia, &c., for the chronic cases; and as coughing is necessary for the removal of excessive muco-purulent secretion, and the consequent relief of dyspnœa, and as this act is a muscular one, it should be excited by a class of medicines which might be called *pneumo-musculo excitants*, such as the stimulating preparations of ammonia, alcohol, nux vomica, iron, cinchona, together with general hygienic measures, and the use of embrocations, sponging, and friction, and the inhalation of stimulating vapours.

X. *On the Datura Tatula, and its Use in Asthma.* By JOHN F. MCVEAGH, M.D. (Dublin Quarterly Journal of Medical Science, August, 1863.)

Dr. McVeagh was first made acquainted with the medicinal powers of the datura tatula, while he was attending an Irish nobleman, in the year 1850. The latter was suddenly seized with a severe attack of asthma, to which he was subject, and as soon as he was able to speak, he stated that he had a remedy at hand which would quickly relieve him. This was the datura tatula, put into a pipe and smoked; and he had scarcely smoked it for a few minutes when the dyspnœa began to abate, and in about ten minutes more he was very materially relieved. The smoking ingredients consisted of an equal mixture of bruised seeds and the dried herb. He had used it for many years with perfect success, having first learned its use at Malta, where it grew abundantly in the governor's garden. Dr. McVeagh subsequently learned from the Right Hon. More O'Farrell, who had been Governor of Malta, that the plant was much prized in that island as a remedy for asthma. Dr. McVeagh afterwards prescribed the remedy in several cases with success. It is not efficacious in cases complicated with disease of the heart or chronic bronchitis; or at any rate the relief is only temporary; but in cases of uncomplicated asthma its effects are very beneficial. Its action resembles stramonium in some degree, but it is more antispasmodic and less narcotic, and rarely causes headache. It may be used for smoking, like tobacco, and also in the form of extract and tincture. The dose of the extract is from half a grain to a grain and a half, and that of the tincture is from 20 to 60 minims.

XI. *On Cinchona Wine, Ferruginous Cinchona Wine, and Iodized Cinchona Wine.* By G. RICHELOT. (L'Union Médicale, June 16th, 1863.)

In consequence of the different proportions of the active principle contained in cinchona wine, M. Ossian Henry has prepared a cinchona wine of definite constitution, which he calls *vin de quinquina titré*, and which agrees particularly well with delicate, aged, or enfeebled persons, whose stomachs do not easily support the employment of bitters. It possesses the advantage of not having a disagreeable taste; but the property which distinguishes it most is the constancy of its composition and its medicinal properties, and the consequent possibility, for the practitioner, of graduating its doses with certainty, according to the effect which he wishes to produce in the system. M. Ossian Henry has also devised a *ferruginous cinchona wine*. Iron and cinchona are incompatible, for the tannin contained in the latter always forms an insoluble compound with iron. This serious difficulty has been overcome by M. Ossian Henry, by taking advantage of the fact observed in his chemical processes, that certain soluble salts of iron undergo no alteration in the presence of cinchona if diastase is also present. He has therefore composed, with Malaga wine, cinchona, iron, and diastase, a very remarkable and novel pharmaceutical product, from which he is able to remove a great part of the bitterness of the cinchona and the astringency of the iron.

The *iodized cinchona wine*, like the preceding compound, is a new product. M. Ossian Henry has been the first to succeed in combining together cinchona and iodine—two substances which are as incompatible as cinchona and iron. This preparation possesses the remarkable property of rendering the internal administration of iodine as easy as it was formerly difficult. The smell and taste of iodine, and its irritating action on the living organs, have rendered the administration of iodine so difficult that the iodides have always been substituted in the internal treatment of diseases. But the iodides do not completely supply the place of iodine. The iodide of potassium, for instance, is found unchanged in the secretions, in the milk, in the saliva, and in the urine, and consequently, when introduced into the system, it acts as iodide of potassium and not as iodine. M. Ossian Henry had great difficulties to contend against in combining iodine with cinchona, for iodine, whether alone or combined with tannin or gluten, precipitates abundantly the cinchona wine. It was therefore necessary, so to say, to envelope and isolate it, so as to place it side by side with the cinchona without allowing any reaction, and without altering its nature in any way. M. Ossian Henry has succeeded in this object by associating iodine with sugar by a process of substitution, and the new preparation presents the cinchona and the iodine intimately united but not combined. In a medicinal point of view, this new product is very important. The disagreeable taste and smell of the iodine, as well as the bitterness of the cinchona, are imperceptible. By this association, the causticity of the iodine, which is so injurious to the mucous membrane of the stomach, is completely blunted, so that under this form the iodine may be prescribed in large doses, and its internal administration may be regulated according to the indications in each case. This combination of Malaga wine, cinchona, and iodine, is said to be likely to form an excellent strengthening and restorative medicine, remarkable for the rapidity of its action, and especially serviceable in the treatment of scrofulous and syphilitic affections.

XII. *On Glycerine Pommades in Diseases of the Eyes.* (Journal de Médecine et de Chirurgie Pratiques, July, 1863.)

Glycerine is a valuable addition to ophthalmic therapeutics, and Professor Gräfe thus alludes to the subject: "Combined with red precipitate, solidified glycerine is an excellent local remedy in phlyctenoïd ophthalmia and its sequelæ. When a tendency is observed to turgescence of the conjunctiva and to granulation, pommades prepared with starch and glycerine are in general more efficacious than all others. This solvent is the best that can be adopted for the external use of sulphate of atropia." The solidified glycerine alluded to is prepared by heating together glycerine and starch in a porcelain capsule, and stirring the mixture continually. M. Foucher considers that nitrate of silver is incompatible with glycerine, because the nitrate is decomposed by organic matter and converted into a chloride. This is the case, however, only when common glycerine is used, and the remark does not apply to the purified article, which may be mixed with nitrate of silver without decomposing it; and the mixture of glycerine and starch, used in the hospitals, forms with nitrate of silver an excellent ointment, which M. Démarquay often prescribes as a dressing for certain forms of venereal ulcer. Some of the new pommades used in diseases of the eyes are composed of solidified glycerine (made by mixing glycerine with starch) with sulphate of copper and corrosive sublimate, the latter being universally applied in syphilitic ophthalmia, and being highly recommended by some surgeons in blepharitis, in senile iritis, and ulcers of the cornea. All the other eye-ointments may also be compounded with glycerine; and one of them, in which iodide of potassium is a principal ingredient, appears especially deserving of notice. M. Gosselin considers that the iodide penetrates into the chambers of the eye through the cornea, and exercises a direct and favourable influence on the absorption of plastic exudations. In addition to these medical applications, M. Foucher recommends the lids to be anointed several times a-day with pure glycerine. This substance mixes with and liquefies the morbid secretions, and prevents the formation of adhesive crusts at the bases of the lashes. When scabs have formed, they are soon dissolved by glycerine, and may be readily removed.

XIII. *On Apiol, as a Regulator of the Menstrual Functions.* By Dr. Barbette. (Journal de Médecine et de Chirurgie Pratiques, July, 1863.)

Dr. Barbette relates the following case, showing the efficacy of apiol (the active principle of parsley) in the treatment of irregular menstruation: A married woman, aged twenty-eight, without family, had never menstruated with any degree of regularity. In her whole life she did not recollect the recurrence of the function for two successive periods, the intervals being always nine, twelve, and even fifteen months. The most powerful emmenagogues were prescribed without any effect, and the patient had also resorted, without benefit, to several mineral spas. Dr. Barbette having been consulted, recommended the use of apiol, four capsules of which were to be taken in the course of two days during the first month; during the second month, six capsules in the course of three days; and eight capsules in four days during the third month. The first doses appeared to be ineffectual; but after the third, which consisted of eight capsules, the menses appeared, and continued without pain for five or six days. On previous occasions the secretion had been attended with considerable suffering, and the remedy would therefore seem to have exercised a sedative action on the womb. The catamenia afterwards returned for five months in succession, and invariably reappeared two or three days in advance. Dr. Barbette considers that these important results were entirely due to the administration of the apiol, which he conceives to be the most effective of all known emmenagogues.

XIV. *On the Comparative Efficacy of Musk and Acetate of Ammonia in the Treatment of Cases of Severe Pneumonia with Delirium.* By Dr. Délioux de Savignac. (Bulletin Général de Thérapeutique, July 30th, 1863.)

Dr. Délioux specifies opium, musk, and acetate of ammonia as the most useful medicines in the treatment of those low forms of pneumonia which have been called typhoïd, malignant, or ataxic, or are accompanied with delirium. Musk is an antispasmodic of well-established reputation, and if it does not actually stimulate, its action is not without a certain tonicity, and is therefore useful in certain nervous conditions when it is necessary to tranquillize but not to reduce the vital powers. In large doses it produces phenomena which are variously interpreted, but which appear to Dr. Délioux to be analogous to those of slight intoxication, such as may be produced by most essential oils, but he has never seen them manifested when the drug is given in the ordinary medicinal doses. It seems to be a remedy well adapted for cases of ataxia joined with adynamia, such as are found combined in typhoid pneumonia. The musk should not be given in too small a dose, if it is desired to produce a proper effect, and it is useless also to give too much. One gramme (fifteen grains) every day is at least necessary, but may be sufficient in the treatment of a case of pneumonia with delirium. The great objection to the use of musk is its very high price, and its consequent scarcity in many places, especially in the country.

The *Spirit of Mindererus*, which is nothing else than a mixture of acetate of ammonia and empyreumatic products, formerly enjoyed an enormous reputation in the treatment of typhous and typhoid cases, malignant fevers, and typhus itself, but until the beginning of the present century it had fallen into disuse. Dr. Délioux considers it to be a modifying agent of great utility in cases of adynamic pneumonia, and that a soothing action is its peculiar pharmaco-dynamic property. Thus, he has observed in the decline of acute diseases, when the fever remains without any obvious connexion with organic lesion, that the administration of acetate of ammonia has rendered the circulation normal, and has removed from the convalescence the last morbid phenomenon which masked or retarded it. Thus it appears that the physiological action of acetate of ammonia is not identical with that of musk, but is very like it in several respects. Both are antispasmodics, moderators of the nervous perturbation, regulators of the nervous influx, and, as they might be called, anti-ataxics. But musk at the same time sustains, and even stimulates the nutritive organic actions, while acetate of ammonia lowers the circulation, and probably acts besides on the blood by attenuating its fibrin, as all the ammoniacal preparations do, and induces a proportionate degree of debility. Dr. Délioux states, that since he has been in Provençe, where diseases are often complicated with nervous symptoms, and where he has seen ataxic cases of pneumonia perhaps more frequently than elsewhere, acetate of ammonia has appeared to him to be more efficacious than musk, and he has therefore employed it with remarkable success. Pneumonia, complicated with eruptive fevers, and which would become malignant and accompanied with delirium, may be mitigated by musk or acetate of ammonia. If an adynamic condition prevails, musk, especially the tincture, together with cinchona, appears to be preferable; but, in Dr. Délioux's experience, acetate of ammonia in large doses relieves the pneumonia, which is consecutive to measles and scarlatina.

XV. *On the Influence of the Air of the Pyrennees on the Affections of the Respiratory Organs.* By Dr. Prosper de Pietra Santa. (L'Union Médicale, July 25th, 1863.)

The air which is breathed in the Pyrennees at an elevation of eight hundred mètres above the level of the sea, possesses the peculiar properties of being more light than ordinary air, of containing less oxygen in an equal volume, of being impregnated with a more considerable amount of watery vapour, and of containing a very large proportion of ozone, or oxygen in a peculiar electrical condition. The atmosphere thus constituted exercises a very beneficial influence on the chronic affections of the respiratory organs, and it forms in these particular cases a very powerful auxiliary to the beneficial operation of the sulphurous thermal waters diffused throughout the region. In the case of patients who arrive at the Eaux Bonnes in a state of congestive sub-irritation and nervous excitability, a slow but progressive amelioration is observed, even before the administration of the waters; the gastro-intestinal functions are regulated and fortified, breathing becomes more easy, and the general phenomena undergo an improvement. This improvement may probably be attributed to the peculiar conditions of the surrounding atmosphere, in which there is less oxygen, but an ozonized form of it, and a lighter atmosphere saturated with watery vapour. These facts probably afford an explanation of the more marked efficacy of the mineral waters when drunk at the springs; for chemical reasonings have not hitherto explained the characteristic difference which is presented by the use of the waters at a distance from their source. The attentive observation of the phenomena exhibited in children by a visit to the Pyrennees appears to illustrate the above remarks. For the first few weeks after their arrival, children in general experience the favourable influence of change of air, purity of the atmosphere, and exercise; their vital activity is increased, and a notable change takes place in their system. But subsequently, under the influence of an imperfect oxidation, and of an impoverished condition of blood, the gastro-intestinal functions become deranged, and symptoms of anæmia and chloro-anæmia precede or follow those of nervous irritability. The pale complexion, emaciation, the *bruit de souffle* in the carotids, and the restlessness of disposition, leave no doubt as to the pathological conditions. The children are ill because they are deprived of a certain quantity of oxygen, and they are improved by ferruginous preparations, cod-liver oil, and cinchona. Thus, the effects of an atmosphere which contains too little oxygen may be easily understood; it is useful for the pulmonary invalid, because he requires to breathe as little as possible, and to introduce a less quantity of oxygen into his lungs; but it is injurious to children because they do not absorb the quantity of oxygen which is necessary for their complete organic support. The sulphurous waters of the Pyrennees are in their nature essentially of an exciting character, but this property is counteracted by the atmospheric conditions of the localities which are sedative and tranquillizing. Dr. De Pietra Santa, from a full consideration of the climate of the Pyrennees, thinks that he has discovered the explanation of the facts that patients there experience a more acute sensation of cold than is always commensurate with the thermometric degree; that certain mineral waters are much more efficacious when drunk at the spring; and that the action of the waters is more immediate when there exists a certain degree of humidity in the atmosphere.

HALF-YEARLY REPORT ON PATHOLOGY AND PRINCIPLES AND PRACTICE OF MEDICINE.

By Francis C. Webb, M.D., F.L.S.,
Member of the Royal College of Physicians, Physician to the Great Northern Hospital.

I. *On Cerebro-spinal Lesions in the Diabetic.* By Dr. Marchal (de Calvi). Communicated to the Academy of Sciences Oct. 12, 1863. (L'Union Médicale, Oct. 20, 1863.)

The paper of Dr. Marchal had for its object the establishment of the position that cerebro-spinal lesions are frequently the consequence and not the cause of the diabetic condition. Experimental physiology having demonstrated that various lesions of the cerebro-spinal axis are followed by glycosuria, it has been the natural consequence of that discovery to regard all lesions of the nervous centres observed in diabetic patients as primitive rather than consecutive. In the same way the anthrax which occurs in the same disease has been supposed to give rise to it, whereas the author asserts that there is not a single example in which this connexion has been established. In 1853 the observation of a furunculous eruption in a diabetic patient, who at the same time was paraplegic and amaurotic, led him to formularize the proposition, "That it is possible diabetes may produce paraplegia as it produces amaurosis, and that it is therefore as essential to examine the urine in the former as in the latter affection."* This he believes to have been the first mention of the production of cerebro-spinal lesions by diabetes. Since that time numerous cases have come to his knowledge in which changes in the nervous centres appeared to be produced during the diabetic condition. Notes of twenty-three, collected from various authors, compose the bulk of the present paper. The symptoms in these cases varied considerably. Some of them were cases of apoplexy; in others there were various affections of the special senses, or of motion and sensation. In some the intellect was affected. One of the facts he considers most striking is taken from a paper by M. Fritz, "On Diabetes in connexion with Cerebral Maladies." A woman, aged thirty-eight, diabetic for seven years, suddenly succumbed to a cerebral attack. On examining the brain a superficial ulceration was found, covered with sanious detritus, between the back part of the left thalamus opticus and the corpora quadrigemina, its base softened, and exhibiting points of capillary extravasation. Here M. Marchal observes that the pre-existence of diabetes was beyond doubt, for it was anterior by seven years to the cerebral accidents. He concludes, that even if we admit that some of his facts are debateable, it is not the less established that diabetes may act in the same manner as gout in predisposing to lesions of the cerebro-spinal axis.

II. *The Transmission of Syphilis through Vaccination.* By Dr. H. Bohn, Privat-docent an der Königsberger Universität. (Schmidt's Jahrb., No. 10, 1863, pp. 97-109.)

A very careful and elaborate essay on the history and literature of the question whether secondary syphilis can be communicated by means of vaccination, the possibility of this being, as Dr. Bohn remarks, the only one remaining of the many serious accusations from time to time launched against vaccination. After remarking that inoculation was in its day charged with the same risk, Dr. Bohn notes that Prof. Monteggia was the first, in 1814, to declare that vaccination might be the means of communicating syphilis, he believed that when a syphilitic patient was vaccinated the resulting vesicle contained a double virus—the syphilitic virus and the vaccine virus. He was followed, and his doctrine supported by cases, by Dr. Marcolini, in the same year, and in 1821 by Prof. Cerioli. In France the question was first taken up in 1831 by Bidart, and was, after an experimental examination, decided in a contrary sense to the Italian professors. From that time many imputed instances of infection are recorded, and the question has been many times discussed in different parts of Europe, all of which cases and discussions Dr. Bohn notices and critically examines. The first criminal accusation against a surgeon for thus communicating syphilis took place in Coblentz in the year 1849, and resulted in his punishment by fine and imprisonment.

Dr. Bohn fully notices the examination made into the subject by the General Board of Health of England in 1855, by a series of questions addressed to 539 physicians and medical societies. The third question proposed was, "Have you ever seen any grounds for believing or suspecting that lymph taken from a perfect Jennerian vesicle can become the vehicle for infecting a vaccinated person with syphilis?" The 539 answers are arranged in four classes:

1. The question is answered shortly in the negative, partly on theoretical grounds, partly from a long and rich experience. In this class were Chomel, Rostan, Rayer, Velpeau, Ricord, and most English physicians.

2. The possibility of "vaccino-syphilitic inoculation" is combated on the ground of direct evidence. Here are found Oppolzer, Sigmund, Hebra, and the whole of the Austrian school, and joined with them is "the learned and

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* Moniteur des Hôpitaux, Sept. 8th, 1853.

sagacious reporter of the General Board of Health," Mr. Simon, whom, by the way, Dr. Bohn dignifies with the title of "Sir John."

3. A few physicians questioned whether, physiologically, the contents of a vaccine vesicle in syphilitic persons might not be infectious as well as their blood.

4. This class, including Bamberger, Rinecker, and Whitehead, considered that the instances of infection were singular and exceptional.

The result was to leave the question very much where it was before.

Then Viennois, of Lyons, took it up, and examining all the recorded cases, classed them in two groups. The first contained instances of persons who when vaccinated had latent syphilis, the eruption of which was caused by the vaccination. The second contained the cases of individuals truly syphilized by the act of vaccination; but Viennois declared that in these instances blood had been mixed with the lymph, and that while the vaccine lymph only produced true vaccination, the syphilitic inoculation was due to the admixture of blood.

Dr. Bohn then examines by the light of this doctrine all the recorded cases and all the dissertations up to the present time, and concludes that an already existing syphilis is excited to a more general, more lively manifestation by vaccination; and that vaccino-syphilitic inoculation—i.e., the cotemporaneous communication of vaccination and syphilis—is a fact, but that *pure* lymph *can* only communicate vaccination, and when syphilitic blood is mixed with it then only can syphilis be at the same time communicated; that true and pure vaccination is therefore purged from the only accusation remaining against it, and that the title "syphilis-vaccinata" must therefore be abandoned.

To this essay a sort of supplement is added by Dr. Millies, who states that when it was received by the editor he was himself engaged on a similar work, but that the "solidity" of Dr. Bohn's communication has induced him to abandon the design, and content himself with only adding to Dr. Bohn's essay some observations which appear not to have been at Dr. Bohn's command, and "which would possibly have led him to a different conclusion."

He then quotes some additional cases — Glatter's, Haydon's, &c.—and notices at some length Melchior Robert's paper against Viennois' hypothesis, and his conclusion that "in the communication of syphilis through vaccination the commingled blood is not the contaminating medium." He points out that Ricord, Cullerier, and many other physicians deny that there is any causal connexion between vaccination and syphilis, and that Ricord, Max, Albertetti, and others believe the Rivalta cases were due to a fortuitous concurrence of circumstances, not to any relation of cause and effect between the vaccination and the outbreak of syphilis. And, finally, he remarks that when Devergie's cases were discussed in the French Academy, Ricord confessed that he could no longer dispute the possibility of the communication of syphilis through vaccination; and that as to whether the medium of infection was the pure lymph, matter, or blood, he thought that it was a proved fact that syphilitic blood was inoculable.

III. *Epidemic of Spotted Fever occurring in the vicinity of Philadelphia in the year* 1863. By Drs. W. W. Gerhard and Lamb. From the 'Transactions of the College of Physicians, Philadelphia.' (American Journal of the Medical Sciences, No. 91, July 1863.)

Dr. W. W. Gerhard, whose well-known observations on typhus and typhoid fevers entitle anything he may advance on the subject of fever to be received with attention, has given an account of a new form of fever lately observed by him in Philadelphia, which he considers entirely distinct from any of the recognised forms of continued or exanthematous fevers. The form of fever he describes he believes to be entirely unknown in Europe; and the only account of it extant is an imperfect description given of it by a number of physicians in New England, where the disease appeared between the years 1807 and 1816.

About the middle of February, 1863, Dr. Gerhard was called to see a boy, aged sixteen, who was taken suddenly ill with intense pain in head and back, with occasional delirium, and nausea and vomiting. In the intervals of the delirium he was dull and heavy, but able to answer questions correctly. On the second day there was an eruption over the whole body of spots varying in size from such as would be caused by the prick of a pin, to an inch or more in breadth. These spots were of a dull-red colour, not in the slightest degree elevated, and rather resembling ecchymoses such as might be caused by the puncture of an insect than a proper eruption. There was no diarrhœa; moderate heat and fever; the tongue was scarcely coated. The patient died in four days from the attack, sinking in a state of coma. A sister of the patient, aged twenty, was afterwards taken ill with the same symptoms and eruption. She died in twenty-six hours from the commencement. No distinctive lesions were found after death.

A few days after the disease showed itself at the Falls of Schuylkill, five miles from the city; it was there exceedingly fatal. In each of two families there were three deaths, in another two. Besides there were about twenty scattered cases, of which ten proved fatal.

The symptoms exhibited were sudden chill and intense headache, and pain in the back, followed by fever; the patient became dull and heavy, either not answering at all or only when loudly spoken to. In some cases there was delirium, but in the majority the cerebral disturbance tended rather towards stupor or coma. In fatal cases there was always coma a few hours before death. There was often vomiting in the commencement, but no epigastric tenderness. There was constipation and disgust of food; the urine was perfectly healthy; the degree of fever varied; the pulse was frequent, not strong, and frequently diminished in strength and rapidity; the heat of skin was comparatively moderate. In some instances the patient exhaled a peculiar odour. The eruption consisted of spots varying in size from a pin's point to ecchymoses an inch or two in breadth. Each spot was of a dull-red or purple colour, varying in shade; the lighter spots became somewhat lighter on strong pressure, but the darker were not modified in any way; they were evidently due to effusion of blood in the true skin. They were in no degree elevated, and were scattered pretty equally throughout the body, being perhaps a little more abundant in the extremities than the trunk. They usually appeared at the end of twenty-four hours, sometimes earlier, and in some cases were perceptible after death, although not very visible during life. The duration of the spots was very variable: in some cases they lasted a week or two, disappearing in the same way as the dark colour of a bruise. The complexion was dull and slightly yellow, but there was no jaundice. The eyes were moderately injected in some instances, but rather in the veins than in the arteries. In one case an eye was destroyed by suppuration. There were no glandular enlargements. No age was exempted, but the larger proportion of patients were between fifteen and twenty. More females than males were attacked. In one case examined after death there was congestion of the brain, and an effusion of serum at its base and in the ventricles. The lungs were slightly congested posteriorly; the right side of the heart contained very dark fluid blood, with a very small and flaccid coagulum. There were ecchymoses under the serous layer of the pericardium and under the serous covering of the stomach, and there was an ecchymosis a quarter of an inch in breadth in one of the glands of Peyer. The fever appears to have originated in the army in Virginia, before it reached Pennsylvania; but it does not seem to have spread by contagion. There was no positive evidence of its having been transmitted from the sick to the healthy; whilst in many cases in Connecticut, where it also prevailed, it attacked persons who had not been in the neighbourhood of the sick. With regard to its mortality, a number of the patients died in from twelve to twenty-four hours, and the largest number in the course of the second day. If the patient lived over forty-eight or fifty hours there was a fair chance of recovery, although fatal cases were met with after the lapse of several days. There appear to have been no critical days noticed. Some convalesced after two or three days, others after a week or two, and in one instance recovery was protracted for many weeks.

The points of distinction between this form of spotted fever and typhus insisted on by Dr. Gerhard are as follow: in spotted fever the disease is very rapid in its course, with delirium, but rarely of an active kind; there is an eruption wholly different from that of typhus; less heat of skin, which is never of the burning temperature of typhus, and with none of the peculiar odour of this disease. Typhus offers a true exanthematous eruption; but that of spotted fever is a scorbutic hæmorrhage. The duration of typhus fever is regular and long, that of spotted fever is irregular, and much shorter. Typhus is unequivocally contagious, in spotted fever contagion is doubtful. In addition, there is the different rate of mortality in the two diseases.

In the treatment of this fever Dr. Gerhard found stimulants the most important remedies.

At the same meeting of the Philadelphia College, Dr. Lamb detailed several cases he had seen of this fatal form of fever.

The first case occurred in Frankford. It was that of a girl, aged eleven. She was taken ill on the 11th March. Dr. Lamb saw her on the 12th; her symptoms at that time were vomiting; jactitation; no sensible increase of heat; flushed face; dilatation of pupils, which seemed insensible to a strong light; tongue moist, and slightly furred; pulse intermittent, above 90, distinct, but not full, easily compressible; no disturbance of respiratory organs; no tenderness of epigastrium or tension of abdominal muscles; extreme sensitiveness to slightest pressure on arms and legs. On the 13th, an eruption resembling urticaria appeared on the arms and legs. The spots varied from a few lines to an inch in length; they were slightly raised, and of uniformly pale red; the pulse was still intermittent. On the 14th, the pulse was very irregular, but yet distinct. On the 15th, the patient could be aroused, but responded very slowly to questions; the pulse was still perceptible and irregular; the pupil of the right eye was much dilated, the left contracted to a mere speck. At noon she was in a profound stupor, and died at four P.M.

The next case was that of a girl aged eight, who lived in a parallel street, about five hundred feet distant from the former patient. She was taken ill on the evening of the 13th. On the 14th, her situation was very similar to that of the first patient. She became comatose about two P.M., and died at five P.M., on the 15th. A few specks, like flea-bites, appeared on the neck and breast.

For ten days no other case occurred in Frankford; three cases then came under Dr. Lamb's notice, all commencing about eight P.M. on the 24th March, two of them in a family about six hundred yards east of the first two cases, and one in Bridesburg, a place about the same distance east of Frankford.

Miss M. E., aged nineteen; her symptoms were acute pain in the forehead; pain and excessive tenderness of the extremities; nausea; delirium; dilated pupils; skin moderately warm; face flushed; pulse about 90, with frequent intermissions; circulation sluggish; a few petechial specks on the legs and arms; the tongue moist, and slightly furred. She died on the 27th. The catamenial flow appeared on the previous night, ten days too soon. She was comatose three hours before death.

The brother of this patient had previously suffered from pulmonary disease. He had the intermittent pulse, the petechial spots, and congestion of the lungs; some pain of the head, but no delirium. He became convalescent on the fourth day.

The third case was that of Mrs. E. R., aged thirty-seven. At eight P.M., she complained of acute pain in the head, and giddiness; the body was warm, and extremities cold; pulse irregular and intermittent, thread-like; pupils greatly dilated; vision indistinct; delirium. She became comatose at four A.M., and died at noon, fourteen hours from the first feeling of indisposition. There were numerous blotches on the body, face, and extremities. Some had the appearance of ordinary ecchymoses, others looked and felt as if the skin had been forcibly raised or pinched up, and blood effused, so as to elevate the surface. The patient had her catamenial flow out of time during the few hours of her illness.

Dr. Lamb also gave the particulars of the cases of the mother and sister of the last patient. The former was at the time convalescent, she having began to recover on the fourth day; the latter was in a condition which allowed hope of recovery. In the case of the daughter, there was slight opisthotonos, with unequal dilatation of the pupils and delirium, and the inferior extremities were covered with an efflorescent eruption, which might have been mistaken for rubeola or variola. The skin was perceptibly elevated. In the case of the mother, the same eruption appeared on the lower extremities. Each patient had intermitting pulse, and in both there was pain and tenderness in the lower extremities.

IV. *Circumscribed Aneurysm of the Walls of the Left Ventricle.* By P. S. Wales, M.D. (American Journal of the Medical Sciences, No. 91, July, 1863.)

J. B., sailor, aged twenty-five, admitted into hospital, Portsmouth, Va., December 24th, 1862, in a semi-comatose condition. On the previous evening had fallen from his hammock convulsed; breathing short and hurried; pulse 102.

Dec. 25th.—No improvement; pupils contracted, motionless; right eye convergently, and left divergently strabismic, both turned up under the lids; conjunctivæ insensible. Face and lips dusky or bluish; tongue covered with thin, yellowish coating; mouth moist; deglutition slow and difficult; bowels obstinately constipated.

26th.—Continues in the same condition; when spoken to loudly stammers out "better," and when asked attempts to protrude the tongue. Respiration 20; pulse 82, normal in volume with some increase of hardness. Mucous râles heard over every part of the chest. Cardiac dulness increased in area to the left side. In the evening he was restless, keeping his limbs in constant motion. Urine passed in large quantities unconsciously. Skin soft and perspiring.

28th.—Tongue brown, lips covered with black sordes; jactitation, facies hippocratica; death.

Post-mortem fourteen hours afterwards.—Head: Strong adhesions between skull and dura mater; veins of pia mater gorged. Two or three superficial patches of red softening on cerebrum; the grey substance of a few sulci yellow from purulent infiltration. Arachnitis, with delicate threads of lymph in some places. Posterior lobe of cerebrum broken down with red softening and suppuration. Left lateral ventricle contained three ounces of serous fluid, with flakes of lymph. Under pons Varolii and medulla oblongata there was a purulent "foyer" of about two or three drachms of pus between the layers of the arachnoïd. The volume of the brain seemed reduced, so that there was a considerable space between it and the calvaria. *Chest:* Lungs of a deep brownish-red colour, except at the anterior margins of upper right and middle left lobes. At a few points lobular pneumonitis had occurred, on section these portions presented all the physical characters of carnification. The bronchial tubes contained muco-purulent fluid. Pleuræ healthy. *Heart:* In situ, this organ appeared of enormous size, and when grasped in the hand seemed to be double. The pericardium contained two ounces of yellowish serum. The left ventricle displayed a cordiform aneurism, which at first sight made the heart look as if it were double. The organ weighed eighteen ounces avoirdupois. The external surface of both ventricles was covered with fat except at the apices, the muscular fibres beneath had a pale, waxy appearance, whilst those of the auricles were of a healthy reddish colour, and distinctly aggregated into bands. The pericardium adhered to the apical segment of the aneurysm. In the right ventricle, besides the ordinary black post-mortem coagula there were yellowish filaments of fibrin entwined with the fleshy columns and the cords, and connected with a large mass of the same material adherent to the columnæ carneæ. The tricuspid and pulmonary valves were healthy and sufficient; there were cadaveric coagula in both auricles, and fibrinous clots in the left ventricle adherent to the columnæ carneæ.

On opening the aneurysm by an incision from the left ventricle it was found that its area was divided into an upper and lower segment by hypertrophied columnæ carneæ. Its cavity contained in the centre soft black coagula, then, proceeding outwards, a less dark granular matter, and then layer after layer of fibrin was peeled out, like the laminæ of an onion. The fibrin appeared organized, and close to the outer rind capillaries were seen shooting into it. Outside the fibrin was a hard, calcareous case, except at the orifice of the aneurysm and at its apex, where there were two round holes in the shell, the former eighteen lines, and the latter nearly an inch in diameter. There was a bulging of the extreme point of the cavity beyond the lower opening, making a small secondary cavity. The calcareous shell closely resembled in appearance the dense cortical substance of the thigh-bone. Outside it was the muscular structure of the heart reduced to a thin layer of fibres, which diminished in thickness towards the dilated apex, where it ceased. The only boundary of the aneurysm in the portion corresponding to the opening in the calcareous crust, was formed by fibrinous clots and thickened pericardium. This portion of the wall was translucent. The mitral and aortic valves were healthy. The heart itself, apart from the aneurysmal tumour, was concentrically hypertrophied and affected with fatty degeneration. Immediately under two of the aortic semilunar valves, and corresponding to that part of the left wall of the right ventricle, against which the inner segment of the tricuspid valves lay when open, the partition was diaphanous, resembling in structure and thickness the membranous diaphragm of the foramen ovale. The aneurysmal pouch when cleared out held exactly four ounces of alcohol. From the point of the tumour where the pericardium alone formed the wall, the muscular tissue increased in thickness towards its base, where close to the margin of the ventricular orifice it had attained six lines. The orifice was surrounded by a strong fibro-cartilaginous ring seated about the middle of the left cardiac margin (*margo obtusus*). The orifice had a triangular shape, with its base outwards. From the apex of the triangular opening to the middle of the base was eighteen lines, the base measured twenty-one lines. From the situation of the communicating orifice its upper half must have been covered by the mitral valves, when they flapped back in the ventricular diastole. The apex of the aneurysm projected an inch and a half below that of the heart. The author regards this aneurysm as corresponding to a similar disease in the aorta from fatty degeneration. It is remarkable from possessing a complete calcareous coating or layer. This, however, was not examined microscopically. The bronchitis and arachnitis, with cerebral softening and suppuration, were, he considers, dependent on the heart disease.

V. *The Origin of Cow-Pox and the Nature of the Vaccine Virus.* Debate in the Academy of Medicine. (Gaz. Médicale de Paris, November and December, 1863.)

M. Bouley has recently brought before the notice of the Academy of Medicine some instances in which he had been successful in producing the vaccine pustule by inoculating with matter taken from the vesicles of an aphthous stomatitis occurring in the horse. The inoculation of the udder of a cow with this matter produced a crop of vaccine vesicles, from which several children were successfully vaccinated. M. Bouley was hence led to suspect that the vaccine disease might be produced indifferently by several different maladies which occur in the horse. This opinion, however, he subsequently renounced. M. Dupaul, in the course of the debate, announced the following conclusions as the result of his investigations:

1. That vaccine virus (as a thing separate and apart) has no existence.

2. That the pretended vaccine virus, which we consider as antagonistic to and neutralizing the variolous virus, is no other than the variolous virus itself.

3. That the equine and bovine species are subject to an eruptive malady which is identical as regards its nature with the variola of the human species.

4. It is almost demonstrated that the same is the fact as regards several other species of animals, pigs, sheep, goats, dogs, apes, &c.

5. The local and general phenomena which the animals present are the same as those observed in man. The only differences as regards the pustules are those which depend on the structure of the skin and on the number of the hairs.

6. As in the human species so in the bovine and equine, variola may appear sporadically and epidemically.

7. From the horse we may easily inoculate the cow, and reciprocally.

8. From the cow we may inoculate without difficulty individuals of the human species, provided that they have not had spontaneous or inoculated variola.

9. From the horse we might also, without doubt, inoculate man, but hitherto experiments have not been tried because the horse is subject to several other grave maladies which might be inoculated at the same time.

10. The cow, the horse, and several other species may be inoculated with variolous matter from the human species.

11. When a variolous epidemic occurs amongst men it often extends itself by contagion to other animals.

12. An epidemic of variola may commence amongst animals and extend to man.

13. Inoculated variola produces a much less degree of general reaction than does variola developed by contagion. This is true in the human species, and especially in other species of animals.

14. The pustules which result from inoculated variola are often limited to the points of inoculation.

15. When a secondary eruption is produced, it is almost always insignificant and composed of a small number of pustules.

16. In a general manner we may say that the variola of animals is more discrete and less severe than that of the human species.

17. The dangers of inoculation of variola in man have been much exaggerated. The unprejudiced study of what has been written on the subject will convince of this.

18. It is probable that animals, as man, are subject to aphthous eruptions.

19. But the *maladie aphtheuse,* as it is described by several modern writers on veterinary medicine, is nothing else than variola.

At the meeting on the 1st of December, M. Rufz de Lavezon reported to the Academy a series of inoculations which had been made in the Jardin d'Acclimatation, on several animals belonging to the menagerie, by Dr. Auzias Turenne and M. Mathieu.

On the 20th of October, a horse, which had been brought from England a fortnight before, was discovered to have a number of very fine pustules developed in its mouth and on various parts of the body. On the 23rd and 26th of October, the mouth and nose of a second horse were rubbed with saliva obtained from the aphthous mucous surface of the first. Some pustules exactly resembling those of the first horse, were developed on the rubbed surfaces. With matter taken from the second horse, a cow was inoculated on the vulva, and a bull on the ear. The result was the production of a magnificent cow-pox. From matter taken from the bull, a zebu, a Javanese mare, a Siamese horse, and a Shetland mare were inoculated. In all these animals inoculation gave positive results, and produced similar fine pustules.

VI. *On Fatal Toxicoïd Icterus (Rapid Fatty Degeneration of the Liver).* (L'Union Médicale, May 16th, 1863; and Archiv für Heilkunde, 1863, 2 fasc. p. 145.)

Professor Wunderlich has drawn attention to a form of fatty degeneration of the liver which is rapidly fatal, and which develops itself spontaneously without any toxical influence entering into its causation. From its close resemblance to the rapid fatty degeneration of the viscera produced by phosphorus, he gives it the name of *Intoxicationsartige Form des perniciosen Icterus* (Ictère pernicieux toxicoïde.) It is characterized by sudden invasion, attacking in the majority of cases young females in whom the menstrual function is established, by violent vomiting in the onset, and by violent thirst without fever. Slight jaundice and apparent amelioration are soon followed by pain and tension of the abdomen, and an exacerbation of the symptoms, and death takes place on the sixth or seventh day after the invasion of the disease. After death, adipose transformation of the liver and of the other organs is found, together with numerous hæmorrhagic spots.

Professor Wunderlich cites the following case:—A girl of eighteen years, of healthy constitution, was, after a fit of passion, seized by vomiting, pain in the head, prostration, and diarrhœa. The fifth day there was a remission of some of the symptoms, but an increase of prostration and slight jaundice. The sixth day pain of the abdomen, meteorism, slight albuminuria, but no fever. In the evening of the same day delirium and death. The autopsy revealed enormous fatty degeneration of the internal organs, especially of the liver, the kidneys, and the heart; also numerous points of bloody extravasation in the cellular tissue, and traces of hæmorrhage in the alimentary canal. Chemical analysis gave no indication of phosphorus, although the symptoms and termination of the disease were exactly those of phosphoric poisoning.

The reporter of this case in the 'Union Médicale,' Dr. Hoefer, notices that Rokitansky, E. Wagner, and others, incline to the opinion that this rapid form of fatty degeneration is only due to phosphorus poisoning. To this M. Wunderlich replies that no authenticated metamorphosis of tissue depends on a single or unique cause. He also relies on the negative result of the chemical analysis in the above case which was made by Dr. Huppert, chief of the chemico-clinical laboratory at Leipzig. Dr. Hoefer observes that we are not told whether the vomited matters were analysed; and the detection of phosphorus in the tissues is in itself a matter of great difficulty, no unmistakable test being known.

VII. *On the Treatment of Dysentery.* By the Regimental Surgeon, Dr. Berger. (Schmidt's Jahrb., No. x. 1863, p. 48, quoted from Wien. Med. Wochens., xiii. 22–6, 1863.)

Dr. Berger calls attention to the treatment of dysentery by nitrate of silver —a means not sufficiently known, according to him, and at the same time the surest of all methods. His attention was directed to the employment of it by the ravages caused by dysentery among the soldiers in 1848–9 during the Italian war, in spite of the use of the most varied and best authorized means. His communication is founded on his treatment, by argent. nitr., of 99 cases in the Military Hospital at Treviso, of which cases only 3 proved fatal.

In the mildest cases, in which there are only hyperæmia and superficial erosion of the mucous membrane of the intestinal mucous membrane, the fæces being mixed with spawn-like, translucent masses of slime, the tenesmus moderate, a regulated diet and mild therapeutical measures suffice for a cure. In cases of the next degree of severity, where there is inflammation of the mucous membrane and commencing ulceration, a cure may be effected by mucilaginous mixtures, and a clyster of mucilage, with x.—xv. drops of tincture of opium every three or four hours, and warm cataplasms.

Ipecacuanha, he considers, removes the gastro-bilious symptoms which often accompany dysentery, without exercising any influence on the disease itself; of the operation of calomel and opium he has no experience.

When the above-mentioned simple method of treatment fails, and symptoms of advancing ulceration are present, recourse must be had at once to clysters of nitrate of silver. The internal exhibition of it in pills or solution, as recommended by Bamberger, is of no use; but employed locally, no other means has so lasting and salutary an effect upon the disease. A clyster of nitrate of silver, gr. vi. to viij., and even gr. x. to three ounces, with a few drops of tincture of opium, is to be given three or four times in the twenty-four hours. A mucilaginous vehicle weakens the favourable cauterizing effects of the salt. Small doses, as recommended by Gros, or clysters given at long intervals, are uncertain, and delay the cure. These means are to be continued so long as stools appear—one to three days. Afterwards, an emulsion of castor oil is necessary. The patient must be kept in bed, and his diet carefully regulated.

VIII. *On the Relation of Substernal Aneurysm to Disease of the Heart.* By Dr. A. H. Douglas, Physician to Chalmers's Hospital, &c. (Edinburgh Medical Journal, October, 1863.)

At the termination of a paper in which he discusses the relation of cardiac dilatation and hypertrophy to aneurysm of the aorta accompanied by disease of the aortic valves, to uniform or peripheral aneurysm (dilatation of the aorta), and to partial or sacular aneurysm of the aorta, Dr. Douglas offers the following considerations as fitted to advance and simplify our views of the pathology of aneurysm and disease of the heart:

1. Hypertrophy of the heart is probably in all cases a secondary lesion, and is the result of Nature's effort to counterbalance a pre-existing hindrance to the circulation.

2. Dilatation of the heart is the natural resolution of local diseases, which may, in the first instance, excite hypertrophy. Embarrassment of the circulation, with venosity of the blood, dropsy, &c., usually arises with this consecutive dilatation.

3. Valvular disease of the heart often co-exists with aneurysm of the aorta, especially peripheral aneurysm; the sigmoïd valves most usually are affected, perhaps from contiguity.

4. In such cases the consecutive condition of the heart does not materially differ from that which occurs in cases of simple valvular insufficiency.

5. A diseased condition of the arterial coats often exists in conjunction with hypertrophy of the heart, as is commonly seen in cases of apoplexy with disease of the cerebral arteries.

6. There is reason to think that the disease of the arterial coats may exist extensively in the system, though the aorta and the arteries at the base of the brain usually present its most advanced effects.

7. In this state the aorta is apt to undergo dilatation, constituting usually peripheral aneurysm.

8. The diseased state of the arteries destroying their elasticity, the circulation is to that extent obstructed, and the left ventricle, under the additional burden, undergoes hypertrophy to compensate the lost tonicity of the arteries.

9. In such circumstances hypertrophy arises in a way analogous to that which occurs in cases of disease of the valves, &c., of the heart.

10. Consequently, hypertrophy of the heart and peripheral aneurysm stand associated together as effects of the same diseased condition of the arteries.

11. Consecutive preponderating dilatation may occur in such cases, but the progress to that stage is liable to be hindered by the accidents of hypertrophy, as, for example, of cerebral hæmorrhage, which is promoted by the co-existing disease of the arteries.

12. Aneurysm of the aorta is often altogether local in its origin, and has no tendency to involve the heart in associated or consecutive disease.

13. Such aneurysms are usually sacular, but they may be peripheral, and they suggest the probability of localization of the disease of the coats of the artery.

14. Proximity to the heart in such cases does not affect that organ.

IX. *The Connexion between Tuberculosis and Insanity.* By T. S. Clouston, M.D. (Journal of Mental Science, April, 1863.)*

This paper is founded chiefly on the statistics of various asylums, both English and foreign, having special reference to the records of post-mortem examinations, and is accompanied by several tables of considerable interest. The following are the results to which the author has been conducted:

"1. Phthisis pulmonalis is much more frequent as an *assigned cause of death* among the insane than among the general population.

"2. Tubercular deposition is about twice as frequent in the bodies of those dying insane as in the sane.

"3. Phthisis pulmonalis is the 'assigned cause of death' in only about one-half of those in whom tubercular deposition is found after death.

"4. The brain in the case of tuberculosis is not so frequently diseased in a marked manner as it is in those dying of other diseases among the insane. In the majority of the cases the brain is pale, anæmic, irregularly vascular, with a tendency to softening of the white substance of the fornix and its neighbourhood, and the grey matter of lower specific gravity than in any other cases of insanity.

"5. Tubercle is not more frequently found in the nervous centres among the insane than among the sane, and when found, it does not in all cases, or even in the majority of them, produce any symptoms, and is not connected with any particular form of insanity.

"6. Tubercle of the peritoneum is not more frequent among the tubercular insane than among the same class in the sane. In the former it is more frequently associated with melancholia and monomania of suspicion than ordinary tuberculosis of the lungs.

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* Read before the Medical and Chirurgical Society of Edinburgh, Feb. 11th, 1863.

"7. The average age at death of the cases of tuberculosis is about three years below the average age at death among the insane generally, and the average age of those in whom *such* tubercular deposit is found is five years below the general average.

"8. The proportion of the tubercular who had had previous attacks of insanity is about the same as among the insane generally.

"9. There is hereditary predisposition in seven per cent. more of the cases of tuberculosis than of the insane generally.

"10. Monomania of suspicion is the form of insanity in which tuberculosis is most frequent, and general paralysis stands at the other end of the scale that marks the frequency of tuberculosis in the different forms of insanity; mania stands next to general paralysis, and melancholia to monomania of suspicion; while the tendency to dementia, in all forms of insanity, is greater among the tubercular than among the non-tubercular. A majority of the cases of general paralysis and mania die non-tubercular; a majority of the cases of melancholia, monomania, and dementia exhibit proofs of tuberculosis after death.

"11. In all the cases of general paralysis who were tubercular the disease had commenced with depression.

"12. In a certain number of cases (about one-fourth of all those in whom tubercle was found) the insanity is of such a peculiar and fixed type that it may be called 'phthisical mania.' In all those cases the phthisis is developed so soon after the insanity that tubercles must have already formed in the lungs, or a strong tubercular tendency been present and about to pass into actual tuberculosis when the insanity appeared. We know that the chief characteristic of tuberculosis is an impaired energy in the nutritive processes; and as a badly nourished bone becomes carious or necrosed for slight causes, or a badly nourished skin becomes subject to parasites, so disordered action results in those imperfectly nourished brain-cells from causes which would not be felt by a healthy brain. It is not the enfeebled nutrition directly so much as the perverted action to which the enfeebled nutrition predisposes, that produces the insanity. The peculiar mental state, the incurability of the insanity, the appearance of the brain after death, and its lowered specific gravity, all point to such a cause for the derangement.

"13. There is a special relation between deep melancholia with long-continued suicidal tendencies and refusal of food and lung disease—either gangrene or tubercular disorganization.

"14. There are a few cases in which the insanity is only a kind of delirium, occurring during previously developed chronic phthisis, and soon passing off.

"15. The prognosis is most unfavourable if tuberculosis occurs in any case of insanity.

"16. Half the cases of tuberculosis die within three years after the commencement of the insanity.

"17. There is no proof that the 'morbid influence of the pneumogastric nerve' has anything to do with the tuberculosis in cases of insanity.

"18. Long-continued insanity does not tend to the development of tuberculosis more than to the production of other diseases.

"19. Phthisis is entirely latent in between one-third and one-fourth of all the cases among the insane, and in almost all the others it is latent for a considerable time. This latency is most frequent in general paralysis, in which the majority of the cases of phthisis exhibit no symptom whatever.

"20. There are very few cases where the commencement of insanity benefits the phthisis; but in a few, where the phthisis is very chronic, an attack of insanity may be followed by the permanent disappearance of the phthisical symptoms, or attacks of mania may alternate with symptoms of phthisis. In by far the majority of such cases, however, the phthisical symptoms are merely marked, when the deposition and tubercle goes on."

X. *Summary.*

For the analysis of the following papers we have not space:

Annalen des Charité-Krankenhauses, &c., zu Berlin, xi. 1. 1863.—The entire number, 334 pages, is taken up with a critical inquiry by Dr. Joseph Mayer into the therapeutics of pleurisy, based on thirty-five cases, which are all given in detail.

On the Doctrine of Thrombosis and Emboli, especially in the Cerebral Vessels. Dr. H. Meissner, Leipsic. (Schmidt's Jahrb., ii. p. 209. 1863.)

Cases of Gastric Fistula. Dr. Müller, and Dr. Witt. (Schmidt's Jahrb., N. 2, p. 171. 1863.)

On the Changes in the Note of Metallic Tinkling in Pyo-pneumothorax. By Professor Merbach. (Schmidt's Jahrb., No. 10, 1863, p. 47. Quoted from Varges, Ztschrs. N. F. II. 1863. p. 16.)

A Case of Acute Atrophy of the Liver. Von Dr. Mann, Privat-docent in Halle. (Annalen des Charité-Krankenhauses, zu Berlin, x. 2, 1863, p. 109.)

General Considerations, Theoretical and Practical, on the Nature and Treatment of Yellow Fever. By M. Cazalas. Read before the Société Médicale d'Emulation. (L'Union Médicale for May 31, p. 408; June 2, p. 427.)

Report on Canine Madness. By M. Bouley. Read before the French Academy of Medicine. (L'Union Médicale for June 9, p. 469; June 11, p. 486; June 13, p. 504; June 16, p. 517.)

On Pellagra and Mental Alienation. By Dr. Pain. (L'Union Médicale, June 18, p. 534.)

On Amenorrhœa and Menorrhagic Fever. By M. Trousseau. (L'Union Médicale, June 23, p. 572; June 27, p. 600.)

On the Disease of the Respiratory Organs in Children. By M. Henri Roger. (L'Union Médicale, July 28, p. 177; Aug. 25, p. 374; Aug. 29, p. 404; Oct. 29, p. 193.)

On a Species of Epidemic Otitis and Otorrhœa which has prevailed for some months in Paris. By M. Bonnafont. (L'Union Médicale, August 1, p. 212.)

On the Diagnostic Value of an Accentuated Cardiac Second Sound. By J. Warburton Begbie, M.D. (Edinburgh Medical Journal, June, 1863.)

Discussion on Yellow Fever in the French Academy of Medicine. (L'Union Médicale, August 13, p. 298; August 15, p. 313; August 18, p. 327; Aug. 20, p. 350.)

Clinical Remarks on the Diseases of the Skin, called Parasitic. By Dr. Maurice Chausil. (L'Union Médicale, August 22, and following numbers, to September 15.)

Discussion on Hydrophobia in the Academy of Medicine. (L'Union Médicale, Sept. 17, and following numbers.)

On Pellagra. By M. Landouzy. (L'Union Médicale, Oct. 8, 13, and 17.)

On Glosso-Pharyngeal Paralysis. By M. Trousseau. (L'Union Médicale, Oct. 6, 10, 17, and 22.)

On the Etiology and Treatment of Asiatic Cholera. By Dr. Chabasse. (L'Union Médicale, August 11, p. 281.)

On the Mensuration of the Heart. By Professor Piorry. (L'Union Médicale, August 6, p. 243; August 8, p. 260.)

Case of Subacute Pleurisy following on a Perforation of the Diaphragm, produced by the Rupture of a Suppurated Hydatid Cyst. By Dr. Peter. (L'Union Médicale, October 24, p. 172.)

On the Present Aspect of the Doctrine of Cellular Pathology. By W. Turner, M.B. (Edinburgh Medical Journal, April, p. 873.)

On Diphtherial Nerve Affections. By Dr. E. H. Greenhow. (Edinburgh Medical Journal, August, 1863, p. 97.)

Cases of Diphtheria; with Remarks on the Treatment of Croup and the Throat Affection in Scarlatina. By Dr. G. Hamilton. (Edinburgh Medical Journal, August, p. 132; October, p. 315.)

Medical Notes on New Zealand. By Dr. J. B. Tuke. (Edinburgh Medical Journal, September, p. 220.)

Cases of Cancer of the Heart and Lungs. By Robert Law, M.D. (Dublin Quarterly Journal of Medical Science, May, 1863, p. 304.)

Cases of Pleuritic Effusion. By Samuel Gordon, M.B. (Dublin Quarterly Journal of Medical Science, May, p. 373.)

On Patency of the Foramen Ovale with Cyanosis. By B. W. Foster, M.D. (Dublin Quarterly Journal of Medical Science, August, p. 112.)

On the Natural History of Articular Rheumatism. By Austin Flint, M.D. (American Journal of Medical Science, July, 1863, p. 17.)

On Melancholia. By W. H. O. Sankey, M.D. (Journal of Mental Science, No. 46, July, 1863, p. 173.)

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

I. *On Foreign Bodies in the Ear.* By Dr. Voltolini. (Preuss. Méd. Zeitung, No. 10.)

The first thing we have to do is to assure ourselves that a foreign body really is within the ear, for it by no means rarely happens that persons apply under the belief that an insect or other body is within the ear, which the most exact inspection fails to discover. In some cases, inflammation of the membrana tympani is the cause of the deceptive sensation, and this becomes aggravated by the unsuccessful searching for the foreign body. On the other hand, persons sometimes have foreign bodies in the ear without being the least aware of it. The author removed a rolled-up, hairy leaf from the bottom of the meatus, in the case of a lady, who had not the slightest idea how it came there, and who consulted him for deafness of the other ear. In another case, a hexangular glass bead was removed, the patient being entirely ignorant that she had any foreign body in the ear. We should always make a very careful examination, and, when possible, by aid of the direct rays of the sun. No artificial or reflected light is a substitute for this; but where it is not attainable, Dr. Voltolini employs an apparatus of his own invention, which is also serviceable in laryngoscopy. The simplest means of all, however, is to fasten a wax-taper to the handle of a bright spoon in such a manner that the flame exactly reaches to the bowl of the spoon. Taking the spoon by its handle, and holding the light against the ear, by looking over it we are not dazzled, and can explore at our leisure. While in some cases the symptoms caused by foreign bodies in the ear are of a frightful intensity, in others they are wholly insignificant, and do not attract attention to the seat of mischief. For want of due examination of the ear, many patients complaining of giddiness, stupor, singing in the ears, &c., are sent to Carlsbad, Kissingen, or the sea-side, when all the mischief is due to a foreign body in the ear. Distant organs of the body may exhibit more or less considerable symptoms without, in some instances, the foreign body in the ear giving rise to any peculiar sensation, so that its presence remains unsuspected.

For the removal of foreign bodies we should first employ only the gentlest means, such as syringing the ear with warm water; and by this substances of the most different form and composition—even lead-pencil—may be removed. Beyond a bent forceps, an ear-scoop with a long handle, and a small corkscrew, almost all the instruments recommended for this purpose are more or less toys, or dangerous. By means of the corkscrew, wadding and similar soft substances may be easily drawn out; and in many cases we can remove bodies by passing the ear-scoop behind them. We should never employ force, and never should pass any instrument a line farther into the meatus than we can follow it with the eye. For want of such precaution, many a patient has lost his life or his hearing. The first effect of rough procedures is to make matters more obscure, the bleeding and swelling which ensue rendering complete inspection impossible. If the gentlest endeavours (or syringing), during which the eye guides the hand, do not succeed, the body should be left at rest in the ear—ay, even were it a dagger's point; and strong as the expression seems, the author justifies it by reference to cases on record in which pointed bodies have remained for years in the ear with impunity. It is not meant to be said that bodies should in general be left in the ear, but that matters should not be made worse than they are by violent manipulations. Leaving the body in the ear, then, warm-water syringing, and soft poultices, are to be daily resorted to, until the ensuing suppuration loosens it and gives it a new direction.

II. *On the Reduction of Dislocation of the Shoulder by Slow Manœuvres.* By M. Alphonse Salmon. (Gazette des Hôpitaux, 137.)

M. Salmon's plan consists in eluding muscular resistance by the employment of slow and gentle procedures, leading the patient to believe that he is rather engaged in examining the nature of the accident than rectifying the displacement. He is laid flat on the back, and so much on the edge of the bed that the half of the body corresponding to the injured side projects beyond, and is, so to say, suspended outside the bed. Even in health, such a position prevents persons exercising extensive movements with the arm without making great effort. An assistant stands beside the bed, to give the patient the idea of preventing his falling, but whose principal object it is really to leave him to make every effort with his sound side, for the purpose of securing for himself a solid position on the bed. The limb having been in the meantime carefully supported, so as to avoid all pain, the surgeon, standing by its side, gently grasps the hand and forearm, and draws the extremity from the trunk with extreme slowness, stopping whenever the patient manifests the slightest suffering, gently kneading the muscles situated around the point of the shoulder, frequently asking the patient whether he is giving him pain, and leading him to believe that the examination will be the more easily made in proportion as the pain is slight. All this may occupy some ten or fifteen minutes, during which the arm becomes, little by little, separated from the trunk, and gradually raised until it is parallel with the axis of the body. In order to effect the reduction, the surgeon gently transfers the limb to the care of an assistant, enjoining him to retain it without any effort in the position given to it, and places himself on the inner side of the limb, opposite to the axilla, as if he wished to explore the cavity of the joint. He now grasps the shoulder with the four fingers of each hand, joining them above the acromion, so as to render the scapula immovable. He next carries the two thumbs gently on to the head of the humerus, situated in the axilla, and, by exerting some pressure upon it—aided if necessary by slight extension made by the assistant who supports the limb—easily causes it to slip into the glenoïd cavity. The arm is then brought to the side, and, to the great astonishment of the patient, who generally has not experienced the slightest pain, the operation is at an end.

M. Salmon adduces six cases in proof of the simplicity and efficacy of the procedure, and exhibiting its superiority to those more forcible means commonly used, which cause much suffering, and often defeat their own object.

III. *On Syphilitic Epididymis.* By M. Dron. (Archives Générales, Nov. and Dec.)

In a brief historical survey, M. Dron shows that, among writers on syphilis, most have denied the existence of any syphilitic affection of the epididymis (apart from that due to gonorrhœa), while some have admitted that this organ may become affected by propagation of syphilitic disease of the testicle. None, however, have recognised a lesion of the epididymis existing independently of any affection of the testis itself. It is the object of this paper to prove that such a form of syphilis does really exist; and to this end the author furnishes accounts of sixteen cases he has met with at the Venereal Hospital at Lyons in the course of less than six months of the present year. Out of this number, it is true that in two there existed at the same time syphilitic orchitis; and the number of this class of cases might easily have been increased, had not the object been chiefly to demonstrate that the affection of the epididymis may occur in an isolated manner. Care has been taken to prove that the occurrence of this affection in the sixteen cases cannot be explained by the existence of gonorrhœa, contusion, or other causes; and there can be no reason to doubt that the tumour of the epididymis really constituted one of the manifestations of syphilis of which these patients were the subjects. The prompt efficacy of the anti-syphilitic treatment adopted strongly confirms this view.

When the head of the epididymis is not exclusively the seat of the syphilitic tumour, it is always the part in which it becomes most developed and persists the longest. Generally, the tumour does not exceed a nut or an olive in size, the largest being equal to a small walnut. Its surface is irregular and knobby, and its consistence increases in solidity with its duration. It is always easily distinguishable from the testis, and it is usually very indolent, so that the patients may not be themselves aware of its existence. In some cases, however, it is somewhat painful. The functions of the organ do not seem to be influenced by the presence of the tumour, which is also usually unaccompanied by lesions of any other portions of the spermatic apparatus. The period of its appearance cannot always be determined, so little has it excited the attention of the patient; but in some cases this has been between three and four months after the occurrence of the chancre, while in others it has been much later. Left to itself, its duration seems to be almost indefinite, as it is met with persisting years after the attack of syphilis. Under treatment, it has always terminated by resolution, about two months being the mean time required for this to take place. Acute or chronic epididymitis, from gonorrhœa or violence, may usually be distinguished easily from this affection; and although tuberculosis of the epididymis may at first resemble it, its very different mode of progress and resistance to specific treatment will establish the diagnosis. When syphilitic testis and syphilitic tumour of the epididymis co-exist, the respective lesions are easily distinguishable; while syphilitic testis, when existing alone, can scarcely be confounded with the affection of the epididymis. This integrity of the latter organ with a syphilitic condition of the testis is by no means a rare occurrence, however prolonged or considerable the testicular affection may be—an additional proof that syphilitic disease of the epididymis, when it exists, is not a mere extension of that of the testis, but an independent venereal accident. Although a lesion not of any great severity, it is usually coexistent with other symptoms indicative of a deep-seated affection of the economy; and it has been in several cases characterized by a disposition to relapse. Nevertheless it has, in all cases that have been watched, yielded to the means employed. Accordingly, as these tumours seemed to have belonged to the secondary or tertiary periods of syphilis, mercury or hydriodate of potass has been resorted to—these remedies having been employed in some cases also simultaneously or in succession. As, however, the lesion of the epididymis is usually only one among various symptoms of confirmed syphilis, it falls in with the treatment applicable to these in general. Local treatment is unnecessary.

IV. *On the Employment of Hypodermic Injections in Ophthalmic Practice.* By Dr. A. Von Graefe. (Archiv für Ophthalmologie, vol. ix. part 2.)

This method of treatment, Dr. von Graefe observes, is especially remarkable for the rapidity of its operation, and for the completeness and precision with which it may be applied. In the present paper he communicates the results of the extensive trials with the acetate of morphia which he has made during the last four years. As an instrument he prefers Luer's modification of Pravaz' syringe, which admits of the injection being made with great rapidity. The middle of the temple is the best locality, as neither considerable ecchymosis nor irritation of the skin is to be feared in consequence of the repetition of the injections. Allowing a day or two intervals, these may be performed a hundred times without any ill effect resulting. The sensibility of the skin is less here than in any other suitable part, while the loose condition of the subjacent connective tissue allows of a sufficient fold of skin to be raised and a sufficient quantity of fluid to be at once thrown in without inducing distension. It is essential that the fold of the skin should be well raised up, so that the point of the canula may be freely moved in the connective tissue prior to the injection being made. Before compressing the piston this fold should be let fall again, or the pressure exerted on the connective tissue may, on the withdrawal of the syringe, give rise to a partial return of the injected fluid. The quantity of morphia employed may vary from one-tenth to half a grain; on an average, one-sixth or one-fifth of a grain—the dose being, in fact, somewhat less than when given internally, as the effect produced is greater.

The following are the circumstances under which Dr. Von Graefe recommends the injections to be employed:—1. After injuries of the eye attended with severe pain, especially when these are accompanied with loss of the epithelium of the cornea, inducing exposure of the nerve and insupportable suffering. If unrelieved, this may give rise to a neuralgia of the cornea, preventing the use of the eye for a long period. In pain arising from contusions or penetrating wounds, the injections are of infinite more service and of less danger than local bleeding and the application of ice. 2. After operations upon the eye, when severe pain arises. They are often of surprising utility where this even depends upon mechanical causes of irritation, as in prolapse of fragments of membrane into the anterior chamber, or pressure of a portion of the lens against the iris. They must, however, be employed with caution after extraction, as they may give rise to vomiting. 3. In ophthalmia accompanying ciliary neurosis, in iritis, glaucomatous choroïditis, several forms of keratitis, &c. A strong morphia injection is an excellent means for reducing a glaucomatous attack prior to operation; and in those cases of glaucoma in which operation is no longer possible, the morphia is of far greater service employed in injections than when given internally. So also it often renders treatment—as for example in iritis—possible by subduing severe pain and its reflex effects. 4. Injection of morphia is the most rapid and certain antidote in poisoning from atropine, a not unseldom occurrence when solution of atropine is ordered to be frequently dropped into the eye, or when the solution has been swallowed in mistake. Chronic poisoning from continuous use of the atropine is often overlooked, the general erethitic debility and impaired assimilation which it gives rise to not being attributed to their true cause. 5. In neuralgic affections, especially of the frontal distribution of the trigeminus, good service is rendered by the injections: and in the typical supra-orbital neuralgia, applied in the course of the nerve, they often abbreviate the paroxysm and render the disease, when obstinate, more amenable to moderate doses of quinine. In the irregular neuralgic pains, not unfrequently met with, especially when the eyes are over-exerted, no remarkable benefit has been derived from morphia injections or other narcotic applications. The cases really suitable for them are where the paroxysms are accompanied by intermissions or decided remissions. In hyperæsthesia of the retina they have not proved of service. 6. Morphia injections play a very important part in certain reflex spasmodic affections of the eyelids, whether accompanying or succeeding inflammatory affections of the cornea and injuries of the eye, or arising spontaneously.

Although, reasoning from analogy, it might have been expected that morphia injections would prove useful in spontaneous mydriasis, Dr. Von Graefe, in several cases in which he has tried them, has failed to derive any decided benefit from them. Notwithstanding that the administration of these injections is a small operative procedure, patients get so accustomed to them as to become very urgent for their repetition; and it behoves the practitioner here, as indeed with the narcotics in general, to be guided by the indications for their employment rather than by the mere comfort of the patient. Dr. Von Graefe has made some trials of hypodermic injection of atropine, but with no satisfactory results.

V. *On a Case of Facial Phlebitis.* By M. Blachez.

(Gazette Hebdomadaire, No. 44.)

A soldier, forty-three years of age, brought into hospital with what appeared slight facial erysipelas, exhibited cerebral symptoms out of all proportion to the local lesion, and died next day. At the autopsy, the brain and its membranes were found quite healthy, but the cavernous sinuses and the circular sinus surrounding the sella turcica were filled with pus. On making a section of the face, well-defined ulceration of the pituitary membrane was found to exist at the anterior part of the right nasal fossa and the corresponding portion of the vomer; and from these ulcerations the pus could be traced through the orbit, the cellular tissue of which was infiltrated with it, to the cavernous sinus. The ophthalmic vein was filled with pus, and the branches of the frontal vein were the seat of intense phlebitis. Pus was found in both pleuræ and the lungs. There were very numerous centres of metastatic deposits.

This case is of additional interest, taken in connexion with the cases of furuncle of the face promptly followed by death, related by M. Trude, a Danish surgeon. In these the furuncles were small, and the pus proceeding from them was traced, with the attendant phlebitis, through the frontal and ophthalmic veins into the sinus. Considerable analogy also exists between this case and one related by M. Vigla, in his thesis on glanders (the case, however, not being one of glanders, but diagnosticated by Blandin as one of phlebitis), and another case related by Dr. Silvester in the 'Medico-Chirurgical Transactions' for 1841. It is probable that other cases may have been, as this one was, mistaken for erysipelas. The redness is, however, more limited in extent than in erysipelas, and it consists chiefly of livid patches following the course of the veins. These latter are hard, tense, and prominent, and the general symptoms are out of all proportion to the amount of local lesion. Even at the autopsy the nature of the case may be overlooked, if attention has not been called to the matter, so that the condition of the veins and sinuses may be examined.

[In a subsequent number of the same journal (No. 47), M. Dubreuil gives an account of two cases of multiple furuncle of the face, which were followed by phlebitis of the fronto-parietal and ophthalmic veins, with exophthalmos. The confluence of the furuncles in these cases was remarkable, and the author suggests that this, together with the great vascularity of the parts implicated, may perhaps aid in explaining why furuncles, which in other regions give rise to adenitis and lymphitis, here induce inflammation of the veins, converting what is usually a mild affection into a very dangerous one.]

VI. *On the Combination of Lithotrity with Lithotomy.* By Prof. ALQUIÉ. (Bull. de Thérap., vol. lxv. Nos. 4 and 5.)

After relating some cases in illustration, Prof. Alquié terminates his memoir with these conclusions: 1. In cases in which lithotomy seems indicated in the adult or aged, and the calculus is of medium or of large size (from three to five centimetres in thickness), lithotrity should be associated with the operation. 2. The same may be said for the child or adolescent when the calculus is thus large, lithotomy being at once practised when the stone is small and the case uncomplicated. 3. When the stone is of very considerable dimensions (6 centimetres or more), lithotrity should be practised through an opening in the perinæum. So, also, when there is a perinæal fistula which can be conveniently dilated. 4. When the stone is only of a medium or moderately large size, lithotrity should be practised by the normal passages, and frequently all cutting operation may be avoided.

VII. *On Lithotrity in Children.* By M. BEYRAN. (Gazette Méd., No. 38.)

Dr. Beyran takes the opportunity of the relation of a successful case of lithotrity in a boy eight years old, to express his opinion that this operation might be performed more frequently upon children than is the case at present with advantage. Now that instruments are constructed combining smallness of proportions with solidity of structure, this operation is often eligible, and the indocility of the child and sensitiveness of the urethra may be easily overcome by preliminary and tentative procedures, which, indeed, are required at all ages. In some respects children are more favourable subjects for it than adults, inasmuch as the stone is usually not large, while there is an absence of the various complications which may be met with in adult life, as stricture, enlargement of the prostate, tumours, or altered conditions of the bladder, &c. M. Beyran does not resort to chloroform, because he thinks that the employment of this agent should be reserved for prolonged and painful operations, which lithotrity is not when the preliminary passage of instruments has blunted the sensibility of the parts and the sittings are of very short duration. One very important rule of practice is to endeavour to prevent the entrance and impaction of fragments of calculi in the urethra, by keeping the child after the operation lying on his back with the pelvis highly raised. In this position he should pass his urine, and an instrument should be introduced two or three times a day. If fragments have entered the urethra, it is preferable to thrust them back again into the bladder rather than to crush them in the canal or extract them by the meatus.

VIII. *On a Case of Ovariotomy.* By Dr. STILLING. (Deutsche Klinik, Nos. 34 and 35.)

Dr. Stilling, the well-known anatomist of Cassel, takes the occasion of the relation of a successful case of ovariotomy to point out the fact of his having performed this operation so long ago as 1834, and again in 1848, the former case being attended with an unsuccessful and the latter with a successful result. He believes that to the precautions insisted upon by him as necessary in the management of the severed pedicle and the protection of the peritoneal cavity from subsequent effusions of pus or blood, much of the success of the later operations has been due. No author has, it seems, given him credit for these, it having become the fashion to describe the operations as performed after "the English method." [We fear his claim of any priority in the matter is rather a barren one, as it is very unlikely that any recent procedures have been influenced by these isolated cases published in obscure German periodicals more than twenty years since.]

IX. *On a Tumour of the Scrotum with Milk-like Fluid* (*Galactocele*). By M. DEMARQUAY. (Gazette Méd., No. 41.)

The young man who is the subject of this case was operated upon in July, 1862, a quantity of fluid, exactly resembling milk in appearance, being withdrawn from a tumour on the left side of the scrotum. An account of this was published in the 'Comptes Rendus de la Société de Chirurgie' and 'L'Union Méd.' for 1862. The patient was entirely cured, but at the end of a year returned, having a similar tumour on the right side. About 100 grammes of milk-like fluid, situated in the tunica vaginalis, were withdrawn by means of the trocar, the testes being found in a healthy condition. An iodine injection was thrown in, and the patient soon recovered. On examining the fluid, which soon underwent partial coagulation, a great abundance of fatty globules was observed, and what is of greater interest, several transparent animalculæ of elongated cylindrical form and lively movement. From the resemblance of the form of these animalculæ to that of the vinegar eel, it was at first supposed that they might have become introduced accidentally into the fluid, but this idea was at once rejected. They were seen while the fluid was still warm by several microscopists, and their representations led M. Davaine, who was unable to detect them in the dried specimens of the fluid, to regard them as resembling very exactly the embryos of nematoïd worms. It is quite the character of embryos to be only recognisable during life, their decomposition after death being as rapid as that of infusoria. It may be of some interest to state that the patient was a native of Havannah.

X. *Practical Remarks on Fractures in Children.* By M. GUERSANT. (Bull. de Thérapeutique, vol. lxv. Nos. 7 and 8.)

Fractures are as common in children as in aged persons. In both, the medullary canal of the long bones is large, and the walls of the diaphysis are thin; and in both, muscular power is feeble in its resistance to the effects of external violence. The fractures of children have this peculiarity about them, that they are sometimes incomplete, either from the bone bending and breaking only through half of its cylinder, or from its being, though broken right through, held in position by the periosteum. This explains why we so seldom see riding and change of position in the fragments, and the rapidity and ease with which reparation is obtained. Even in compound fractures the periosteum sometimes remains sufficiently intact to prevent displacement. For the same reason crepitation is often absent, or very obscure, and manipulation must not be too freely employed for its discovery, as a complete may thus be rendered an incomplete fracture. An exact diagnosis is, in fact, sometimes difficult. In general, when the cases are simple, and the child in good health, union will take place with perfect regularity and without shortening. M. Guersant has frequent visits from conscripts whom he has treated in childhood for fracture of the thigh, in the vain hope of obtaining certificates for exemption; but in many of the cases mere examination failed to show that fracture had ever existed. Even when there is some shortening of the femur, after awhile it ceases to be apparent, or a compensatory deviation of the pelvis is established. The union of fractures is sometimes retarded by acute disease; but with respect to the effect of rickets, this disease in its early stages impedes the formation of callus, which is not the case at its later period.

When we are not certain that a fracture is present, it is always best to apply an apparatus; and M. Guersant is of the opinion of Dupuytren, that the sooner apparatus is applied for fracture in children the better, taking great precautions to prevent or remove constriction. When in compound fractures the mutilation is considerable, continuous irrigation is an excellent method, which should afterwards be only gradually discontinued. Children recover better under these circumstances than adults; and when the injury is such as to render amputation necessary, its results are far more favourable than in the adult. M. Guersant has had frequently to regret not having more promptly decided upon its performance in cases in which the limbs have been crushed, primary amputation having been attended in his hands with more success than secondary. For crushed fingers or toes, however, cold irrigation, continued until the complete separation of the crushed parts, constitutes the best treatment.

In treating fractures in children, whenever, as is so often the case, frequent examination of the part has to be made, and in those of the lower extremity, in which the urine may defile the apparatus, Mr. Guersant prefers the movable apparatus, employing the immovable in cases not calling for such precautions. In the fractures of new-born children it suffices to retain the apparatus for twelve days; in those of about two years of age it will be prudent to require twenty days; and between the second and fifteenth year it should be kept on for an average of twenty-five days. It is rare at the end of these periods for consolidation not to have taken place.

XI. *Summary.*

Amputation.—Weber on Amputation above and below the Ankle. (Langenbeck Arch., vol. iv. No. 2—Critical comparison of the various operations.)—Linhart on Tibio-tarsal Amputation. (Wurzburg. Med. Zeit., vol. iv. No. 4. Autopsy of a case three years after Pirogoff's Operation.)—Porter's New Mode of Arresting Hæmorrhage after Amputation. (Dublin Quarterly, Nov.)—Volcker, Statistics of Amputation and Resection. (Langenb., vol. iv. No. 2. Tabular view of 119 cases occurring in Esmarch's Clinic at Kiel, 1854–62.)

Anus.—Demarquay, Case of Littré's Operation for Imperforate Anus, the child living for near four months. (Gaz. des Hôp., No. 129.)

Astigmatism.—Bumstead, Remarks on Astigmatism. (Amer. Med. Times, vol. vii. No. 18.)

Bronchocele.—Gouget and Rozan, Accounts of Epidemic Bronchocele in Garrisons of Colmar and Briançon. (Recueil du Med. Milit., Nos. 45, 46, 47.)

Cæsarean Operation.—Heillman, Successful Case of Cæsarean Operation. (Deutsche Klinik, No. 31.)

Caries.—Volkmann, Histology of Caries and Ostitis. (Langenb. Arch., vol. iv. No. 2.)

Club-foot.—Hueter and Volkmann on the Etiology of Club Foot. (Langenb. Arch., vol. iv. No. 2; and Deutsche Klinik, Nos. 34, 35.)

Dislocation.—Richet, Two Cases of Dislocation of the Cervical Spine. (Gaz. des Hôp., No. 144.)—Demme on Dislocation of one side of the Pelvis. (Schweizerische Med. Zeit., vol. i. No. 3. A rare case of dislocation without fracture, accompanied by rupture of the kidney.)

Ear.—Brenner, Treatment of Diseases of the Ear by the Galvanic Current. (Virchow's Archiv, vol. xxviii. No. 1.)—Duplay, Review of Recent Works on the Pathology of the Ear. (Archives Gén., Nov.)—Bonnafont, Three Cases of Deafness from Osseous Tumours of the Meatus Externus.—(Bull. de l'Acad., Oct. 15.)—Anderson on Diseases of the Ceruminous Glands of the Ear. (Glasgow Journal, Oct.)—Pagenstecher on Perforating the Mastoïd Process. (Langenb. Arch., vol. iv. No. 2.)—Weber on a new Speculum Auris. (Deutsche Klinik, No. 44.)—Ockel, Report on Diseases of the Ear at the Maximilian Hospital in 1858–61. (Petersburg Med. Zeit., vol. v. No. 9.)—Grüber on Perforating the Membrana Tympani. (Allg. Wien. Med. Zeit., Nos. 39–43. The author is convinced that, with our better instruments and more exact diagnosis, this operation should be revived.)—Pollitzer, Contributions relating to Diseases of the Ear. (Ibid., Nos. 30–33. The author describes a simple apparatus for injecting the Eustachian tube, and enlarges upon its utility in diagnosis and treatment.)

Excision.—Szymanowski on Partial Excision of the Sternum. (Deutsche Klinik, No. 31.)—Bennett, Case of Resection of Head of Humerus for Enchondroma. (American Quarterly, Oct.)—Eulenberg, Six Cases of Excision of the Hip-joint. (Greifswalder Med. Beitrage, vol. ii. No. 1.)

Eye.—Schiess, Contribution to the Statistics of the Diseases of the Eye. (Schweizensche Med. Zeit., vol. i. No. 3. Relates to nearly a thousand cases.) —Petrucci on Cataract Operations performed at Tours. (Gas. des Hôp., No. 133. These cases, 247 in number, occurred during five years of M. Herpin's practice—the recoveries amount to 247, and the failures to 51.)

Fractures.—Doig, Notes on Fractures. (Glasgow Journal, Oct.)—Hodgkin, New Apparatus for Compound Fracture of Femur and Tibia. (Amer. Med. Times, vol. vii. No. 15.)—Allaire on Fracture of the Metacarpus. (Rec. de Med. Militaire, Nos. 43, 44. An elaborate memoir upon the subject.)—Palmer, New Apparatus for Fracture of the Clavicle. (American Quarterly, July.)—Duer on a Case of Fracture of the Coronoïd Process of the Ulna. (Ibid., October.)—Prince on the Treatment of Ununited Fracture. (Ibid.)—Lumniezer on Excision and Metallic Sutures for False Joint from Ununited Fractures. (Wien. Wochenschrift, Nos. 43, 44.)

Gun-shot Wounds.—Quesnoy on Extraction of a Ball which had remained in the Foot three and a half years. (Recueil de Med. Mil., No. 44.)—Howard, Treatment of Gun-shot Wounds of the Chest by hermetically sealing. (Amer. Med. Times, vol. vii. No. 14. By this term the author indicates what he states to be a very successful practice of cleaning and paring the edges of the wound down to the ribs, and then securing union by sutures and collodion.)

Hemeralopia.—Icard, Observations on Hemeralopia observed in a Military Hospital. (Mem. de la Soc. de Méd. de Lyon, vol. ii.)

Hernia.—Porter on Symes's Operation for the Radical Cure of Hernia. (Dublin Quarterly, Nov.)—Hufschmidt, Remarkable Cases of Operation for Hernia. (Schweizer, Med. Zeit., vol. ii. No. 1.)—Moor on Case of Congenital

Umbilical Hernia. (Ibid., No. 3. An interesting anatomical account, with the view of illustrating the origin of this form of hernia.)—Falges on a New Truss. (Gaz. des Hôp., No. 103.)—Goyrand on Performance of Operations for Hernia in Extreme Cases. (Gaz. Med., Nos. 48, 49. Goyrand insists upon the danger of temporizing in these cases, the exhausted state of the patient depending in fact upon the existence of the obstruction.)—Fleury on Hæmorrhage after Operations for Hernia, performed during the Catamenia. (Gaz. des Hôp., No. 137.)

Hospital Gangrene.—Hamilton, Table of Thirty-three Cases of Hospital Gangrene. (Amer. Med. Times, vol. vii. No. 18. The bromine treatment was found to be more successful than that by nitric acid.)

Jaw.—Van Biervliet on Osteoplastic Excision of the Upper Jaw. (Bull. de l'Acad. de Belgique, No. 7.)—Debout on Mechanical Restoration of the Lower Jaw. (Bull. de Thérap., Nos. 4-9.)

Laryngoscope.—Merkel, Report on Recent Writings on the Laryngoscope. (Schmidt's Jahrb., No. 9.)—Turck, Clinical Illustrations by means of the Laryngoscope. (Wien. Allg. Med. Zeit., Nos. 29-34.)

Lithotomy.—Chassaignac on the Employment of the Ecraseur in Lithotomy (Gaz. des Hôp., No. 122. Case related to the Society of Surgery, but received with no encouragement.)—Sandford, Remarkable Case in a Boy. (American Quarterly, July. There were 47 calculi removed, giving an aggregate weight of 2 oz., the bladder being filled by and contracting around the mass.)—Dolbeau on a Remarkable Case. (Gaz. des Hôp., No. 101. The stone adhering to the bladder, could not be removed, and the patient died. The calculus measured 12 centimetres by 8, and weighed 151 drachms!)

Ophthalmoscope.—Just on Employment of the Ophthalmoscope for the Examination of Recruits. (Küchenmeister's Zeitschrift, No. 6.)

Orbit.—Maisonneuve on Subperiosteal Extirpation of an Eburnated Exostosis of the Orbit. (Gaz. Méd., No. 40. The author insists upon the great superiority of this procedure to that of attacking the tumour itself.)

Ovariotomy.—Crockatt on a Case of Ovariotomy. (Edinb. Monthly, Nov.)—Keith on Cases of Ovariotomy. (Ibid., Oct.)—Kimball on a Successful Case of Ovariotomy. (Boston Journal, vol. lviii. No. 20.)

Parotid Gland.—Verneuil on Extirpation of the Parotid with Preliminary Ligature of the Common Carotid. (Gaz. des Hôp., Nos. 110-132. An interesting discussion ensued at the Society of Surgery upon the general question of the propriety of preliminary ligatures.)

Phimosis.—Guersant on Operations for Phimosis in Children. (Bullet. de Thérap., vol. lxv. No. 4.

Plastic Operations.—Ollier on a Case of Rhinoplasty performed by means of Periosteal and Osseous Flaps. (Mém. de la Soc. de Méd. Lyons, vol. ii.)—Verhaeghe's Memoir on Perineoplasty. (Bull. de l'Acad. de Méd. de Belgique, No. 5.)—Lente on a Case of Cheiloplasty performed by Simonowski's method. (Amer. Med. Times, Sept. 12.)—Debout on Blepharoplasty. (Bull. de Thérap., vol. lxv. Nos. 3-6.)—Beck on Uranoplasty. (Langenbeck, Archiv, vol. iv. No. 2.)—Sédillot on Uranoplasty. (Gaz. Méd., No. 46.)—Warren on Operations for Cleft Palate. (American Quarterly, Oct.)

Polypus.—Moura-Bourdillon on Removal of Polypi of the Larynx. (Gaz. des Hôp., No. 129.)

Spina Bifida.—Case of Successful Operation for Spina Bifida. (Wien. Allg. Med. Zeit., Nos. 39-45.)

Spinal Column.—Thader on Spondylitis Deformans. (Langenbeck, Archiv, vol. iv. No. 2.)

Strabismus.—Donders, Pathology of Strabismus. (Annales d'Oculistique, Sept.)—Meyer on Strabismus. (Gaz. des Hôp., No. 135. The author describes an instrument for measuring the degree of strabismus.)

Stricture.—Voillemier on a New Urethrotome. (Gaz. des Hôp., No. 119.)—Beyran on a New Rotatory Urethrotome. (Ibid., No. 104.)—Parmentier on Urethrotomy. (Union Méd., No. 129. The author relates cases from Demarquay's practice in illustration of the importance of regarding this operation as only adjuvatory to dilatation.)—Bron on Urethrotomy. (Comptes Rendus de la Soc. de. Méd. de Lyons, vol. ii. A discussion on the subject took place at the Lyons Medical Society.)—Dittel on the Varieties of Urinary Fistulæ consequent on Stricture. (Wien. Allg. Med. Zeit., Nos. 32, 33.)

Syphilis.—Diday on Irrenoculability of Chancre. (Mém. de la Soc. de Méd. de Lyon, vol. ii. With a discussion.)—Bron on the Transmission of Syphilis by Vaccination. (Schmidt's Jahrb., No. 10. A critical review of the writings upon the subject.)—Reade on Syphilitic Disease of the Nervous System. (Dublin Quarterly, Nov.)

Teeth.—Mitscherlich on Replantation and Transplantation of the Teeth. (Langenb. Archiv, vol. iv. No. 2.)

Tongue.—Maisonneuve on Extirpation of the Tongue by *Cauterization en Flèches.* (Gaz. des Hôp., No. 138.)—Demme on Glossitis and its Treatment. (Schweizerische Med. Zeit., vol. ii. No. 1.)

Tracheotomy.—Laborde on a Canula Easy of Removal. (Bull. de Thérap., vol. lxv. No. 9.)—Szymanowski on the Operation of Tracheotomy. (Prag. Viertel., No. 3. A criticism on recent improvements in the operation.)

Tumours.—Spence, Operation for an enormous Tumour of the Face and Neck. (Dublin Quarterly, Nov.)

Uterus.—Kæberlé on Amputation of the Uterus and both Ovaries. (Presse Belge, No. 46. This operation was performed in consequence of the existence of a fibrous tumour of the uterus. Six months afterwards the patient continued quite well.)

Varix and Varicocele.—Sistach, Statistical Investigation of Varix and Varicocele. (Gaz. de Méd., Nos. 38–47. An elaborate memoir, prepared chiefly in relation to the exigencies of military service.)

Vesico-Vaginal Fistula.—Ulrich on Apparatus in Operations for Vesico-Vaginal Fistula. (Wien. Wochenblatt, No. 29.)—Cousot's Cases of Successful Treatment of Vesico-Vaginal Fistula by Cauterization. (Bull. de l'Acad. de Méd. de Belgique, No. 7.)

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D. Lond., F.R.C.P.
Obstetric Physician to, and Lecturer on Midwifery at, St. Thomas's Hospital.

I. The Unimpregnated State.

1. *On the Tubo-ovarian Connecting Apparatus of Pank, and the Mechanism of the Transit of the Ovulum into the Fallopian Tube.* By Dr. F. A. Kehrer. (Henle und Pfeuffer's Mag., 1863.)
2. *The various Cysts of the Fallopian Tube.* By Prof. C. Hennig. (Arch. d. Heilk., 1863.)
3. *On Chronic Uterine Infarctus.* By Prof. Seyfert. (Spit. Zeitung, 1863.)

1. Dr. Kehrer has subjected to fresh examination the presumed discovery of Pank, in 1843, of an organic connexion between the ovary and the pavilion of the Fallopian tube by means of pseudo-membranous bridges formed at the time of ovulation. Pank concluded that these bridges of false membrane were not the result of morbid peritonitis, but have a physiological cause and function, their purpose being to favour the transmission of the ovulum into the Fallopian tube; and that, this function fulfilled, they undergo retrogressive metamorphosis. Dr. Kehrer's observations were made upon sows. He found that free

peritoneal cellular outgrowths were formed either upon or around the ovaries and tubes more or less developed in every period of life; and that these cellular growths were therefore constant formations. Hence, says Kehrer, it is not correct to regard them as pathological products. In a small proportion of cases, bridge-like bands are formed between the borders of the infundibulum and the ovary, forming a kind of pseudo-membranous capsule. He observes that it is an interesting fact to find at the precise spot where the peritoneum is perforated—that is, at the ostium abdominale of the Fallopian tube—through which it communicates with the outer world, constant traces of a process that might be called peritonitis, if it were proper to give this name to a constant condition.

2. Professor C. Hennig gives a systematic account of the varieties of cystic formation found in the human Fallopian tube. He says that cysts in this structure are found in about 40 per cent. of all female autopsies.

(1.) *Terminal Hydatids.*—These are found slightly stalked, mostly simple, on the infundibular end. They vary in size from that of a hemp-seed to that of a pea. They are the remains of the extreme end of Müller's duct.

(2.) *Glandular Cysts* arise from the distension of the normal glandular tubes of the Fallopian tubes. They most frequently occur in advanced age.

(3.) In *Fallopian Gestation* a bladder structure is formed, which presents chorion villi when minutely examined.

(4.) The external cysts of the tube are mostly very small. They feel hard, look yellow, and are situated on the peritoneal investment. They probably arise from solid colloïd bodies.

(5.) *Saccular Dropsy of the Tube*, according to Rokitansky, mostly affects both tubes, and arises from chronic tubal catarrh, in consequence of which the two ends gradually become closed. The resulting cyst is serpentine, generally simple, rarely chambered through folds of mucous membrane, and may hold several pounds of variously-coloured fluid, of various consistency. The contents may be purulent, and lead to perforation. It may periodically be emptied externally through the uterus.

(6.) *Ovario-tubal Dropsy.*—It may happen that at the time when a Graafian follicle bursts the infundibulum may become adherent to the ovary, and thus give rise to the "cystes tubo-ovariennes."

3. According to Seyfert, with the exception of the period of reproduction, the uterus is never the seat of independent primary local affections; but the pathological processes observed in this organ have either spread from neighbouring structures, or are the local expression of a constitutional disease. Chronic uterine infarctus is solely the result of defective involution of the puerperal uterus, caused by an exudation-process affecting the entire uterine substance during childbed.

II. PREGNANCY.

1. *On Jaundice during Pregnancy.* By Drs. ZANDER, LEXIS, MARTIN, VIRCHOW, and L. MAYER. (Monatsschr. f. Geburtsk., Feb. 1863.)
2. *On Epidemic Jaundice in Pregnant Women; its Influence as a Cause of Abortion and Death.* By Dr. BARDINET, of Limoges. (L'Union Méd., Nov. 1863.)
3. *On the Causes of Icterus Gravidarum.* By Dr. FICINUS. (Monatsschr. f. Geburtsk., Aug. 1863.)
4. *On Ante-version of the Gravid Womb.* By Dr. V. HÜTER. (Ibid.)

1. At a meeting of the Berlin Obstetrical Society an interesting discussion arose upon the subject of jaundice during pregnancy, in which Drs. Zander,

Lexis, Martin, Virchow, and L. Mayer took part. Several cases were related. The association of jaundice with acute yellow atrophy of the liver was considered. Virchow had never observed a case of acute atrophy of the liver ending fatally in a pregnant woman. He related a case of jaundice in which dissection revealed a so-called tight-lace liver; the growing uterus had caused a nearly complete compression of the lower half of the liver forwards and upwards without causing atrophy. Liver diseases, especially acute parenchymatous hepatitis without jaundice, he says is not infrequent. The liver is often found swollen and fragile, the cells enlarged. An epidemic puerperal diphtheritis was raging in the Charité; the liver always showed marked acute changes, but no jaundice was observed during life. Virchow did not believe that the atrophy of the liver, described as acute, was really an acute process. Besides the disintegration of the cells, thickenings of the cellular tissue and of the vessels were found, and these indicated a pre-existing chronic process. He doubted whether the severe accidents attending the disease were due to the affections of the liver. He had always found contemporaneous parenchymatous disease of the kidneys with albuminuria, and so it appeared to him more probable that uræmia complicated with jaundice was concerned. The weight attached to the presence of tyrosin and leucin he could not acknowledge, since the same materials were found in enlarged livers—for example, in febrile puerperal diseases.

Dr. Martin related three cases of icterus gravidarum. The first had been completely observed in his Clinique; the second was recognised only on the dead body; the third was most probably due to phosphorus poisoning; it was the subject of a post-mortem Cæsarian section. In the first case, a healthy young woman, eight months pregnant, was seized with loss of appetite, loss of strength, intense headache, protracted constipation, and intense jaundice. The liver was not found to be enlarged; the stools were grey; urine contained much bile-colouring matter. There was no fever. A dead child was born nearly at term. Fever set in twenty-four hours after labour, with pain in the left side of the uterus. The lochia were very offensive. The patient was, however, recovering on the ninth day, when the same bad symptoms returned. Hæmorrhage followed. She recovered after a long illness.

2. In a memoir read to the Academy of Medicine, Dr. Bardinet relates the history of an epidemic of jaundice that occurred at Limoges at the end of 1859 and beginning of 1860. He stated the following propositions:

(1.) Jaundice may be produced in an epidemic form amongst pregnant women.

(2.) It is thus manifested in three different degrees.

(3.) Sometimes it remains in the state of *simple* or *benign jaundice*, and in no way interferes with gestation.

(4.) Sometimes, presenting a first degree of malignity, it constitutes what may be called *abortive jaundice*, and determines either an abortion or a premature labour, without other disasters.

(5.) Lastly, it assumes the character of *malignant jaundice*, and determines ataxic and comatose symptoms, which rapidly entail the death of mother and child.

The epidemic described did not bear upon pregnant women only; but it exercised a peculiar action upon them. Of 13 cases, 3 ended fatally. No post-mortem examination was made. Of the 13 children, 7 survived. None showed any sign of jaundice. The periods of pregnancy when jaundice appeared were: 6 cases during the sixth month; 1 at seven and a half; 2 in the eighth; and 1 the day after delivery at term.

In this very important memoir the author cites the accounts of similar epidemics described by Kerksig as occurring in the Palatinate in 1794; by M. Carpentier at Roubain, in 1854; by M. Douillé at Martinique. He also discusses particularly the propriety of inducing labour. The decision is rendered difficult by the fact, that some cases in no way compromise the safety of the patient or the continuance of pregnancy.

3. Dr. Ficinus relates two cases of jaundice in pregnant women. In the first case a primipara presented herself, much emaciated, and dark-yellow; the disease had lasted two months. She had still some weeks to go. Labour came on prematurely, resulting in the birth of a dead child. In fourteen days afterwards the jaundice had completely disappeared.

Case 2.—A woman suffered in her four pregnancies from cramps in the stomach and vomiting. The emaciation was extreme; œdema and albuminuria appeared in the latter months, and in the last three or four weeks jaundice. Immediately upon this an intolerable itching of the skin set in. Here was a case of jaundice appearing four times in one woman: the first child was born emaciated and sickly, but quickly recovered; the second and third children were born alive, one dying in convulsions at 20 weeks, the other becoming ricketty.

4. Dr. Hüter recals the references in other authors to Ante-version of the Gravid Womb, and condenses cases from Baudelocque, Boivin, Hachmann, Wilcke, and Godefroy. He then relates four cases from his own observation. As a point in the symptomatology of ante-version, as distinguished from that of retro-version, he calls attention to the usual absence of retention of urine. He explains this by the circumstance, that in anteversion the uterus falling upon the bladder from above leaves the entrances of the ureters free below the seat of pressure; whilst in retro-version, the uterus, rising from below, compresses the urethra or bladder below the entrance of the ureters. A full bladder is an obstacle to the occurrence of ante-version. The following is a case of ante-version occurring suddenly. A very large and powerful woman had borne three children normally. When four months pregnant, whilst busy washing the floor, she felt pain in the sacral region, and straining to void urine and fæces. This continued for two days, when blood escaped from the vagina in considerable quantity. She was thus observed by Dr. Hüter. Expulsive pain and effort like labour persisted; an elastic catheter drew off only a very small quantity of urine; pulsation of the abdomen was difficult, from the contraction of the abdominal muscles, but a hard tumour rising about an inch above the symphysis was felt. The anterior wall of the vagina was deeply depressed by a smooth uniform tumour. At first the os uteri could not be felt, but was finally reached by passing the fingers high up and backwards in the cavity of the sacrum. The cervix uteri was so bent upon the uterus that the lips of the os were in contact with the anterior wall of the vagina. Morphia was administered, and attempts were made to raise the fundus uteri from behind the symphysis. These only effected a slight elevation of the fundus, but were followed by mitigation of the pelvic distress. Next day the patient was in a half-comatose condition, the bladder was much distended, and some hæmorrhage continued. In the course of the day all these symptoms subsided. In the subsequent course of her pregnancy the patient suffered from frequent vomiting, and at intervals from hydrorrhœa. She had a natural labour.

III. Labour.

1. *Report of the Lying-in Institution of the Charité at Berlin during the Winter* 1861–2. By Dr. Nagel. (Annal. des Charité-Krankenh., 1863.)
2. *On the Mechanism of the Expulsion of the Head in Face-to-Pubes Presentations.* By Dr. J. Kidd. (Dublin Quar. Journ. of Med., Nov. 1863.)

3. *On the New Method of Turning by combined External and Internal Manipulation.* By Dr. Braxton Hicks. Proceedings of Obst. Soc. of London. (Lancet and Med. Times and Gaz., Nov. 1863.)
4. *On Cephalic Version by External Manipulation.* By Dr. Nivert. (Gaz. d. Hôpitaux, 1863.)
5. *On Tonic Spasmodic Uterine Contraction.* By Dr. Martin. (Mon. f. Geburtsk., June, 1863.)
6. *On Spasmodic Contraction of the Uterus, especially on Spastic Strictures of the Internal Os Uteri during the stage of Dilatation.* By Dr. Poppel. (Mon. f. Geburtsk., May, 1863.)
7. *A Cæsarean Section, with Remarks.* By Dr. J. L. Diener. (Mon. f. Geburtsk., Feb. 1863.)
8. *The Bruising or Squashing (Quetschung) of the Placenta as the Means of Arresting Hæmorrhage in Placenta Prævia.* By Dr. Pfeiffer. (Mon. f. Geburtsk., Sept. 1863.)
9. *On Placental Polypi and Placental Remains in the Cavity of the Uterus.* By Dr. Stadtfelt, of Copenhagen; translated by Dr. Moore. (Dub. Quart. Journ. of Med., Nov. 1863.)
10. *On a New "Porte-Nœud" to carry a Loop over the Child's Foot; and on a new Replacing Apparatus for Prolapsus of the Umbilical Cord.* By Dr. Hyernaux. (Bull. de l'Acad. Roy. de Méd. de Belgique, 1863.)

1. Dr. Nagel gives a detailed history of the labours occurring in the Berlin Charité during the winter 1861–2. The unfavourable influence of hospital midwifery is evinced in 83 severe puerperal diseases, and 46 deaths amongst 357 lying-in women—that is, nearly 23 per cent. fell ill, and 13 per cent. died. Such a mortality cannot possibly be accounted for by any concentration of cases of difficult labour. Hospital influences only are capable of producing such disastrous results; accordingly we find that 33 of the deaths are directly ascribed to puerperal fever.

A point of interest in this report is the research made into the weight of the new-born child at birth, and during the first following days. Nagel weighed 175 children. Adding the observations together, the following results were obtained: They weighed on the first day, 1157⅛ lbs.; second day, 1108½ lbs.; third day, 1103⅛ lbs.; fourth day, 1101⅞ lbs.; fifth day, 1107⅝ lbs.; sixth day, 1106¾ lbs.; seventh day, 1109¾ lbs.; eighth day, 1108¼ lbs. Thus the gross loss on the first day was 48⅝ lbs.; each child lost on the average 8½ ounces within the first twenty-four hours. From the fourth day there is a slight gain in weight.

2. Dr. Kidd relates two cases in which the head entered the pelvis with the face to pubes. In the first, a living child, at term, it was necessary to use the forceps. He calls attention to the circumstances: that, in such cases, the head descends till the forehead, where it joins the nose, rests on the arch of the pubes; that it rests here, as the occiput does in the first and second positions, and the occiput makes a sweep of the perinæum; that it is therefore necessary, when forceps are used, to carry the handles well forward. In another case the head was small, and softened; it descended low, and the chin emerged before the rotation began, or the occiput swept the perinæum.

3. Dr. Braxton Hicks describes a method of turning by combined internal and external manipulation. The head or shoulder presenting is pressed towards the nearest iliac fossa by two fingers passed through the cervix, whilst simultaneous pressure in the opposite direction is made by the other hand applied outside to the breech. By this double manœuvre the knees are brought down over cervix, and one is seized by the finger. Several cases were

related showing the extensive application of the method. In the discussion, Dr. Barnes referred to the history of the operation; and from experience of upwards of 100 cases, illustrated and enforced the principle upon which the operation is based.

4. Dr. Nivert, interne of the Paris Maternité, gives an historical account of turning by external manipulation. He says the operation has been carried out in the Maternité, the woman lying on her back. During the intervals of the pains the head is surrounded by a gentle and gradually increasing stronger pressure through the abdominal walls and the uterus by both hands; and through continuous pressure is pushed in the proper direction, when the membranes are ruptured, the better to secure it *in situ*.

[This operation falls very far short of the efficiency and extensive application of the method described by Dr. Braxton Hicks, and successfully practised by him and the reporter in numerous cases.—R. B.]

5. Dr. Martin analyses the phenomena of uterine spasmodic contraction, and relates the history of a case in which a blue-red coloration of the presenting parts of the child was caused by a stricture of the uterus. In such cases he advocates the administration of repeated grain-doses of ipecacuanha, to cause vomiting, and opium. Dr. Martin also relates a case in which he was of opinion that death was the consequence of protracted stricture of the lower segment of the uterus impeding delivery. The dimensions of the pelvis were ascertained to be normal; the position of the child (head) was natural; the only condition accounting for the dystochia being this spasmodic contraction of the uterus. He relates another case of dystochia from stricture of the uterus, in which a laceration of the vagina took place under spasmodic contraction. This gradually yielded under several doses of tartar emetic, gr. ij., and ipecacuanha, gr. j.; and a dead child was delivered by forceps. The mother recovered slowly under tedious cicatrization of the vaginal laceration. He relates two other cases, in which the head was caught by uterine stricture in presentations of the breech. In one of these, the mother recovered after suffering from inflammation of the cervix uteri; the other died of metro-phlebitis. He cites instances showing how a chill from exposure of the patient, and especially the injudicious use of secale, have caused spasmodic stricture of the uterus. He draws a distinction between stricture, trismus, and tetanus. He applies the term stricture to spasmodic, ring-like contraction of the os uteri internum; the difference between trismus and tetanus is not clearly described. It is a difference of degree rather than of kind. In trismus the spasmodic contraction extends upwards from the os externum uteri, embracing more or less of the lower segment of the uterus; whilst in tetanus the contraction extends higher, sometimes involving the entire organ.

6. Dr. Poppel relates several cases of extreme spastic stricture of the lower segment of the uterus during labour. In one case, cited from Professor Lehmann, death followed after vain attempts to deliver. Dr. Poppel considers the value of the various methods advised to overcome this contraction; and concludes that when warm-baths, chloroform, opium, belladonna-salve, &c., have failed, to resort to incisions.

7. Dr. Diener practises in the canton of Zurich. He says the conditions which call for the Cæsarean section are very rare in that district. The case he relates, with much detail, was that of a woman aged thirty-two, weak from birth, living in a room often damp. Her diet was sparing in nutritious and vegetable food. She had two natural labours; then a delivery by forceps; next a dead child was born, bearing a deep depression in the frontal bone. She

had suffered during four months of this fourth gestation from dragging pains in the sacrum and extremities. An incomplete paralysis of the legs followed, and persisted four years. She took cod-liver oil, but the deformity increased, although she gained strength, and in 1861 was again pregnant. The characteristic deformity of osteo-malacia was very pronounced. The accession of labour was waited for, and the Cæsarean section determined upon. The child was extracted living. The mother died on the third day. There was no trace of peritonitis. The uterine wound was perfectly closed. The conjugate diameter was 2″ 9‴; transverse, 3″ 4‴.

8. Dr. Pfeiffer discusses the several explanations that have been offered of the arrest of hæmorrhage when the placenta, in cases of placenta prævia, has been wholly detached, as in Simpson's method, or partially detached from the cervical zone, as in Barnes's and Cohen's method. He rejects the explanation given by Barnes, that it is owing to uterine contraction, and attributes it to the bruising or squashing of the placenta, which is presumed to attend the operation. He proposes to carry this idea into practice. He advises to pass the hand into the vagina, and, by one or two fingers passed through the cervix, to separate the placenta all round for half an inch or an inch, bruising the placenta as much as possible. [This theory and practice, of course, repose upon the hypothesis that the bleeding comes from the placenta, and not from the uterine surface.]

9. Dr. Stadfeldt gives a valuable memoir on placental remains in the uterus after labour at term and abortion. He agrees with Braun in opinion that such remains may occasionally be developed long after into form of polypi, giving rise to all the symptoms of polypus. He says also that these remains play an important part in metrorrhagia, both in the fifth period of delivery (i. e., after expulsion of placenta), and later in life. He believes that the "fongosités intra-utérines" of French authors are but forms of placental remains. These remains are more common than is supposed. Dr. Stadfeldt performed 65 dissections of puerperal women in the Fredrik's Hospital and Lying-in Institution at Copenhagen, and was present at 5 others during the years 1861, 1862. In 7 of these 70 cases considerable masses of placenta were found adherent. In 5 of the 7 the placenta was expelled naturally; in 2 it was removed by hand introduced into the uterus. This, he properly observes, does not exhibit the true frequency of their occurrence, as these remains probably cause and coincide with an increased amount of illness and death among puerperal women.

10. Dr. Hyernaux, of the Maternité at Brussels, in memoirs addressed to the Royal Academy of Medicine of Belgium, gives an elaborate and useful account of the various contrivances adapted to the replacement of the prolapsed umbilical cord, and to the seizure of the foot of the child, as preparatory to turning. He describes instruments of his own invention designed for these purposes. His "porte-nœud," for passing a loop over the child's foot, is especially ingenious. It consists of a hollow tube, furnished with a handle at one end; at the other end is a curved tube, resembling a horse-shoe, composed of two longitudinal halves, which may be brought into juxtaposition or separated at will. The two halves brought together form a complete tube, which receives the loop of the tape. A portion of the loop preserved free forms with the curved end of the tube a ring, which is carried over the foot. This done, the tube is opened, the entire loop is set free, and tightened upon the ankle.

The following Memoirs are indicated by title; they are not analysed, either from want of space, or because they are contained in easily accessible publications:

On the Mechanism of Labour in extremely contracted Rachitic Pelvis. By Dr. L. Feist. (Monatsschr. f. Geburtsk, 1863.)

On Proctocele Vaginalis as a Cause of Dystochia. By Dr. E. A. Meissner. (Ibid.)

Intra-uterine Perforative Peritonitis with a Hydrocephalic Child. By Dr. Breslau. (Ibid.)

On the Resistance of the Membranes of the Ovum as a Hindrance to Labour. By Dr. J. Poppel. (Ibid.) Dr. Poppel instituted experiments to show the power that is required to tear the membranes, and hence estimates the force *à tergo* which is brought to bear upon the membranes in labour.

On the Physiological Action of the Uterus in Parturition. By Dr. A. S. Donkin. (Edin. Med. Journ., Dec. 1863.)

Case of Enlarged Ovary. By Dr. J. Brisbane. (Edin. Med. Journ., Dec. 1863.)

On Prolapsus of the Gravid Uterus. By Dr. Gusserow. (Monatsschr. f. Geburtsk., Feb. 1863.)

Three Cases of Eclampsia Parturientum. By Dr. J. G. Grüllich. (Ibid., May, 1863.)

On a Case of Acute Mollities Ossium, occurring in a woman during her third pregnancy, upon whom the Cæsarean operation was performed: Preservation of the child; subsequent death of the mother. By J. C. Lory Marsh, M.D. (Lancet, Nov. 1863.)

On Two Successful Cases of Cæsarean Section. Communicated by Dr. Gabr. Taussig, of Rome. (Med. Times and Gaz., Oct. 1863.) The first case was performed by Professor Panuzzi. A point for remark in the treatment is the use of adhesive straps surrounding the body, to secure the union of the incision, instead of the "sutura cruenta;" and the application of cold. The second case was operated on by M. Cocchi in a similar manner. He applied snow during the first few days. Both mothers and children did well.

Cases in Midwifery. By Dr. Purefoy. (Dublin Quarterly Journal of Med., Nov. 1863.)

Report of Cases (26) of Forceps in the Dublin Lying-In Hospital for ten months—June 1862 to April 1863. By John Cronin. (Proceedings of Dublin Obstetrical Society: Dublin Quart. Journ. of Med., Nov. 1863.)

A Case of Ruptured Uterus. By Dr. J. R. Kirkpatrick. The head was hydrocephalic, containing 32 ounces of fluid. (Proc. of Dublin Soc.: Ibid.)

A Case of Ovarian Dropsy; and Fibrous Tumour of the Uterine Wall enclosed in a bony shell. By Dr. Jackard. (Amer. Journ. of Med. Sc., Oct. 1863.)

Rare Case of Peculiar Formation and Site of Placenta Prævia, with Recovery of Mother and Child. By Dr. B. Schuchardt. (Mon. f. Geburtsk., 1863.)

Notice of Diphtherial Gingivitis in Lying-in Women. By Dr. Matthews Duncan. (Edinb. Med. Journ., Dec. 1863.)

History of an Extra-uterine Gestation. By Dr. Winckel. (Monatsschr. f. Geburtsk.)

A Case of Extra-uterine Gestation. By Dr. L. Mayer. (Op. supra cit.)

Report on the Progress in the Knowledge of Generation during the year 1862. By Dr. W. Keferstein, of Göttingen. (Zeitschrift für rationnelle Medecin, 1863.)

NOTE ON A NEW MODE OF APPLYING SOME EXTERNAL AGENTS TO THE EYE.

By J. F. Streatfeild, F.R.C.S.

The use of paper as a medium for the application of sulphate of atropine and various other soluble salts to the eye has been before the public since my communication on the subject in the Ophthalmic Hospital Reports, in the number for January, 1862. This medium has been very much approved, and I may say generally adopted, owing to its convenience, and more or less also owing to its peculiar efficacy in some cases.* It has occurred to me that an improvement might be made if, in the new medium, we could secure, along with the convenience and efficacy pertaining to the paper, some vehicle which would not need to be removed from the eye after it had been applied, and the desired result had been gained. I have not now time or space even briefly to relate the advantages arising from, or the way of making the application by, the paper vehicle. I can only refer to my communication above mentioned, and to another in the succeeding number of the same journal. But I may allude to the alarm felt by some patients at any investigation of their eyes, even if only to remove the scrap of paper that has been introduced, and to the trouble sometimes found in removing it when it has got up beneath the upper lid. As the proposed substitute for paper need not be removed, the surgeon will often be saved a delay of twenty minutes, and will not be obliged to wait for the desired effect in order to remove the scrap of paper. Therefore, also, the patient can be trusted to employ the new medium himself, which is not the case with the old one. In order to obtain what I wanted, I first tried what is called "wafer-paper;" but this is brittle when dry, and loses all consistence when wet, so that it becomes unmanageable as soon as it is moistened; and moreover, the pulpy substance in the eye, if not washed out, might irritate. I am now employing gelatine, rolled out in sheets of the thickness of thin writing-paper, and imbued with a definite proportion of sulphate of atropine or any other salt required. This retains the advantages of, and only requires the same mode of application as, atropine paper, but *it need not be removed from the eye.* It is soon dissolved in the tears, and acts in every way well. The dose employed thus, it appears, is more quickly applied to the eye than when a similar dose has to be dissolved out of the meshes of tissue-paper; and to this it is that I attribute the pain felt by some of my patients when I have used a large square of "gelatine atropine-paper." They have not complained of pain when I have used a smaller bit (or dose), less than a whole square. I am thinking of having a check of small squares printed in green on the gelatine, to define the doses, and to make the transparent gelatine more readily visible, *when about to be used,* for it need not be looked for afterwards.†

BOOKS, &c., RECEIVED FOR REVIEW.

Topics of the Day: Medical, Social, and Scientific. By J. A. Hingeston. London, Churchill and Sons. 1863. pp. 900.

Manual on Extracting Teeth, &c. By A. Robertson, M.D. London, Trübner and Co. 1863. pp. 198.

Manual of the Metalloids. By J. Apjohn, M.D., F.R.S. London, Longman and Co. 1864. pp. 596. (Galbraith and Haughton's Scientific Manuals.)

On the Immunity enjoyed by the Stomach from being Digested by its own Secretion during Life. By F. W. Pavy, M.D. (Reprint from 'Transactions of the Royal Society.')

On the immediate Treatment of Stricture of the Urethra, by the employment of the Stricture Dilator. By B. Holt, F.R.C.S., &c. Second Edition. London, Churchill and Sons. 1863. pp. 110.

Remarks on some of the numerical Tests of the Health of Towns. By A. Ransome, M.B., and W. Royston. 1863. (Manchester and Salford Sanitary Association.)

Variation of the Death-rate in England. By Mr. W. Royston. (Paper read before the above-mentioned Association, Nov. 15th, 1860.)

On Ovarian Dropsy, with Cases of Ovariotomy. By J. Keith, M.D. (Paper read

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[*] The method was *invented and patented* in Paris a year after it was published in London!

[†] It is made by Mr. Squire, chemist to her Majesty, 277, Oxford-street, London, and will be shortly ready for the trial I hope it will obtain of my professional riends.

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Guide to the Asthmatic: on Asthma, its Nature, Complications, and Rational Treatment. By C. J. BERGER, Doctor of Medicine of the Faculty of Paris.—*Paris*, 1863. pp. 196.

THE early history of our knowledge of pulmonary emphysema may be comprised in a few sentences. We look in vain for any notice of it in the writings of the Greek, Roman, and Arabian physicians; and with the exception of an occasional post-mortem observation by the continental pathologists of the seventeenth and eighteenth centuries, an account of the examination of the lungs of a broken-winded horse, given by Sir John Floyer, in his 'Treatise of the Asthma,' and the well-known, but fragmentary, description by Baillie, in his 'Morbid Anatomy,' the history of pulmonary emphysema as a recognised disease commences with Laennec. Some of the earlier continental observers, as Bonetus and Morgagni, were aware that the lungs were occasionally found, after death, increased in volume, and abnormally distended with air; others, as Van Swieten and Storck, had noticed prominent vesicles of air under the pleura; but the true seat and pathology of the affection seems to have been generally overlooked. Ruysch and Valsalva, however, have both recorded cases in which they recognised enlargement and distension of the air-vesicles; and Sir John Floyer, in his account of the dissection of a broken-winded mare, distinctly describes the dilatation of the air-cells, and asserts the probability of a similar change taking place in man. The passage which is quoted by Forbes in his translation of Laennec, is so curious for its antiquated phraseology and for its pathology, the latter certainly in advance of the time at which it appeared—1698—that we are tempted to reproduce it here:

> "As it happens in external flatulent tumours, they at first go off and return, but at last fix in permanent flatulent tumours, so it is in the flatulent asthma—the frequent nervous inflations induce at last a constant windy tumour or inflation; and it ought to be considered how far holding the breath in hysteric fits, or the violent coughing in long catarrhs, or the great distension of the lungs by an inflammation, may strain the bladders and their muscular fibres, and thereby produce the same rupture, or dilatation, or hernia, as happens in the broken-winded. This must be observed by the help of the microscope; and if the air blown into the lobe will not be expelled thence by the natural tone or muscle of the bladders, that the lobe may again subside of itself, 'tis certain some injury is done to the ventiducts; the bladders are either broken and admit the air into the membranous interstices, or else they are over-distended, like a hernia in the peritoneum; and this will produce an inflation of the whole substance of the lungs, and that a continual compression of the air and bloodvessels, which will produce a constant asthma."[1]

Baillie, who has been frequently said to have anticipated Laennec's discovery, describes and figures the dilatation of the air-cells, and was the first to compare the altered lung-structure to that of the lungs of amphibia—a comparison which has become stereotyped. He, however, regarded this condition as a very rare one, having himself met with only three cases. He attributes the cause of distension of the cells to an accumulation of air in them, arising from some obstacle to its free egress from the lung, and supposes it probable that an accumulation so arising may break down the partitions between contiguous cells, and so form one of large size. He has also noticed two other pathological conditions appertaining to pulmonary emphysema, but clearly failed to connect them in any way with the preceding observation. One was, that the lungs are not infrequently found abnormally distended with air, so that on opening the chest they fail to collapse. Associated with this condition, he found the branches of the trachea usually more or less filled with mucous fluid, and to this obstacle to the egress of the air he attributed the gradual distension of the cells. He also observed that, in certain cases, vesicles of air were found attached to the edges of the lungs. But he not only failed to trace their connexion with the lung-structure, but expressly asserted that they did not communicate with the air-cells. He believed them to be "a morbid structure, formed in the same manner as the air-vesicles attached to the intestines and mesentery of some quadrupeds, and concluded that the contained air was secreted by the neighbouring capillaries." He adds, that although, when the air-vesicles have been found much enlarged, the person during life has been remarked to have been long subject to difficult breathing, yet he knew of no symptom by which the disease may be diagnosed from other thoracic affections.

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[1] Treatise on Asthma, p. 244.

Such was the fragmentary and imperfect state of knowledge of pulmonary emphysema previous to the appearance of Laennec's great work. To say that his description of it is in itself perfect, or that subsequent observations and reflection have entirely confirmed his reasoning as to its causation, or his clinical experience as to its auscultatory phenomena, would be affirming too much. But in reading his account of the disease, and comparing it with those of his successors, no one can fail being struck with the accuracy of his clinical and anatomical observations, and the sagacity of his conclusions. Although in the strictest language he cannot be said to have discovered the disease, and although he had not obtained a full insight into its mechanical or pathological production, he was its first clinical observer, the first who elevated it into the rank of a substantive condition to be diagnosed and treated; and we fully agree with the spirit, if not with the letter of Rokitansky's eulogium: "Had Laennec done nothing else for medical science, his discovery of this diseased condition (vesicular emphysema), and of the causes giving rise to it, would have sufficed to render his name immortal." One great step in advance made by Laennec was to draw a clear and well-defined distinction between the vesicular and interlobular forms of pulmonary emphysema. The vesicular form, as described by him, consists of a simple dilatation of the air-cells—"in some sort, an exaggeration of the actual condition of the viscus." Besides the fact of dilatation, his anatomical description embraces the frequent reunion of several air-cells through rupture of their intermediate partitions; the occasional occurrence of distended vesicles, prominent, globular, and apparently pediculated on the surfaces of the lung; the communication of such enlarged vesicles with the adjoining air-cells and with the bronchi; the distinction to be drawn between such enlarged superficial vesicles and extravasations of air under the pleura, the air contained in the former not passing under the contiguous pleura when pressed by the finger; dilatation of the bronchial tubes in connexion with emphysematous portions of lung; the different extents to which emphysema may affect the lung, the whole or parts of one or both being involved; the existence of partial emphysema with phthisis, and its constant occurrence in patients who have suffered from dyspnœa, from whatever cause. He also notices the increased volume and imperfect collapse of the emphysematous lung, and the altered character and diminution of crepitation yielded on pressure. The latter he refers to a slower escape of air from the cells, depending, he believed, either on "a more difficult communication between the air in the cells and that in the bronchi, or else on a diminished elasticity of the air-cells themselves." The first-named cause he finds in the obstruction produced in the smaller bronchi by the condition which he termed "dry catarrh," and which he believed to be the most frequent cause of emphysema. The diminution of elasticity of the air-cells, he thought, might be a concomitant of a thickening of their membrane, due to habitual distension, and concluded that emphysema was productive of a certain degree of hypertrophy. His reasoning as to the cause of vesicular emphysema is chiefly or entirely mechanical; he does not appear to have entertained the idea of an altered or pathological condition of the pulmonary tissue (apart from the bronchial tubes), as the first step in the chain of causes from which emphysema may result.

As it is not our intention to give the present article the character of a mere historical summary, we shall not attempt to review *seriatim* the views and conclusions of the successors of Laennec, but shall rather endeavour to epitomize some of the chief of them, in an examination of the actual state of scientific opinion and knowledge on the subject.

The work of Dr. Waters, which we have placed first on the list of materials for the present review, is a valuable contribution to pathological science in this particular, that it recognises and defines more clearly than we think has been hitherto done two forms of vesicular emphysema. One is the *partial lobular* or *lobular* form, which is familiar as supervening on various forms of disease of the respiratory organs in which long-continued and violent cough is a prominent symptom. The other, the *lobar* or general emphysema, is a graver malady, in which at least a whole lobe of a lung, if not the whole of one or both of the organs, is affected. This latter form of disease frequently comes on insidiously, without any previous history of bronchial or pulmonary affection, without the patient having been subject to severe cough or other cause which might be supposed sufficient mechanically to induce the dilatation of the air-cells. The subjects of the disease are not infrequently young; and the only rational mode of explanation of the morbid condition is that, in the absence of any sufficient mechanical cause, some imperfect process of nutrition, some degeneration, in fact, has been going on in the tissue of the lung, which has so modified the structures composing the walls of the air-cells as to allow of their abnormally yielding to the distending force of the contained air. We have no doubt that a neglect in distinguishing these two forms of disease has led to much of the discrepancy which prevails, not only as to the pathology, but as to the mechanical causation of vesicular emphysema. Although we willingly attribute to Dr. Waters all the merit due to a clear and truthful account of the two varieties of vesicular emphysema, and their essential difference, it must not be supposed that he was the first to insist on the importance of recognising the *lobar* form of the disease as due to degenerative change. Many years ago, M. Lombard described the lobar emphysema, and remarked that in it the intervesicular tissue appeared to have been absorbed, and the bloodvessels obliterated: these changes he believed to be the cause, rather than the effect, of the dilatation. This also was the condition to which Andral gave the name of *atrophy* of the lung—a term which he preferred to that of vesicular emphysema, proposed by Laennec. Dr. C. J. B. Williams also, in his earlier writings, has described two opposite conditions of the texture of the lung as accompanying emphysema, although he apparently did not see sufficient reason to erect them into distinct nosological groups, markedly differing in the extent of emphysematous disease, its cause and history. The different degrees of dilatation, he wrote,

> "may be accompanied by very opposite conditions of the texture of the lung. Sometimes this has acquired an increased rigidity; it does not collapse where the chest is opened, and resists the impression of the fingers more than a healthy lung. There are, notwithstanding the dilatation of the air-cells and its general lightness, hypertrophy and toughness of some of its textures, and portions near the root are sometimes found considerably indurated. With this condition there are commonly associated an altered state of the air-tubes, redness and thickening of the mucous membrane, hypertrophy of the longitudinal fibres, dilatation or partial contraction. In other cases, there is the opposite condition of the lung. The texture is more flaccid and yielding than usual; and when the margin of a lobe is pressed between the fingers, it feels thin, almost like a single membrane. It often does not collapse on opening the chest, and this appears to arise from a loss of elasticity, for it pits on pressure like an œdematous lung, and is commonly much paler than usual."[1]

In this passage, it seems to us, we have—although it is by no means the inference of the author—in the former case, a description of a lung, in which emphysema has supervened on chronic bronchitis or other inflammatory disease of long standing; in the latter, that of a lung in which degeneration has been the primary cause of the dilatation of the vesicles.

What may be the exact nature of the degenerative change which renders a large portion, and even in many cases the whole of the vesicular tissue of one or both lungs capable of abnormally yielding to the ordinary distending force of the received air, is a question which, as yet, has received no satisfactory answer. The theory of fatty degeneration has been advanced, but it rests on very slight foundation. In the Thirty-first Volume of the 'Transactions of the Medico-Chirurgical Society' is a paper by Mr. Rainey, in which he describes the microscopic appearances of the lung-tissue in a case of emphysema, where the intervesicular membrane was studded with brightish spots of a circular form, varying in size and number, which proved to be due to the presence of oil in the tissue. In this case, however, the emphysema was partial, not general; for Mr. Rainey states "that the general aspect of the lungs, especially in the vicinity of the emphysematous part, was healthy," with the exception of some small tubercular deposits. Apart from this single observation, there are, we believe, no published facts to support the theory of fatty change, although that theory has been adopted by Dr. Williams in his recent Lectures delivered before the Royal College of Physicians of London. Dr. Waters, whose well-known and careful observations on the structure of healthy lung give him a claim to speak with authority on such a matter, distinctly negatives the fatty-degeneration theory:

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[1] Dr. Tweedie's Library of Medicine, vol. iii. p. 156.

> "With the view of ascertaining whether emphysema is preceded by or has associated with it, fatty degeneration of the pulmonary tissue, I have made a careful examination of a large number of specimens of lungs which were the seat of the disease. These specimens were taken from lungs which presented the affection in all its varieties, whether partial or general. I have not only submitted to examination the diseased portions, but (where the disease was partial) pieces taken from contiguous parts, and where the lung-tissue was apparently healthy. The general results of my investigations may be briefly stated as follows:—In the large majority of cases, I have found no indications whatever of fatty matter; in some few instances, however, I have seen deposits of fat in the walls of the air-sacs. My examinations have been conducted with the microscope on recent and dried specimens, and also by heating the lung-tissue between pieces of glass, so as to dissolve out the fat, if present, and thus get indications of its existence." (pp. 28, 29.)

Dr. Waters, in addition, states that, on the supposition that the disease might be due to some affection of the capillary bloodvessels, he has carefully examined for atheromatous or fatty deposits the branches of the pulmonary artery, from their commencement to their termination in the pulmonary plexus. In some cases, he has found no indication whatever of such conditions; in others, he has found atheroma of the pulmonary branches and their capillaries; but this has always been associated with atheromatous deposits in the aorta, showing that the pulmonary vessels only shared in a general tendency to arterial degeneration.

Dr. Jenner, whilst allowing that fatty degeneration may be one of the anatomical conditions on which loss of elasticity and contractility depends, has been led by his observations to the conclusion, that the most frequent change in the lung-tissue productive of emphysema is fibrous degeneration, "the consequence of the exudation of that variety of lymph which escapes from the capillaries where they are the seat of slight but long-continued congestion, in a person of tolerably healthy constitution." From the context, however, it appears that he is specially referring to the partial variety of emphysema in which one or more vesicles at the apex or margin of the lung are greatly enlarged, rather than to the general pulmonary or lobar form. There is no doubt that the congestion of the pulmonary plexus, which takes place in various inflammatory affections, may terminate in an exudation of lymph which shall materially impair the elasticity and contractility of the vesicular tissue. This is probably the condition referred to by Laennec, Louis, and Dr. Williams as hypertrophy of the lung-substance, although they regard it as the effect, not the cause, of the emphysema. This pathological change is constantly met with in the lungs of old persons who have been the subjects of chronic bronchitis:

> "So common," writes Dr. Jenner, "is the exudation of this variety of lymph in the congestions of old persons, that the changes of tissue which result from its presence are to be reckoned among the degenerations incident to old age. The loss of elasticity and contractility, and permanent dilatation of the part which is its seat, if subjected to a distending force, is well illustrated by a

reference to the large size so often attained by varicose veins when their walls are the seat of this degeneration."[1]

The applicability of these observations to one form of the affection is scarcely to be questioned; but admitting that many cases of emphysema, chiefly of the partial variety, have their origin in fibroïd degeneration, the result of congestion and effusion, there still remains the more serious form of disease, involving frequently the whole organ, overtaking the patient insidiously, without being preceded by any appreciable congestive or inflammatory condition, to be accounted for.

Although the exact nature of the change is not understood, its anatomical characters have been most carefully studied by Dr. Waters, whose description we shall now follow. Dr. Waters agrees with other writers that there are certain anatomical changes which are common to all kinds of vesicular emphysema. These may be said to be, simple dilatation of the air-sacs, increase in the size of the alveoli, or cup-like depressions which occupy the sides and bottom of the air-sacs, and a diminution in the height of the alveolar septa, and ultimately their more or less complete obliteration. The dilatation is accompanied by a thinning of the walls of the air-sacs, and a divergence of the elastic fibres which enter into their composition. This condition terminates in a perforation of the walls themselves:

"The perforation at first is but slight; here and there a circular or oval opening may be seen in the membrane; as the disease progresses, these openings become more numerous, and larger; in some instances, the whole of the walls of the air-sacs and the septa of the alveoli being perfectly riddled with small openings, so that a horizontal section of the lung-substance has a general cribriform appearance. These openings are, for the most part, either circular or slightly oval. They exist in all parts of the walls, and are often seen in the septa between the alveoli, before the air-sacs are sufficiently distended to obliterate the septa. The subsequent steps in the progress of the disease consist in a further distension of the air-sacs, an enlargement of their perforations, and a rupture of the fibres of which their walls are composed. As these results take place, the walls become more and more imperfect, and the openings in them coalesce. A further breaking down of the walls then occurs, so as to leave but very partial partitions between the cavities; and in the most advanced stages of the disease these partitions undergo other changes, and are reduced to mere membranous shreds, or thin fibrous cords, passing in various directions, traversing, in fact, the distended sacs, two or more of which, by the destruction of their walls, have united to form a single cavity. These cavities occasionally assume a large size, and project from the margin of the lung; they sometimes also form appendages, being connected with the body of the lung merely by stalk-like processes." (pp. 15, 16.)

The nature of these appendages, as also of the dilated cavities, is proved by their presenting on their inner surface the epithelial covering of the normal air-sacs.

The great anatomical distinction which Dr. Waters has observed between the lobular or partial and lobar or general forms of the disease, apart from their seat and extent, is, that in the lobular kind he has found the air-sacs distended to a very considerable extent, but with little or no perforation; whereas in the lobar variety he has noticed the opposite condition—very extensive perforation, with certainly not more, and in some instances less dilatation than in the former. This fact, he justly concludes, points to the existence of a degeneration of the lung-tissue, which renders it more liable to break down under dilatation.

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[1] Medico-Chirurgical Transactions, vol. xl. p. 26.

The condition of the bloodvessels in emphysematous lung accounts for its anæmic appearance. In the stage of simple dilatation, the capillaries of the pulmonary plexus are farther apart, and their meshes are wider; when perforation takes place, and the walls of the air-cells are broken up, the capillaries become ruptured, giving rise to one form of the hæmorrhage which is observed occasionally in the disease. In the extreme condition in which the air-cells are only divided by membranous or fibrous shreds, the bloodvessels, much reduced in number, are seen traversing the remains of the septa, and from their diminished numbers it is clear that the respiratory function of the part must be almost abolished. The only result at which Dr. Waters has arrived by microscopical examination of the separate tissues entering into the structure of the vesicular walls is, that in emphysematous tissue the elastic fibres sometimes appear less regular in their outline, and have less tendency to curl at their ends; in other respects, he has observed no change of structure in the fibres, nor in the basement membrane of which the walls are composed. This, however, he argues, is no proof that degeneration does not exist, but simply that our present means of investigation fail to demonstrate it:

> "It is quite possible that the elasticity of the yellow fibres may become impaired, or even destroyed, without any such structural alteration resulting as could be appreciated, even with the highest powers of the microscope; and it is also equally possible that changes may occur in the bloodvessels, giving rise to malnutrition of the pulmonary tissue, and yet that we shall be unable to distinguish them. We see in this disease, as in many others, the secondary changes, . . . but the primary and essential ones we cannot recognise." (p. 31.)

The condition of the bronchial tubes varies with the variety of the disease and the history of the case. In old-standing cases, Dr. Waters has observed the tubes leading to the emphysematous tissue dilated. The amount of vascularity of the lining membrane of the bronchi will vary with the previous amount of bronchitis. The smaller tubes, however, leading to an emphysematous portion, are usually of a paler colour than those leading to a healthy portion of lung—a fact in consonance with the anæmic condition of the pulmonary tissue, which ultimately follows the dilatation and perforation of the walls of the air-vesicles, whatever may have been the nature of the pathological condition which has led to the impairment of their structure. In old cases of the lobar form of the disease, Dr. Waters has noticed that the circular muscular fibres of the bronchi become more highly developed —a fact which is very apparent in the smaller tubes.

This description is the most complete that we have met with of the anatomical appearances presented by emphysematous lung. But it would be doing great injustice to other observers to suppose that many of the points which Dr. Waters has carefully worked out have not been previously noticed. For instance, Dr. Walshe speaks of "atrophous degeneration of the walls of the cells," as leading the way to emphysema in a large proportion of cases. Again, in reference to the fatty-degeneration theory, he says that the occurrence of oil in the walls of the cells is not constant, and that when present, its relationship, of cause or of effect, to the existing atrophy is uncertain. He refers also to the obliteration and destruction of minute vessels entailed by emphysema, and clearly points out that, whilst one form of emphysema results from atrophous degeneration, the disease, however arising, necessarily brings about *secondary* degenerative changes and atrophy. On the other hand, it must not be concluded that atrophy and degeneration have been universally recognised as either the cause or consequence of the disease. Louis, following Laennec in this particular, considered that the change in the pulmonary tissue was one of hypertrophy rather than of atrophy, and accounted for it on the principle that all hollow organs become thickened in proportion as they are dilated, as, for instance, the œsophagus, the stomach, the heart, the bladder, when these viscera are dilated in consequence of obstructive disease of their outlets. The answer given by Dr. Waters to this reasoning is obvious. First, that in emphysema there is no obstruction to the exit of air from the air-sacs; and secondly, that the air is not driven from the air-sacs by active forcible contraction of their walls, but by passive elastic reaction. In emphysema, the elasticity being impaired or lost, the air is retained passively, there is no extra work to expel it performed by the air-cells, and consequently no element of hypertrophy exists.

Having thus reviewed the leading opinions as to the pathological conditions in which emphysema may originate, there yet remain a class of cases wherein no such train of morbid changes can be asserted to exist. Dilatation of the air-vesicles may be produced in otherwise perfectly healthy lung-tissue from mechanical causes alone. In the forms of disease we have hitherto had under consideration, the mechanical cause is secondary to the pathological; but there are numerous cases, such, for instance, as those of acute pulmonary emphysema, occurring in childhood, where the mechanical is the only mode of production which can be asserted. In every case of dilatation, whether in a previously healthy or diseased lung, that dilatation must be the result of force applied to the inner surface of the air-cell. But how the force so acting is produced, and by what mechanism the distending power is applied, has been a fruitful source of discussion since the disease was first observed and described.

Two theories—the inspiratory and the expiratory—have in turn asserted a claim to be regarded as the true explanation of the phenomena, and each has been supported by a reference to clinical observation, as well as by an appeal to physiological and mechanical laws. All allow that the immediate physical cause of distension is the pressure of air on the inner surface of the vesicle; but it is asserted by one set of reasoners that the forces employed in producing the pressure are those by which the air is made to enter the lung, whilst their opponents maintain that to the forces which expel the respired air is mainly due the result in question.

Laennec's theory, although sometimes called expiratory, is in reality, as Dr. Jenner has observed, an inspiratory one. We have already seen that the cases which he observed led him to regard the disease as originating in the condition which he termed "dry catarrh," and he believed its primary cause to be the obstruction of the smaller bronchial tubes by "pearly sputa," or by the swelling of their lining membrane. The powerful inspiratory force would, he reasoned, suffice to overcome such obstruction, and to secure the passage of the air into the cells; but when there, the force of expiration consisting only of the elasticity of the parts and the feeble contraction of the intercostal muscles, would be unequal to its expulsion. The air would thus be imprisoned in the cells, and each forcible inspiration would increase its quantity and their distension, the latter being also augmented by the rarefaction of the air consequent on increased temperature.

We need scarcely observe that there are fatal objections to this theory. The first is, that the forces of inspiration and expiration are not nearly so disproportionate as Laennec assumed. In calm respiration it is probable that they nearly counterbalance each other, whilst in healthy forced expiration the muscular power brought into action is considerably in excess of that in inspiration. But besides this, actual observation has shown Laennec to be in error with regard to the effect of plugging of the smaller bronchi by secretion. Gairdner and others have demonstrated that the result of such obstruction is to produce collapse, and not dilatation, in the pulmonary tissue to which the obstructed bronchi lead. As the bronchial tubes taper towards their extremities, each inspiration has the effect of forcing a plug of mucus into the narrower portions of the tube, from which it will with more and more difficulty be dislodged. The result, therefore, is, that with each inspiration the tube becomes more tightly closed against the passage of air. If the plug be placed over a bifurcation, it will be liable to fall back upon one of the subdivisions during inspiration, "in the manner of a ball-valve upon the orifice of a syringe, and thus completely occlude it."[1] Each expiration will expel a portion of air, which, however, owing to the valve-like action of the plug, and the weaker force of inspiration, will not be restored. The effect, unless the plug be expelled by violent coughing, must, after a time, inevitably be perfect collapse of the portion of lung supplied by the obstructed tube. It has also been proved, by the experiments of Mendelssohn and Traube, that when small bodies, such as shot, are introduced into the trachea of an animal, and pushed as far as practicable into the bronchi, collapse of the corresponding portion of lung has been the invariable result. In addition, there are the clinical objections to Laennec's theory first urged by Louis, that bronchial inflammation is more commonly observed in the lower and back parts of the lungs, whilst partial emphysema, on the contrary, is generally found affecting the upper and anterior portions, and that the symptoms of dilated air-cells are not always preceded by catarrh, whilst habitual dyspnœa is not always rendered permanently worse by the occurrence of an acute catarrhal attack.

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[1] Edinb. Monthly Journal of Medical Science, vol. xi. p. 242.

What may be termed the supplementary theory of vesicular emphysema has been advocated, although not to the entire exclusion of others, by Carswell, Williams, Elliotson, Watson, Hasse, and Rokitansky, and has been most carefully worked out and illustrated by Dr. Gairdner, who, moreover, believes it to be of universal application. The frequent occurrence of emphysema side by side with tubercle, first demonstrated by Carswell, seemed to suggest a relationship between them of the nature of cause and effect. The partial or complete obstruction of tubes or cells, preventing the entrance of air into them, has been supposed to necessitate extra pressure on adjoining tubes and cells to which the entrance of air is free, and hence their distension and ultimately permanent dilatation follow. If the enlargement of the thorax during inspiration be disproportionate to the capacity of the air-cells, from deposition in or collapse of some portion of the latter, then, as the lungs are kept in contact with the thoracic parietes by the air expanding their tissue, healthy cells must become unduly expanded to supplement the non-expansion of the obliterated ones. Given diminution of bulk and incapacity of distension in one portion of the lung, another portion is assumed to receive more than its normal complement of air to fill up the space previously occupied by the disabled tissue. This is essentially the inspiratory theory current in the present day. Of course, this reasoning supposes that the enlargement of the capacity of the thorax by inspiration, in such cases, is as great as in the healthy condition. But is this true? Is it not rather the fact that local deposition in or collapse of the lung-tissue may be recognised and measured by the relative immobility of the chest-wall over the affected part? During inspiration the chest expands, to make room for the dilating lungs, and the amount of air inspired seems to limit its expansion. Again, as is observed by Dr. Waters, in inspiration, air is drawn equally to all parts of the lung; there is no power in the muscles of the chest nor in the lungs themselves which can determine currents of air to particular parts. Even if we admit that, after collapse of a portion of lung, the previous quantity of air is still admitted into the chest, so as abnormally to dilate the unobliterated air-cells, we are at a loss to understand how the dilatation should be limited to isolated parts, as we see it in partial emphysema; the air would rather be diffused throughout the whole lung, the entire structure of which would partake in the expansion. Further, Dr. Waters* urges that, in the case of collapse of a part of the lung, if emphysema be its consequence in the manner alleged, the emphysematous portion should be in immediate contiguity to the collapsed. The reverse of this is more frequently the case. Collapse is most frequent in the lower and posterior regions of the lung, whilst emphysema occupies the apices and margins. These reasons are, we think, sufficient to induce us to deny to the supplementary theory universal application.

The expiratory theory has met with an able expositor and supporter in Dr. Jenner. His paper, published in vol. xl. of the 'Transactions of the Royal Medical and Chirurgical Society,' placed the claims of the expiratory force on surer grounds than they had before occupied, by calling in question the truth of the allegations urged against any expiratory theory by Dr. Gairdner. The latter asserts that the expiratory act is mechanically incapable of producing distension of the lung, or any part of it, because it "tends entirely towards emptying the air-vesicles by the uniform pressure of the external parietes of the thorax upon the whole pulmonary surface." He reasons that the air-vesicles, retained at their maximum state of fulness by a closed glottis, could no more suffer further distension by the expiratory power than could a bladder blown up and tied at the neck be still more distended by the application of equable pressure to its external surface. "The air-vesicles can sustain no distending pressure from the column of air within the tubes, as that air only becomes compressed in virtue of a force which opposes exactly as much resistance without as it creates pressure within."[1] To this Dr. Jenner replies, that it is not true that during expiration every part of the lung is equally compressed; nor is it the fact that, during violent expiratory efforts, every part of the thoracic walls is equally unyielding. The phenomena of the act of coughing disprove it:

> "Before coughing, a person makes a complete inspiration—i. e., he distends as far as possible the air-cells; he then closes the glottis, and compresses forcibly the lungs by the thoracic and abdominal parietes; the moment the compression of the lung has attained a certain point, he opens more or less the glottis, and the air is driven forward by the muscular effort and the elasticity and contraction of the lungs and of the thoracic walls with a force proportionate to the compression to which it was subjected before the opening of the glottis. Now it is manifest that if there be parts of the thoracic walls which are more yielding, or which, during powerful expiratory efforts with a closed glottis, contract less than others, that the air, immediately before the opening of the glottis, will be driven from the compressed portions of the lung into the air-vesicles of the lung situated under such parts of the walls with a force proportionate to the general muscular and other powers in play, to the local want of compression, and to the degree of yielding of the walls at those particular spots. That there are such parts, and that they are exactly those which are most frequently the seat of vesicular emphysema, and the sole seat of extreme dilatation of the air-cells, is demonstrable."[2]

Such a part is the apex of the lung above the first rib, covered as it is by soft tissues. Supra-clavicular bulging, from distension of the pulmonary tissue, may be detected during violent expiration, with a more or less closed glottis, by application of the hand and by percussion; and if the apex of the lung be emphysematous, the bulging during violent expiration becomes extreme, and the note elicited by percussion almost tympanitic. The air-cells of the anterior margins of the lungs also, where those organs are covered by the more yielding rib-cartilages, and are protected from pressure to a considerable extent by the heart and great vessels, "the margin of the base of the lung, the part of the lung in the vicinity of the root of the organ below the entrance of the bronchus, and the little ridge of lung which lies behind the trachea on the right side, forming the posterior margin of what may be termed the tracheal groove of the lung," are parts, according to Dr. Jenner, less compressed in coughing and other acts of violent expiration, with partially closed glottis, and they are, therefore, the regions into which air is necessarily driven from the more compressed parts, and hence they become the chosen seats of emphysema. The effect of partial closure of one or more bronchi may, equally with that of partial closure of the glottis, be the over-distension of some of the air-cells lying beyond the tubes, if those cells be not subject to uniform compression during expiratory efforts. Whilst, therefore, complete closure of the bronchi, as Dr. Gairdner has shown, produces collapse of the pulmonary tissue, to which they lead, Dr. Jenner finds in imperfect occlusion a cause of emphysema.

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[1] Monthly Journal of Medical Science, vol. xiii. p. 10.

[2] Jenner: Med.-Chir. Trans., vol. xl. pp. 29, 30.

We see no reason for doubting that an expiratory theory, such as Dr. Jenner's, will account satisfactorily for the production of partial emphysema in its chosen seats, which are exactly those parts of the lung least compressed during expiration, least affected by inspiration, and seated under portions of the thoracic parietes most readily yielding to pressure. We have little doubt that the emphysema which follows hooping-cough and bronchitis may be thus produced. The accident of mediastinal and general emphysema occurring during the powerful expiratory efforts of parturition, and the frequency of emphysema in horses and other animals which are forced to make violent expiratory efforts with a closed glottis in drawing, furnish also confirmatory arguments. Of five broken-winded horses examined by Dr. Jenner, in four the anterior lobes of the lungs only, and that part of the inferior lobe of the right lung which is protected by the trachea, were affected; in the fifth, in addition to these two parts, the margin of the inferior lobe:

> "Now," he adds, "the anterior lobe of the lung in the horse is situated above and over the heart, under the four upper ribs. It is placed, that is to say, in a part of the thorax where it can, during expiration, be subjected to very little external compression. So the part of the right lung, which in all five horses was emphysematous, was protected from compression by the trachea. In the lungs which were the most emphysematous there was no evidence of other disease, so far as I could judge; no trace of recent or of old collapse."[1]

Dr. Waters entirely accords with Dr. Jenner as far as the production of partial vesicular emphysema in the apices and margins of the lung by violent expiratory efforts is concerned, and he quotes the phenomena presented in the case of M. Groux, a man in whom congenital fissure of the sternum existed, and who was examined by many physicians and physiologists in this country some years ago,[2] as illustrating the effects of violent expiratory effort upon portions of lung which are not subjected to the pressure of the thoracic walls. In M. Groux's case, "during a violent expiratory act, the lung of one side came forward in the upper part of the fissure, and formed a distinct elongated resonant tumour, no such result taking place during inspiration." Dr. Waters, however, sees in the direction of the expiratory force a reason for the more frequent occurrence of emphysema in the apices and borders of the lung; and, with regard to the anterior margins, he is disposed to attribute to the direction and strength of the expiratory currents more influence than to the fact that these portions of lung are only compressed by the more yielding rib-cartilages:

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[1] Med.-Chir. Trans., vol. xl. p. 37.

[2] A description of M. Groux's case will be found in the Number of this Review for January, 1861, see p. 273.

> "I have shown above that the apices of the lungs are the parts covered by the least resisting walls; and it will at once occur to all, that the parts which contain the least volume of air are the anterior borders and the margins of each base. These parts are not only the thinnest, but they are also out of the direct line of strongest pressure which the lungs undergo in expiration. Violent expiratory efforts are chiefly made with the abdominal muscles, and the most powerful agents are the recti; the contraction of these muscles, forcing upwards the abdominal viscera and the diaphragm, produces the greatest amount of compression at the base of each lung; the air is consequently driven upwards in a strong current. There being no corresponding force acting at the upper part of the chest, on the apex of the lung, this latter is not emptied; on the contrary, it becomes forcibly distended by the upward current. Further, the strong currents of air from the central and basic portions of the lungs overcome those from the thin portions; and thus these latter, instead of being emptied, become, like the apex, forcibly distended. Dr. Jenner supposes that the cartilaginous portions of the thoracic walls are somewhat yielding, and thus accounts for the production of emphysema along the borders of the lung. This explanation seems to me doubtful, and the one I have given as far more probable." (pp. 47, 48.)

In comparing the expiratory theory of Dr. Jenner with the supplementary theory of Dr. Gairdner, we cannot but think the former preferable as accounting for the production of partial emphysema in connexion with pertussis, bronchitis, phthisis, and other diseases in which violent cough is a prominent symptom. Besides the arguments already adduced against Dr. Gairdner's theory is the fact that in fatal cases of hooping-cough, in which emphysema has occurred, collapse of portions of lung-tissue is not a constant concomitant. But whilst we admit the application of an expiratory theory to account for the production of lobular emphysema, it does not command assent in those cases of the lobar form of the disease arising from some defect in the nutrition of the lung of constitutional origin, and preceded by no acute or chronic pulmonary affection. The only mechanical forces acting in such cases appear to be, as Dr. Waters asserts, those of ordinary respiration. Although the pressure of inspiration is insufficient permanently to dilate the air-vesicles of healthy tissue, this is probably not the case when the elasticity of their walls is seriously diminished or lost. It is excessively difficult to produce artificial emphysema by inflating lung-tissue: healthy lungs may be distended far beyond the point which it is possible expansion can reach during life without any injury to the fibres or basement membrane of their vesicles; but it is not to be supposed that degenerated and weakened organs retain the same distensibility and resiliency. Dr. Waters' supposition seems here a probable one, that the unhealthy and abnormally weak lung-tissue gives way before the pressure of inspiration which it would in health resist, and having yielded is unable, by its diminished elasticity, to recover itself. As increased respiratory efforts are made to dilate the chest, the dilated lung continues to dilate until no further increase in the capacity of the thorax is possible. He states that not unfrequently in these cases of lobar emphysema he has found the whole of both lungs involved, but no appearance of collapse of any portion of the pulmonary tissue—a fact which militates strongly against an exclusive supplementary theory. If Dr. Waters' observations be correct, and if his reasoning be valid, it will be seen that the varieties of pulmonary vesicular emphysema differ both in their pathological and mechanical causation, and that no exclusive theory can be applied in explanation of all the phenomena included under the term.

We have hitherto confined our review solely to the characters and mode of production of the vesicular form of the disease, but rarefaction of the pulmonary tissue proper does not comprise the limits to which it may extend. It is true that in many cases the dense and strong membranous wall which surrounds each lobule is the barrier beyond which the air does not pass; but in most cases where there is extensive vesicular emphysema the air ultimately effects its escape by the rupture of this membrane, and becomes extravasated in the interlobular cellular tissue. Interlobular emphysema, as an independent affection, is very rare; Dr. Waters has never seen it. Lebert regards interlobular emphysema as an early result of the vesicular form. In this Dr. Waters does not concur; he has rarely observed it, except where the vesicular disease has been extensively established. We may believe, however, that it is frequently established in the course of the acute emphysema of childhood which follows pertussis and other lung affections characterized by violent cough. In some cases the extravasated air shows little tendency to spread; in others, and especially in the lobar variety of the disease, the air can be made to traverse the whole, or a greater part, of the lung, passing between the lobules. It becomes infiltrated under the pleura, and occasionally that membrane is raised in large bullæ on the surface of the lung. These swellings may attain the size of a pigeon's egg, or even of a small orange. The air also may pass, by means of the cellular tissue surrounding the bronchial tubes and bloodvessels, into the mediastinum, and thence to the subcutaneous cellular tissue of the head and face, and other parts of the body. In this way is produced the subcutaneous emphysema, which occasionally is noticed as the consequence of violent expiratory efforts in parturition. The general emphysema (*emphysème généralisé*) of childhood is a further stage of the interlobular form of emphysema. M. Guillot, in a paper published in the 'Union Médicale' for 1853, has recounted a number of cases of extensive sub-pleural emphysema in children, the result for the most part of long-continued hooping-cough. In three instances the emphysema extended to the mediastinal cellular tissue, and in four to the sub-cutaneous tissue of the neck, head, trunk, and limbs. Similar cases have been observed by M. Ozanam; and to the 'Archives Générales de Médecine' for 1862, an elaborate paper on the Production and history of Emphysème Généralisé has been contributed by M. Henri Roger. M. Roger has collected 19 cases of this form of disease. All the subjects were young. Of the 19, 5 were less than two years, and 10 between two and four. In every case the emphysema had been preceded by acute and violent affection of the respiratory passages. Eight of the 19 were subjects of intense hooping-cough, and the remainder were cases of double broncho-pneumonia, suffocative catarrh, or rapid phthisis. In every case there had been considerable dyspnœa, and forcible repeated and incessant cough. In all the cases examined after death there was extensive vesicular and interlobular emphysema, extravasation of air in the mediastinal tissue, and more or less extensively in the connective tissue of the exterior of the body. M. Roger thinks that the air may reach the mediastinum either by spreading from a sub-pleural collection near the root of the lung, or by passing from the interlobular cellular tissue to that which surrounds the bloodvessels and bronchi. He is inclined also not to limit the production of emphysema solely to violent efforts with a closed glottis, such as coughing, although he has no doubt of this being a fruitful source of the affection; but he allows also that the violent inspiratory efforts made in hooping-cough, &c., may be sufficient to determine dilatation and rupture of the pulmonary vesicles. Four only of the 19 children recovered, the remainder succumbed to the severity of the original pulmonary affection. In the favourable cases the air was re-absorbed in a period varying from nine to twenty-one days. The comparative frequency of this extreme form of the disease in childhood is accounted for by the character of its diseases, and the less resisting condition of the thoracic walls. According to Huschke, the pulmonary parenchyma in infancy is denser than in age, and would therefore, it may be supposed, offer greater, not less, resistance. But universal emphysema is not limited in its occurrence to the first years of life. Dr. Waters met with it in a young man the subject of phthisis. In this case the emphysema spread upwards to the face, and downwards as far as Poupart's ligament on each side. The post-mortem revealed emphysema of the cellular tissue of the mediastinum, and surrounding the root of one lung.

Before quitting the subject of the production of the disease, it may be observed that the existence of a constitutional variety depending on some degeneration or mal-nutrition of the pulmonary tissue, derives an additional and powerful argument from the observed hereditary character of emphysema. Jackson found that of 28 persons affected with pulmonary emphysema, 18 were the children of emphysematous parents, and several had brothers and sisters in whom the disease had shown itself; whilst of 50 unaffected persons, only 3 had emphysematous parents. Another argument urged by Dr. Waters is founded on the beneficial effects which in his hands have resulted from a treatment of extensive vesicular emphysema by remedial measures which are known to be of use in diseases attended with degeneration of tissue.

We have left ourselves but little space in which to notice the more practical bearings of the subject. The symptoms and physical signs of emphysema are so well known that we shall not dwell on them here. It is only necessary to say that the first symptom which marks the presence of the lobar form of the disease is frequently only increasing dyspnœa and a general feeling of oppression, or "smothering of the chest," as the patient terms it. This may be developed in patients of young or middle age who have escaped those severe bronchitic attacks which in others lay the foundation of the emphysematous affection. The frequency of the development of asthmatic symptoms in the course of extensive emphysema has led many writers to confound the two diseases. Their intrinsic difference is, however, fully recognised by the English school of physicians. The pamphlet of Dr. C. J. Berger, 'On Asthma,' which has lately appeared in France, is devoted to the establishment of the position that asthma is a nervous affection independent of any organic disease; and, amongst other, to English readers, unnecessary discussions, he combats the conclusions at which Louis arrived, that as emphysema is commonly present in asthma, the organic lesion is the cause of the latter. His work concludes with a recommendation, in the treatment of the asthmatic, of that system of passive exercise, frictions, &c., to which the French give the name of "*massage.*" The title of the work, 'Guide de l'Asthmatique,' is evidence that it is as much intended for the general as the professional reader. In discussing the physical signs of emphysema, Dr. Waters especially notices the *râle crepitant sec à grosses bulles* which Laennec believed to be peculiar to, and pathognomonic of, the disease. The existence of this peculiar sound has been denied by many writers. Dr. Waters, however, thinks that he has met with it occasionally in emphysematous patients; he differs, however, from Laennec as to its mode of production. Laennec described it as a dry râle produced by the distension of the emphysematous tissue. Dr. Waters, on the contrary, thinks it is produced by fluid in the finer air-passages. If this be the case, we cannot agree with him that, "as a sign of emphysema, the râle is important, and may assist in confirming a diagnosis." It resolves itself into a mere variety of sub-crepitant râle. These hair-splitting distinctions only tend to throw discredit on the art of physical diagnosis. Dr. Waters' chapters on the Sequelæ, Complications, and Treatment of Emphysema are sensible and practical. The treatment he recommends for the lobar form arising from mal-nutrition is essentially tonic, and of all remedies the preparations of iron have proved in his hands of most avail. He says he was led to employ iron from a knowledge of its value in those diseases which are dependent on mal-nutrition and cachexia, especially Bright's disease and fatty degeneration of the heart. A treatment commenced on theory is too frequently found to fail when put to the practical test; but this was not the case in the present instance. Dr. Waters asserts that he has employed iron in a large number of cases, and has found its administration of so much benefit that he considers it the most valuable medicinal agent we possess for the treatment of emphysema. The ethereal tincture of the acetate seems to be his favourite preparation, especially when the case is complicated with bronchitis. In the treatment of those severe attacks of bronchitis which so frequently jeopardize emphysematous patients, Dr. Waters recommends the stimulating plan of treatment, and he gives some cases which illustrate the benefits to be derived from wine and brandy under conditions of almost hopeless aspect. He has also tried the administration of turpentine in drachm to half-ounce doses in a case where alcoholic stimulants have failed in rallying the patient from a state of semi-asphyxia, produced by the supervention of acute bronchitis on emphysema. The effect of the turpentine seemed to be that of increasing the power of expectoration, relieving dyspnœa, and diminishing effusion into the bronchial tubes. The patient recovered. The few remarks on the treatment of the chronic forms of bronchitis, and of the other complications of emphysema—asthma, cardiac disease, dropsy, &c.—with which Dr. Waters concludes his essay, accord with his views of the pathology of the disease. To combat the conditions of anæmia and atrophy by a tonic *regimen* and medicines, and to subdue intercurrent attacks of bronchitis by counter-irritation and expectorants, are the main features of the plan recommended.

In conclusion, we would add that we think Dr. Waters, in the work we have noticed, has fully maintained the high position as a careful and diligent investigator, in which his work 'On the Anatomy of the Healthy Lung' originally placed him.

REVIEW II.

A System of Surgery, Theoretical and Practical; in Treatises by various Authors. Edited by T. HOLMES, M.A. Cantab., Assistant-Surgeon to St. George's Hospital, and Surgeon to the Hospital for Sick Children. In Four Volumes. Vol. III.—*London*, 1862. pp. 916.

THE third volume of Mr. Holmes's 'System of Surgery' has been for more than a twelvemonth before the profession. We owe an ample apology to our readers for having so long delayed calling their attention to its contents. The volume contains essays on subjects of operative surgery, and on the diseases and injuries of the organs of special sense—of the air-passages, as far as they can be thought surgical in their nature, and of the organs of locomotion and innervation. The last essay in the present volume, upon the diseases of the tongue, is the commencement of the series relating to the diseases and injuries of the organs of digestion.

The first essay in this volume is by Mr. Thomas Smith, upon subjects of *Minor Surgery*. Many of the subjects usually considered to be matters of minor surgery are treated in other parts of the present work, under the special diseases or particular localities with which they are more immediately connected. The writer has aimed only at giving a brief description of such proceedings as are of general application in the treatment of many and various diseases.

Of the many implements—unimposing in appearance—which the surgeon has at hand in his consulting-room, or carries with him in his pocket in going his rounds, few are of more importance, from the frequency with which they are brought into use, than the ordinary calico roller—"roller," we presume, in the surgeon's hand, the "bandage" when applied upon the person of the patient. Elastic materials are less generally useful as "binders;" they fail in one of the chief purposes of a bandage—that of making sufficient pressure for the purpose of steadying the muscles, or, as Mr. Smith describes it, that of "maintaining uniform pressure on subjacent parts." A sufficient amount of firmness is needed in the cloth from which the bandage is made, provided the texture is not too close to hinder the use of pin and needle to fix it when applied.

In these days of labour-sparing machinery, we have often wondered to see in our hospitals that the rollers, which are dispensed with no sparing hand in the wards and out-patient rooms, continue to be made by *manually* tearing the cloth into strips, and rolling these strips one by one into shape. A machine of very simple construction would do all the work. In Madrid we have understood that such a machine has long been in use.[1]

The spiral bandage, taking his example from its use upon the trunk as well as the extremities, receives due notice at Mr. Smith's hand, as being by far the most general in its application. The rules given in most manuals of surgery for the application of it—upon the leg, for instance—are few and simple, and, with the aid of an illustrative engraving, are intelligible enough to the student. Yet they seem made chiefly for model legs, or for patients who can be kept for a considerable part of their day in the recumbent position, or under a limited allowance of bodily exercise, for the purpose of clinical demonstration. The best description we remember to have seen is that given in Mr. Hunt's 'Guide to the Treatment of Diseases of the Skin.' There is a rough and ready way of doing the job, not very easily reduced into a systematic treatise. The nurse often manages to make the bandage hold on for a longer time than her master. One of the best practical lessons on the subject we remember to have received in our early practice was from a gouty patient—a large, powerful man, with a lower extremity of proportionate size. Neglecting the orthodox rule to reverse the roller, folding it upon itself in making each time the circumference of the limb, where it increases in size, he had learned on his own person, when the turn became necessary above the ankle, to carry the head of the roller by one spiral turn above the muscles of the calf, and to bring it spirally downwards from that part toward the ankle. From this reversal of our accustomed practice, he was not conscious of having found any inconvenience from the distension of the veins. The practice may be adopted where we are able to allow the patient his usual exercise.

To give the requisite amount of support above the knee—in the thigh, for instance, as in the trunk—a towel, which is always at hand in a sick-room, forms a very efficient bandage. It is easily applied, and with a few stitches from a needle and thread, the necessary variety of pressure can be always obtained. In the hypogastric region, the most urgent call for pressure is after parturition, for the purpose of securing contraction of the uterus, when it is inclined to dilate with internal hæmorrhage. The spiral bandage, in the form of a double figure of eight, passing round the groins, is here most useful in keeping the bandage and compress in place, and restraining the partially-contracted uterus within its proper pelvic resting-place.

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[1] Some of our hospitals possess machines by which the strips of cloth, after they have been made by hand, are wound tightly into rollers.

However skilfully applied, all these bandages serve only a temporary purpose. The "immovable apparatus," as we call the apparatus for restraining the contained parts from motion, which is required for chronic cases, should answer its purpose without needing frequent application or readjusting, and should not offer any obstacle to the surgeon's examination, if he wish to make one. The ease with which the surgeon can remove the apparatus, or can open it in part, is a circumstance requiring consideration in the choice of material, quite as much as the ease with which he can apply it in the first instance, or the efficiency of its purpose to keep the confined parts at rest and immovable. The materials mentioned by writers are as various as are the diseases or lesions to which they are applicable. A practitioner in the country, of extensive experience, tells us that it has long been his practice, when summoned to a simple case of fracture of the leg, to desire some paste to be made, and to ask for an old newspaper or two. With these materials he makes a very efficient "immovable apparatus," and he finds the patient's progress in every way satisfactory under the treatment.

Among the different subjects of Mr. Smith's essay, vaccination receives a share of his attention. It is entitled to the prominent place it holds in professional estimation from the increased importance now given to the operation in the educational curriculum of the examining boards. It seems to be one of the numerous operations which each practitioner performs most successfully in his own way. The attempts which have been made to prescribe a "normal" mode of performing the operation, and of preserving the lymph, do not appear more likely to be attended with uniformity of practice than attempts at one line of practice in the greater operation of amputation.

Mr. Lister, of Glasgow, contributes an essay upon the operation of *Amputation*, in which he traces with careful industry the history of the operation and the different improvements—the backward and forward journeys—in this important department of surgery. "While the human frame remains liable to derangement from accident and disease, the removal of hopelessly disordered parts, in the way most conducive to the safety and future comfort of the sufferer, will ever be a noble object of surgical effort." We observe that Mr. Lister's acceptance of the word amputation does not apply to the removal of *all* parts. Thus amputation of the breast, the testis, and other parts, escapes notice in this volume.

What always kept the old surgeons back in this operation was, the fear of hæmorrhage. It was not the want of the tourniquet only; for at an early period some kind of bandage was used to check the flow of blood during the operation. It was the want of the modern ligature, or other means to seal effectually the open mouths of the divided vessels, after the part had been removed.

The history of the tourniquet, the invention of it, the improvements and the disuse of it, are matters as curious as the history of the operation of amputation itself. Louis, it seems, was the first operator who, after the general adoption of the tourniquet, avoided the use of it in muscular parts (in the thigh, for instance), and preferred compression made by the finger of an assistant at a point nearer to the trunk, where the flow of blood could be effectually checked, without (as he thought) hindering the contraction of the divided muscles. Such a mode of controlling the hæmorrhage has been thought to have the additional recommendation that it does not so much interfere with the return of the blood by the veins. The late Mr. Liston, to whom we are indebted for many improvements in practical surgery, was one of the most persistent advocates of this practice of *digital compression*. Before his death, we have been informed—but after the publication of the latest edition of his writings—he abandoned this practice, and adopted the tourniquet. The objections made to the use of the tourniquet apply, we think, with less force to the instrument itself, than to the mode in which it is adjusted at the time of operation—often, it may be observed, with an evident want of appreciation of the elementary principles of mechanics. If the strap is properly passed through the lower platform, so as to tighten easily by turning the screw, and if the pad is placed so as to make due counter-pressure against the screw, it will seldom be found that any considerable amount of blood is lost, or that the pressure required to hinder that from happening has interfered, to any appreciable extent, with the free retraction of the divided muscles.

When it became a matter of certainty that the surgeon could control the hæmorrhage during the performance of his operation, and that he could secure the patient against the recurrence of the bleeding from the divided blood-vessels, the operation itself lost part of its terrors. It was an easy matter to cut the soft parts of a limb with a knife, and to divide the bone with a saw; but something more was required to secure one of the two great objects proposed in amputation—the usefulness of the stump. The earliest great improvement in the operation was that suggested and afterwards practised by Cheselden, of so making the necessary incisions that the soft parts, especially the integument, should be of a greater length than the bone. This was effected in the circular method by the "double incision," the skin and fat being turned back before the muscular substance was divided.

Of the different modern modes of performing the operation (in all of which this important object is attained), Mr. Lister, following the teaching of the Scotch schools (if he will allow us to use such an expression), gives the preference decidedly—too decidedly, we think—to that by flaps. His directions for the performance of the particular amputations in the extremities are in every case applicable to that mode exclusively. "Its great merit," he says, "is its facility and speed; for the flaps are cut with great rapidity, and when they are drawn up by the assistant, the bone is exposed with the utmost readiness at the part where it is desirable to divide it." The practical objections, however, that have been urged against this mode of performing the operation are clearly stated by Mr. Lister; and he tells us that Mr. Liston, "the most strenuous" advocate of the flap operation, in the latter period of his practice changed that method, and adopted a mode by which "the skin and fat are divided by two crescentic incisions, with the convexity downwards, so as to form short antero-posterior flaps of the integument, which is raised from the fascia considerably higher than their angle of union; after which the operation is completed as in the ordinary circular method. This modification of the circular operation," he adds, "was also suggested independently by Mr. Syme, and has been used by him for several years past." It is only a just tribute to the memory of an excellent practical surgeon that we should inform our readers that this mode of operating was commonly practised between forty and fifty years ago in St. Bartholomew's Hospital by the late Mr. Vincent.

The dressing of the stump—"a matter quite as important to the successful issue of amputation as the manner in which the limb is removed"—receives most careful attention from Mr. Lister. The principles to be ever borne in mind are, that when the operation has been properly performed, so that the soft parts may meet over the bone without any tension, the primary dressings should retain the parts in apposition, without opposing any obstacle to the escape of discharge. This being done under the eye of a competent operator, we can hardly say we know better directions for the subsequent dressing than we once heard given concisely by a hospital surgeon to a young practitioner, whom he left in charge of a patient at a distance:—"Keep the edges of the wound together with strapping, and don't give a purgative." "What of the ligatures?" said he. "Leave them alone; they will come away with the discharge."

Some useful observations of a general nature are added by Mr. Lister upon the instruments necessary for the performance of an amputation, and the mode of using them. These, we think, must depend much on the peculiarities of hand of the individual operator, and its special adaptation to his eye. With a light hand, with flexible fingers and wrist, with full power in every one of their many joints—while it is certain that "the knife must be held firmly in the hand," as Mr. Lister remarks—the true position of the amputating-knife in the hand of the surgeon is, we should say rather, as the fencer holds the foil.

> "There is another error (Mr. Lister writes) to which the habits of dissection may lead, far more serious than a cramped and awkward use of the knife—viz., that of directing the edge of the instrument towards the skin in raising a flap of integument. Such a practice, necessary in anatomy, to avoid injury to the subcutaneous structures, will, if carried into amputation, most seriously endanger the vitality of the flap, which derives its supply of nourishment from vessels ramifying in the fat, and must perish if those vessels are extensively divided through scoring of the *tela adiposa*. I am satisfied that integument designed to form a covering for the stump is often made to slough, for want of scrupulous attention to this simple principle."

In order to make sure of the vitality of the integument reflected in the circular operation, by retaining its proper supply of nourishment from the vessels in the fat, the late Mr. Stanley, several years ago, proposed to reflect the deep fascia of the limb, retaining its connexion with the skin, before dividing the muscles. It was a tedious dissection, it was an unsightly proceeding; and he abandoned it after making trial of it, as he did not think there was any commensurate benefit.

A short essay on *Anæsthesia* follows, also by Mr. Lister, restricting his subject exclusively to anæsthesia produced by the inhalation of chloroform:

> "The first really valuable suggestion," Mr. Lister remarks, "was made in the year 1800 by Sir Humphry Davy, who having himself experienced relief from pain when breathing nitrous oxide gas, threw out the hint that it might probably be employed with advantage to produce a similar effect in surgical practice."

In the seventh edition of his 'Dictionary of Surgery' (1838, p. 53), Mr. Samuel Cooper refers pointedly to the subject. In the article on 'Amputation,' he mentions "the laudable attempts, made at different periods, to render the patient less sensible of the agony produced by the removal of a limb . . . . The inhalation of stupefying gas has been tried, and so has magnetism." Yet the knowledge of such a fact remained without being reduced to practice, till Dr. Horace Wells proved its correctness in experiments upon himself with ether. Unhappily for his own reputation as a man of science, he abandoned the practice in disgust, under the ridicule directed against him upon the failure of an experiment made publicly before his professional brethren.

In the seventh volume of the 'Medical Facts and Observations' (1797) is a paper by Dr. R. Pearson, recommending the inhalation of sulphuric ether, alone or impregnated with cicuta, in cases of pertussis. He says that "children, and even infants, can be made to inhale this by wetting a handkerchief with ether, and holding it near the nose and mouth . . . . The only unpleasant circumstance," he adds, "attending the inhalation of this ethereal tincture of cicuta is a slight degree of sickness and giddiness, which, however, soon go off."

The mode of administration is here shadowed out, and *some* of the narcotic effects are noted. But the honour of the practical application of the anæsthesia so obtained, to purposes of surgery, remains with the American dentist, Dr. Wells.

Great as are the benefits conferred by chloroform, Mr. Lister regrets, with others, that it is many times either withheld altogether, or given so scantily as to be nearly useless. This arises from the fear inspired by several fatal cases that have happened. The thousands of cases in which it has been given with success, should teach us that it may be used so as to be practically free from any risk. "How, then," asks Mr. Lister, "are the fatal cases to be accounted for?" The causes which have long been recognised by the profession as leading to a fatal termination, or, at all events, accompanying it, and the cases, as related, in which fatal accidents have happened, are severally brought under review, and examined with care, in order to furnish a satisfactory answer to the question Mr. Lister puts.

The conclusion he draws is one in which we think our readers will readily concur:

> "The only rational explanation of this seems to be, that when some great operation is to be performed, plenty of well-qualified assistants are present; and each of them, including the giver of the chloroform, is duly impressed with the importance of his office, and bestows the requisite pains upon it. But when some trifle is to be done, the whole affair is apt to be regarded too lightly; and the administration of the anesthetic is perhaps confided to some unsuitable person, who also allows his attention to be distracted by other matters."

One other cause, we cannot help thinking, has not been made the subject of sufficiently serious investigation. From the recorded cases where death has happened, it does not seem that an "overdose of chloroform" is by any means of uniform occurrence. One circumstance, however, does strike the reader, from the frequency with which it is found in the published reports. In far the greater number of the recorded cases, where the means by which the anesthetic was administered is noted, it was by a napkin, a towel, or some other material *impervious to the atmospheric air*—an instrument, in short, which might be thought to pass under Mr. Lister's general condemnation of "any apparatus which has the effect of preventing the free access of the atmosphere." In the "only death ever witnessed under chloroform," by Mr. Lister, it was given on a "cloth." We are not told whether it happened—as in another case of danger (happily not fatal) where the risk was observed by a bystander—"at a time when the administrator was gazing with interest upon the proceedings of the operator." Whether "watching the breathing carefully," as directed by Mr. Lister, or the state of the circulation only, as directed by Dr. Dyce of Aberdeen and others, are we sure that, in the cases of this unfortunate occurrence, the administrator was watching *the patient?* Are we sure that he was not watching the operation?

The Boston Society for Medical Improvement, in condemning chloroform as being in every way inferior to sulphuric ether, denounce everything in the shape of an inhaler. "Ether should never be given from any inhaling apparatus,"—we are not told why. A towel is recommended instead. Yet fatal cases happen in the United States of America as well as in Europe.

Mr. Coote contributes an essay on *Plastic Surgery,* taking the chief examples for his subject from the operations performed on the face and neck. Without going too much into detail, he has here dealt rather with general principles, applying them to such subjects as are not included under other headings in the present series.

After minute directions for performing the requisite operations, and for attending carefully to every matter of detail in the dressing and after-treatment, Mr. Coote concludes with a caution to "warn the young surgeon that in these *operations de convenance* the sources of mishap are numerous, and disappointments will occur in spite of the endless variety of modifications adopted by different surgeons." Useful diagrams are given of some of the operations, without which, indeed, written descriptions can scarcely be made available as the sole guide for practice in complicated, or rare, cases of surgery.

In discussing the treatment for the contracted cicatrices of burns, Mr. Coote strongly urges the principle of gradual extension, which has been so successfully practised by Mr. Tamplin, as one of the greatest improvements in modern surgery. The almost universal failure of every kind of cutting operation which has been suggested for the permanent cure of these unsightly deformities, has led to their being pretty well discarded as useless in many of the large public hospitals in London. We are inclined to doubt, from the result of our own experience, and that of some friends having good opportunities for observation, whether these operations are necessary so often as is supposed. At all events, it is certain that in those regions of the body where flexion and extension are easily made, and where requisite muscular force is continually in active exercise—as in the groin, the ham, and the elbow—the cicatrix very often *disappears* during the growth of the patient. Young subjects, active healthy children, who, after the healing of large sores, have been left with extensive bands of contraction, and have not received any surgical treatment, will be found some years afterwards, when grown up, without any other remains of these unsightly bands than the scar, which, while it marks the spot where the deformity once existed, does not oppose any obstacle to the perfect freedom of their limbs.

A long and carefully-prepared Essay, upon the Diseases of the *Ear*, is contributed by Mr. James Hinton. The knowledge of the diseases of this organ of special sense—even more than of the diseases of the eye—seems to be but of modern date. Mr. Hinton's essay exhibits, in a remarkably clear and condensed style, an accurate epitome of the present state of our knowledge and experience of these diseases, with their pathology and the appropriate treatment. The study of these diseases has been beset with difficulties, and the practice is unsatisfactory. When the function of the organ is disordered, as the tissue affected is not within the reach of manual examination, and but a small part only (the external) can be brought within the power of the eye, we have to trust for our knowledge of the symptoms to the description which the patient gives, and to form our diagnosis from such imperfect data. A certain amount of imperfection in the sense causes little or no inconvenience, and it is seldom that those who suffer thus apply in early stages for professional help, or care to continue treatment until hearing is complete.

Writing of the inflammation of the tympanum which accompanies the various febrile diseases, especially in children, Mr. Hinton remarks:

> "It is seldom that any indication—at least any that would be observed by a person not on the watch for it—is given of the inflammatory action within the ear; and the first symptom that is noticed is a discharge from the meatus, which occurs, for the most part, only after irreparable injury has been done."

As a means of guarding against the evil consequences of such an oversight, Mr. Hinton urges careful and repeated examination of the membrana tympani with the speculum and lamp. Instead of assuming that inflammation is not present, he thinks it should be a rule with the medical attendants of children during these diseases to prove the absence of this affection of the tympanum.

The diseases of the *Nose*, as one of the organs of special sense, form the subject of an essay by Mr. Ure. The various diseases of the organ and its different textures, the treatment applicable, and the operations required, are considered; much of what is curious and interesting, as well as practical, will be found in the essay. The affections of the special sense of smell—which has often been lost without regret, and for the recovery of which a surgeon is seldom, if ever, consulted—are not included. Belzoni the traveller thought it an advantage that he had not such a sense; and among those whom we have known to have lost it after injury, we do not remember that we ever heard any expression of regret for the loss, or of longing for the recovery.

The surgical diseases of the air-passages are brought under notice in a short article by the late Mr. Henry Gray, upon the diseases of the *Larynx*. Though generally falling under the especial care of the physician, there are many circumstances connected with the mechanical obstructions of the upper part of the breathing apparatus which imperatively demand the attention, and claim the experience and treatment, of the surgeon. These form the subject of the present essay, which, left to a certain extent imperfect at the time of the writer's short and fatal illness, has been revised under the care of the editor of the series.

In connexion with this essay, and as an appendix to it, must be read the few following pages comprising a short essay on the *Laryngoscope*, originally drawn by Mr. Gray, and (like the former one) left in a fragmentary condition by his sudden death. It has been revised by Mr. Durham. A short history is given of the early use of mirrors by different observers, before the introduction of the present efficient and less-complicated instrument. Mr. Durham has given a good description of the different kinds of laryngoscope generally approved, with an excellent description of the mode of using them. In the first attempts to make examination, "numerous difficulties arising from various sources are liable to occur; but in almost all cases they may be overcome by perseverance, and satisfactory results obtained."[1]

Mr. Moore contributes a short essay upon the diseases of the *Absorbent System*—the glands as well as the vessels, which, closely associated in disease, cannot be conveniently or advantageously studied apart from one another. Of the wounds of lymphatic vessels there is but little to be said; they must be injured in every considerable wound, yet we take little notice of the circumstance. It does not seem to interfere with the healing process of the wound of the other tissues. The clear lymph coming from the injured tube is mixed with the flow of blood, or with the subsequent oozing of interstitial fluids, and escapes separate notice. A practitioner of considerable experience, and an original observer, told us that, being in attendance on a gentleman with extensive ecchymosis and discoloration about the foot and ankle after an injury, he made several small punctures with the point of a lancet. At his next visit to the patient, he found a considerable discharge of yellow fluid staining the dressings. This came from one of the punctures on the dorsum of the foot, and continued to flow for several days. He considered that it must be from a wound of one of the lymphatics, which was thus emptied of some of the dark extravasated blood which it had taken up. Whether the distinguishing feature of chylous urine arises from a rupture of a lacteal vessel, and the consequent escape of some of its contents, concurring with a breach of surface in some part of the tract of the urinary organs, as lately suggested by Dr. H. V. Carter, must remain to be settled by further inquiry and pathological examination.

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[1] We hope in our next number to present our readers with a notice of several recent works (chiefly German) which have appeared, upon the use of this instrument.

Mr. Moore gives a clear and succinct account of the course and peculiarities of acute and chronic inflammation of the absorbents, the most numerous and striking examples of which can be taken from the pictures so freely presented by the strumous and unhealthy constitutions of the poor attending the large hospitals in London. The many different phases and pathological changes resulting from particular constitutional tendencies are faithfully described. The local treatment he recommends is, we think, sometimes rather too active for these cases of, confessedly, sluggish progress in weakly constitutions; at least, we have generally found a less meddlesome practice sufficiently successful.

The diseases of *Veins* form the subject of an essay by Mr. Callender. Historically speaking, these diseases seem to have received attention from an early period: they were sufficiently well known by their effects at a time when the circulation of the blood was unknown. When it was not known that the veins, though containing blood, carried it only in one course, toward the heart, it is not a matter of wonder that little real knowledge should exist on the subject, or that what was observed should be explained only by vague theory. Our knowledge of the pathology of these as of other diseases has been much advanced since the time of John Hunter—the "first writer to describe inflammation of the lining membrane of veins." Subsequent investigation has produced increased knowledge; though, from the contradictory opinions and conflicting observations of modern writers, it would seem that the pathology is yet far from being understood.

Adhesive Phlebitis has been variously described by different pathologists, and denied by some. The differing conclusions of modern observers have been drawn from experiments made upon brutes. Mr. Callender has repeated some of these, and, "taken for what they are worth," he considers that they show clearly that those vessels do not inflame when irritated, and that the lymph which is occasionally found within them owes its presence to an effusion external to the vessel. The coagulum of blood within the vein, which has only of late years received the attention it really deserves, has probably been mistaken by some former observers for processes of an inflammatory character.

Mr. Callender's remarks upon this subject are full, and in some respects original; they are well worth studying in his own words. He has traced with care the circumstances depending on this obstruction of veins, and causing it, or predisposing to it; and he has described well the inflammation arising from it.

The formidable disease of diffuse or suppurative phlebitis he considers to be a diffused phlegmonous inflammation, following the course of the veins. The parts around are first inflamed, and not the lining membrane of the veins. "There is no preparation," he says, "of so-called suppurative phlebitis I am acquainted with in which the parts around the vein are not inflamed and thickened."

The tedious and chronic condition of these vessels exhibited in varices has received ample attention from the author; the anatomical changes have been investigated by repeated dissection, and they are well and minutely described. Of the treatment there is little new to be said. From the usual measures adopted, palliative or radical, "it is quite certain that only a temporary benefit is obtained."

The diseases of *Arteries* form the subject of a long and elaborate essay, most carefully prepared, and most completely followed out, by Mr. Moore, Mr. Holmes, and Mr. Ernest Hart.

Mr. Moore has written the earlier part of the essay. This comprises atheroma, and the general subject of obstruction of the arteries, not including aneurysm. The pathology and the effects of this diseased condition are well described.

The remaining part of the essay—concluding the subject of diseases of the organs of circulation—is occupied by the subject of aneurysm, and is contributed by Mr. Holmes—a chapter here and there being the work of Mr. Hart. Though from its etymology the term aneurysm might, perhaps, with propriety be restricted to a general dilatation of an artery, the word has long been accepted among surgeons as defining a tumour containing blood, and communicating with the cavity of an artery. Many and various classifications have been attempted, based chiefly upon varieties in the anatomy of the sac. "The most natural nomenclature appears to me to be," Mr. Holmes remarks (and we think the majority of practical surgeons will agree with him), "that which calls an aneurysm formed by the coats (one or more) of the artery a true aneurysm, and one formed by the surrounding cellular tissue a false aneurysm." It is a matter of great importance to know whether the blood is contained in a sac or not, and whether the aneurysm is of spontaneous origin or the result of injury. The minute anatomy of the sac cannot be known before dissection. Mr. Holmes has attempted to bring the varieties described by former writers into a simpler and more useful classification. The numerous supposed causes, predisposing and exciting, of spontaneous aneurysm are carefully considered. Extended investigation will doubtless supply others; it cannot be thought that we are yet acquainted with all of them.

Of the pathology of the disease, the causes (as far as known), the early symptoms, the progress, the occasionally spontaneous cure, and the different kinds of appropriate treatment, this essay gives a full and most complete report. It will be found a valuable contribution to the literature of surgery.

Instrumental compression, though far from being applicable to every case requiring surgical interference, is, generally speaking, Mr. Holmes thinks, one of the least unsuccessful modes of treatment. It is a heavy drawback, that it needs such a variety of nicely-adjusted apparatus, and the most judicious and unremitting care on the part of the attendants. "It is very desirable, however, to have several instruments at hand, and to vary the point of pressure." Pressure by the finger alone, that of a succeisson of assistants, or even the patient's own, has already, we are assured by Mr. Hart, yielded admirable results. It is a recommendation of such a simple and comparatively painless mode of treatment, that it may be applied to arteries, such as the carotid and facial, which are not easily accessible to any form of instrumental compression. When flexion of the limb suspends the pulsation, such a method will aid the pressure made by other means, and will sometimes, as in popliteal aneurysm, be sufficient to effect a cure. The recently-suggested method of manipulation appears, from the few cases yet published, to be somewhat hazardous; it is ingenious, to say the least of it. In some cases, in the extremities, it has been attended with success. It needs farther trial. "Galvano-puncture appears at present to deserve to rank only as an exceptional expedient." But as a resource, where neither compression nor ligature can be advantageously applied, Mr. Hart thinks "it has a sphere of useful action." Styptic injections have, there can be no doubt, a powerful action; but experience has shown that the dangers of the proceeding are considerable, and that its applicability is limited.

These are some of the means which have been suggested at different times, and put in practice, in the hope of finding a cure for this really formidable, and too often fatal, disease, less hazardous to the patient than the more strictly surgical operation of putting a ligature round the trunk of the vessel—an operation "which is far more fatal than most surgeons appear inclined to admit." They have been themselves proved, one and all, to have been fatal at times. "On the other hand," says Mr. Holmes, in one of the comparisons he has instituted, "subclavian aneurysms are almost always ultimately fatal, and have never yet been successfully treated by the Hunterian ligature." They are not always so. In the early part of 1841, Mr. Skey tied the subclavian artery on the left side, for aneurysm, in a clergyman. The tumour produced complete numbness of the left arm, with great loss of power and coldness of the fingers. The patient's recovery was a little retarded by an attack of phlebitis or "phlegmasia dolens" of the left leg. He is now an active man, taking a leading part in the business of the county in which he resides. We have seen him repeatedly during the last twelve years; he has been, and still continues, in perfectly good health, with the exception that he seems rather feeble on one leg.

Numerically less fatal in the result than the constitutional form of disease, if we may use the term, traumatic aneurysms deserve attention. Mr. Holmes makes some judicious observations upon these embarrassing cases, and strongly urges the expedience in many of them of "leaving the case to the powers of nature, aided, of course, by rest and suitable position," rather than proceeding too hastily, in the absence of alarming symptoms, to cut down upon the parts in search of the injured bloodvessel.

Mr. Holmes has given the surgical anatomy of his subject—the regional surgery of aneurysm, and the operations on the various arteries—with equal clearness and distinctness. He has pointed out well the difficulties in performing the different operations, some of which, indeed, have been found insuperable in the hands of the most dexterous operators, with the aid of the best assistants. Minor difficulties are not overlooked; but the reader is assured that "a little patience and anatomical knowledge will dispose of these difficulties." Mr. Holmes has examined with critical diligence the published works of preceding writers, adopting with approval what is valuable, and giving his reasons when he calls in question their suggestions or their practice.

Mr. Guthrie's remarks, and his directions for the treatment of injured arteries, generally clear, and always forcibly expressed, receive their due share of attention from Mr. Holmes. We will take the liberty of referring more at length to one of these, which we think has rather been misunderstood by writers—we mean ligature of the posterior tibial artery in the upper part of its course. The operation is hardly ever required unless for a wound, or aneurysm the result of an accident. It "can be effected," we are told, "in one of two ways." In the first, an incision is made along the inner edge of the tibia, the fascia and muscles are freely separated from the bone; the deep fascia covering the vessels is then exposed, and divided with proper precaution, to search for the artery under it. This mode of reaching the vessel—which is easily enough done on the dead body—has been condemned by Mr. Guthrie as "difficult, bloody, tedious, and dangerous—dangerous from the chance of failure," in the living subject. It is the older mode proposed for the purpose; and although, as Mr. Guthrie says, "not very easily accomplished," has been many times adopted with success. Dr. R. Harrison, of Dublin,[1] mentions that he was present when the operation was performed, many years ago, by the late Mr. Travers, in St. Thomas's Hospital. We have been assured by another eye-witness, that the operation certainly was, as described by Mr. Guthrie, "difficult, bloody, and tedious;" though not, we have understood, in that instance followed by a fatal result. The other mode of securing the artery is that known as Mr. Guthrie's, and generally said to be by an incision through the muscles of the calf, in the mesial line of the leg.

Upon reference to Mr. Guthrie's published Lectures (1846), it will be seen that he advises the incision to be made in the mesial line of the limb, for an injury or aneurysm, in the middle of the leg, when it

[1] The Surgical Anat. of the Arteries, 1839, p. 405.

is not known which of the two arteries—the posterior tibial or peroneal—is injured, and when it is possible that both may be injured. Such an incision, he remarks, will "enable the operator to place a ligature on either, or on both." In other cases, he says it should be made a little to the side, or "directly over the artery supposed to be wounded." In the cases related by Mr. Arnott,[1] and by Mr. Hussey,[2] of Oxford, it was clear from the circumstances, from the situation, and course of the wound, and the quantity of scarlet blood flowing from it, that the posterior tibial artery was the vessel injured. If we may judge from the reports of these two cases, it does not seem that the operation of cutting through the substance of the muscles was the "simple operation," which Mr. Guthrie, who advised it, supposed it would be found. It is, moreover, an operation we believe he never had occasion to perform.

A young farmer, while occupied in disengaging some part of the machinery in a mill from the steam-engine, which he thought was at rest, received a heavy blow on the calf of the leg. A large and hard swelling formed in the part. About six weeks after the accident, the swelling was punctured with a trocar, and afterwards probed. Ten days after this operation, a sudden and profuse gush of blood took place from the wound, and a very large amount of blood was lost before professional assistance was obtained. In this emergency we were summoned to the patient at a distance. The case seemed to be one of false aneurysm, most probably of the posterior tibial artery. At the desire of those in attendance, who wished that something should be attempted less severe than the amputation of the limb, which we proposed, we cut through the calf of the leg lengthwise, rather to the tibial side of the mesial line, and, as well as we could judge, directly over the course of the posterior tibial vessels. Turning out a mass of coagula, in which we were much helped by the profuse hæmorrhage which occurred, we endeavoured to clear the parts, and trace the artery. The hæmorrhage from the various vessels in this region, independently of that from the proper posterior tibial artery, has been described as most severe by Mr. Arnott and Mr. Hussey. In the present instance it seemed to be uncontrollable. Rather than run more risk from such a cause, in the exhausted state in which the patient lay, we proceeded without further question to amputate the limb below the knee. The patient recovered favourably. A hasty dissection was made of the amputated limb in the twilight of a summer's evening, while waiting for the last train to take us home. The parts were much disorganized, and the different tissues were not easily recognised. There were two large openings in the posterior tibial artery in the upper part of its course, about two inches from each other.

A short essay upon the *Affections of the Muscular System* is contributed by Mr. Tatum. It comprises the most frequent kinds of injury of sufficiently serious nature to be made the subject of surgical inquiry—such as sprains, or ruptures and contusions, and the inflammation often following such injuries, or arising, as it sometimes does, from other causes, simple or specific. The most serious, and often obscure, affections of muscles—such as atrophy, fatty degeneration, and ossification—are well described in their progress and anatomical peculiarities. Some of these cases Mr. Tatum has had the opportunity of watching for many years, without being able to check their progress in any material degree. Some of the affections of tendons are included in the essay, as well as those of the mucous bursæ, as being appendages of the muscular system. Interspersed among the anatomical details will be found some short but excellent practical directions for the treatment of these very troublesome diseases, often found to be serious, especially when situated among the tendons and bursæ about the hand and foot.

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[1] Med.-Chir. Trans., vol. xxix.

[2] Med. Gaz., Jan. 1851.

Dr. Little contributes an essay on *Orthopædic Surgery*, restricting himself chiefly to the consideration of such parts of the surgery of Deformities as are not included in other essays of the series—namely, those which are to be remedied by surgical operation and mechanical treatment. Though Scarpa first directed the attention of surgeons to a better knowledge of this class of affections, the strictly surgical treatment received its chief impulse by the publication of Stromeyer's labours in 1831. Grateful for services rendered to him personally, and enthusiastic in spreading the benefits of the instruction derived from a master he loved, the author of the essay before us—"the Apostle of Tenotomy," as Stromeyer termed him—was the channel through which the knowledge of this new branch of surgery came among us in England. That joints became stiff when the bones were not used was a piece of practical knowledge soon obtained. It was a pathological fact receiving illustration during a fracture or other injury of an extremity, when the limb was kept at rest; and it was remedied by passive motion. Striking examples were furnished by the elbow and the knee, when the limb was kept for any length of time in the bent position; the permanently flexed condition, or "contraction," was not so easily cured by the ordinary attempts to straighten the limb, even when aided by the patient's own attempts at exercise, if at an age to give such aid. The permanent contraction of muscles, accompanied in many cases with a paralytic state of the opponents—"structural shortening and possible degeneration"—was more than could be cured by the incomplete mechanical contrivances of the instrument-maker, to whose care, in default of the surgeon, they were till lately generally consigned.

Certain large classes of deformities, arising from the contraction of muscles, are brought under especial notice in this essay—those arising from injury or inflammatory disease in the joints or neighbouring parts, those which are the result of paralytic affections or spasm, and those which exist at the time of birth. In saying that these last "can be assigned only to a preternaturally excitable or spasmodic condition of the muscular fibres of the shortened muscles," we think Dr. Little seems too much inclined to under-rate the concurrent effect of position in utero, to which Mr. Tamplin and some other writers attribute a much larger influence than Dr. Little allows. It is, at all events, a doctrine in pathology which cannot be taken as being absolutely settled. It must be admitted, however, that Dr. Little has here, as well as in others of his writings, ably supported the opinions he advances on the subject.

The common club-foot (*Talipes varus*) is the deformity which exhibits in the greatest degree the incidents of orthopædic experience. Dr. Little takes this as the example by which to illustrate best the principles on which the management of all other deformities should be conducted. Many of the slighter cases of congenital varus may almost be left to the manipulations of the nurse. The more severe cases need an amount of patience and persevering attention on the part of the surgeon, in conjunction with great skill and ingenuity on the part of the instrument-maker—to say nothing of the co-operation of the patient—which can hardly be appreciated by practitioners who are only seldom called upon to treat a case.

We refer now chiefly to cases where there is a very great increase of the lateral, or transverse, arch of the foot, in which the fifth metatarsal bone becomes approximated to that of the great toe, with a deep furrow between—cases "in which no reasonable effort of the surgeon's hands will temporarily extinguish the contraction and deformity." These cases were, probably, never cured before Stromeyer's time. Among the plates in Sheldrake's Essay on Distortion (1816), there is not one representing this severe degree of deformity. The nearest approach to it,[1] though by no means representing the extreme degree which we now know to be curable, is that of a young lady aged eight years, who passed from his care, and was not permanently cured. Perhaps he did not undertake such cases. It appears to have been his doctrine that "if the cure is not attempted till the bones are completely ossified, it cannot be effected." The teaching of the present day is very different from this. Doubtless, the earlier the age at which the treatment is undertaken, the less difficult is the cure. But we do not find the complete ossification of the bones a very serious difficulty: adult age is far from being an insuperable obstacle to the cure.

Dr. Little retains his preference for two knives in dividing the tendon of the Tibialis posticus muscle—the sharp-pointed one to puncture the sheath of the tendon, and the probe-pointed one to divide the tendon itself. For our own part, we are inclined to prefer the single instrument with two blades lately introduced by Mr. Tamplin. The probe-pointed blade slides easily forwards into the opening made by the pointed blade, and the tendon is felt at once.

The apparatus recommended by Dr. Little for mechanical extension after division of the tendons is not quite that which is now in use in the Orthopædic Hospital. It is a recommendation of the one he advises, that, in the case of adults, it allows of their taking walking exercise with less risk of injury to the instrument, and at the same time favours the mobility of the ankle-joint in the required direction. With reference to the multiplicity of new and complicated apparatus so freely recommended by different writers, Dr. Little quotes with approbation the remark of Stromeyer, that "every practitioner will select that of which he best understands the action and mode of application."

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[1] App., Case XXX., Pl. 8, 1806.

The space at Dr. Little's command has probably restricted him much in his subject. The description he gives of the few deformities comprised in his essay—for it is far too short to include all which come within the province of orthopædic surgery—are clear and concise. The principles of treatment are well laid down, and due caution in following them out with gentleness and perseverance is impressed upon the practitioner. In orthopædic surgery generally, "success," Dr. Little says, "depends more upon the manner in which the mechanical treatment is conducted than upon operative interference."

The essay closes with some judicious remarks upon the gradual extension of joints by mechanical apparatus, and upon the more recent modes proposed of violent extension by force under chloroform.

The diseases of the *Bones* form the subject of an elaborate essay by Mr. Holmes. Simple inflammation, as occurring in bone, is minutely and fully described through the different stages of its progress, in its general symptoms, and its various results, destructive and reproductive. Ostitis, periostitis, osteo-myelitis, chronic abscess (or limited suppuration), caries, and necrosis are all separately considered, and appropriate treatment suggested. Though it has been taught from the time of Hunter that the diseases of the bones are essentially similar in character to those of the soft parts, there are, Mr. Holmes remarks, diversities in rate of progress, and other characters, which require very different treatment, and entail a very different amount of danger. Such diversities, "although they are not really essential, yet cause striking differences to ordinary observation, and have led to the designation of the results of inflammation in bone by names differing from those which are used for the same changes in soft parts."

In many questions of treatment arising during the progress of these diseases, especially questions of operative interference, so much depends on the peculiarities of individual cases, that it cannot be expected, in an essay of this nature, that perfectly clear directions should be given for the guidance of the surgeon in every case he may be called upon to treat. Still, under the different headings, the principles of treatment are carefully expressed, and enough will be found in it to illustrate the points necessary for practice.

From these cases, which may be called cases of local origin, Mr. Holmes proceeds to the consideration of the constitutional affections of bones —strumous, rheumatic, or gouty, and malignant. The scrofulous constitution, which modifies disease in almost every tissue of the body, shows its peculiar and distinctive characters in bone during inflammation. Mr. Holmes's observations upon the pathological anatomy of these affections are in many cases the results of original research.

Mr. Holmes makes a separate class of "syphilitic affections of bone," without specifying more particularly to which of the numerous forms of venereal disease—pustular, papular, scaly, &c.—he restricts the term syphilis. The "chronic limited inflammation of the periosteum alone," which he mentions as peculiar to syphilis, will be recognised by the reader by the character of the nocturnal pains, in the long bones chiefly, which accompany the constitutional symptoms of the scaly form, in regular sequence. These pains being aggravated by exposure to cold and wet are ordinarily attributed by the patient—and sometimes by the practitioner, not always informed of the other attendant and more characteristic symptoms of venereal infection—to rheumatism. The "node," as generally described, we venture to say—though Mr. Holmes does not refer to the distinction—is rarely, if ever, seen in the scaly form of disease, being most frequently seen in connexion with papular or pustular eruptions. Whether a node is ever seen except in persons who have been the subjects of a free exhibition of mercury—we mean to an extent materially disturbing the circulating system—and, if so, in what constitutions, and in company with what other symptoms, is a question we would submit as well worthy of more extended investigation. Certainly nodes may be seen where there can be no suspicion of venereal taint, and, as in the well-known case of Dr. Baillie's pupil, without sexual intercourse. The "ulcerative or gangrenous affections" constitute caries and necrosis, some of the destructive effects of phagodena, and are seen among the constitutional symptoms, irregular in the order of their appearance, sometimes even coexisting with the primary symptoms, in company with the "horse-shoe" sore. It is of these more grave and more obstinate developments of disease, that Mr. Holmes's remarks will be acknowledged as correct:—

> "There seems no reasonable doubt that such affections do also occur in persons who have never taken mercury, especially when the disease has been allowed to go on unchecked, and the constitution is at the same time enfeebled by alternations of debauchery and hardship, as is the case sometimes with sailors, and more frequently with prostitutes."

Malignant diseases—progressive and incurable in the bones as elsewhere—have received greater attention of late years: the pathological anatomy, it may be said, has hardly been investigated till recently. An excellent account is given of them by Mr. Holmes. The same may be said of his history of innocent tumours and other enlargement of bone.

The complicated structure of *Joints* presents various tissues for examination as the subjects of diseased action: these are treated by Mr. Athol Johnson. It is only since the investigation of these diseases has been pursued in relation to their origin in each separate structure that any "precision of diagnosis" has been arrived at, and that any really physiological system of treatment has been adopted. In advanced stages of disease, all the structures become involved, and the distinctive symptoms of disease peculiar to the separate tissues can no longer be recognised. In tracing the history of the present increased knowledge of these affections, Mr. Johnson makes due and honourable mention of the labours of Dr. Haygarth, Sir Benjamin Brodie, Cruveilhier, Dr. R. Adams, and other English and foreign writers who have preceded him.

Almost from the time when the synovial membrane was recognised as a distinct structure, it has been found to be the tissue most prone to disease of all the structures in a joint, while presenting at the same time the most extensive surface for the spread of diseased action. The different forms of inflammation of this membrane, either simple or modified in their character by local causes or constitutional conditions, have received a large share of Mr. Johnson's attention. Though he professes only to point out the "leading types," the peculiarities of these and their pathological anatomy are well described. "In many cases, the distinctive characters of each of these varieties are clear and well defined," observes Mr. Johnson. "In others, however, the boundary-line is not always drawn with precision; and the surgeon finds in these diseases, as in most others, that the definite descriptions necessitated in books are not always borne out in practice." The treatment advised during the period of active disease, which, indeed, must be followed often through a long and protracted period of convalescence, is cautious and good; it is given with minuteness, and is well worthy of the reader's study.

After the consideration of the diseases of the synovial membrane, Mr. Johnson proceeds to those diseases which originate in the articular extremities of the bones—more frequently seen in young children, or in subjects under puberty, than in full-grown people. Although the prospect of a successful issue depends greatly on the treatment being commenced at a very early period, Mr. Johnson, we are happy to see, coincides in opinion with other excellent practical observers of the most advanced schools of "conservative surgery," as it has been called. He says that when disease is most advanced—"when the joint has become implicated, and suppuration has taken place, care and perseverance on the part of the surgeon and patient will usually be rewarded ultimately by success." This was the teaching many years ago of Mr. Abernethy: "However complicated the disease of a joint may be, if the patient's health continue good, we ought not to despair."[1] Though the prognosis in the affluent classes is favourable, it is less so among the poor and needy. Among them, with the many drawbacks of poverty, ignorance, and inattention, the time required for recovery becomes considerably prolonged; we will not go so far as to say that the chance of it is hopeless. We are unwilling to believe that the people of this country, of the agricultural counties in particular, are become such an unhealthy race that they are no longer able to recover from strumous disease, when it affects a joint, except at the expense of an eradicating operation. Where the confidence of the patient and his natural guardians is gained, as it may be by gentleness, by kindness and attention, combined with skill and professional knowledge, the possession of which the poor and needy are as well able to discern as the rich, the best means of ensuring observance of the surgeon's directions are gained, and his services will not often be thrown away. It is seldom, very seldom, we are inclined to think, that in young subjects the treatment, under the personal superintendence of a competent surgeon, will fail of success, if he does not himself fail in his efforts. Yet we cannot deny the truth of some remarks we once heard Mr. Skey make: "He knew of no disease that required more patience, more knowledge, more perseverance, more anxious watching day by day, before the end was accomplished, than joint disease." Mr. Johnson adds that, "in early life, the limb may generally be preserved." Mr. Hilton, in his 'Lectures' lately delivered at the College of Surgeons, told the assembled members of the College that they would be justified in believing something more than this as the result of treatment. Mr. Tamplin has said that, "out of the large number of cases of diseased joint in children, which had occurred at the Orthopædic Hospital, he had not seen one that failed to be cured by the most ordinary care and attention."

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[1] Lectures on Surgery, 1830, p. 179.

It is wonderful how little inconvenience the patient sometimes suffers during the long period through which the disease is in progress. We have seen a lad with bony ankylosis of the hip-joint, the result of disease, which had been about two years in progress, without having had any professional treatment, and without rest, except for a few weeks of the time after a slight injury. During the greater part of the period he had been employed on a farm looking after cattle. A lad was lately referred to us by a professional friend, to whom he had that day applied on account of a pain in the shoulder. Three years before, he had received some sharp blows about the shoulder, under punishment from his schoolmaster. Pain and stiffness followed, which received but little attention from his parents, and none from anybody else. We found the head of the humerus firmly united by bony union to the scapula, without any sign of existing disease.

On the subject of the painful startings, which so frequently harass the patient, and deprive him of sleep, when the position of the limb allows the action of the muscles of flexion and extension, where irregular pressure on the articular surfaces is maintained by the muscles in a state of spasmodic contraction, Mr. Johnson says, that "Dr. Bauer has proposed to divide the tendons of the contracting muscles, independent of any malposition, as a means of relieving the starting pains." This operation was proposed, for the same reason, about forty-five years ago, to the late Mr. Vincent, at St. Bartholomew's Hospital, by the house-surgeon of that day, now long retired from the practice of the profession, who with anxious but unavailing solicitude had watched the patients with ulceration of the cartilage in diseases of the knee-joint. Worn out with suffering, without other known means of relief, they gladly submitted to amputation. The division of the hamstring tendons could not, he said, put them in a worse position.

Mr. Johnson gives some useful directions upon the subject of passive motion during the later stages of inflammation of the larger and more important joints, with the view of avoiding the stiffness or false ankylosis, which often follows severe inflammation or injuries. The importance of the position of the limb is pointed out. Mr. Johnson says, "the position assumed by the limb is practically of great importance, for it is capable of maintaining or aggravating the disease at the time, as well as of giving rise ultimately to great embarrassment in the event of ankylosis occurring." Where bony ankylosis becomes inevitable, he points out the urgent necessity for the surgeon's rectifying the position assumed by the limb; "for recovery by ankylosis to be desirable, the limb must be fixed in a proper position."

Much discussion used to be held formerly upon questions of "proper position" in cases of ankylosis. Mr. Johnson has not entered much into these, beyond directing that the limb should be placed in the position in which it may be employed to the greatest advantage. In the elbow, it used to be said that the proper position was at about a right angle—at least, the test was that the patient should be able to put his hand to his mouth.

Some years ago, a friend told us he had under his care a gentleman of education, a lawyer by profession, who had the misfortune to suffer from ankylosis of long standing in the left elbow-joint, the limit being extended much beyond a right angle. Pitying his helpless state,—unable to put his hand to his mouth,—our friend inquired into the amount of disability which he suffered from the faulty position (as it was thought) of the limb. The patient, long practised in the use of the limb, gave his opinion that, if the joint was fixed, his own was at the proper angle. He enumerated, with the accustomed accuracy of his profession, the advantages he enjoyed (our friend begged him to put down the list in writing): he could do this, that, and the other—he could drive a coach and four—he could tie his neckcloth (he showed how he did it). Many other things were within the compass of his power; "and," added he with a smile, taking our friend by the collar, "I can give a fellow a good licking." Not long since, a lad passed under our observation with his right elbow ankylosed at an obtuse angle. When a boy, he was well able to clean knives and shoes: he is now employed as teacher in a village school.

Next after the essays upon the diseases follows one by Mr. Holmes, upon *Excision of Bones and Joints*. This comprises an inquiry into the indications for such an operation, as well as descriptions of the operations, and the parts, whether bones or joints, accessible to such treatment. The cases suitable for excision are pointed out, and the rules given for the operations for removal of bones, or for the excision of their articular extremities, and for the after-treatment of the cases, are remarkably good; they show an abundant amount of practical knowledge of the subject, and an intimate acquaintance with every matter of detail. Mr. Holmes is not to be classed as one of the most strenuous advocates for the modern operation of excision of joints. He has examined the subject carefully, and without any undue bias in his own mind, often calling in question the statistics which have been publicly brought forward to support the propriety of the operation, and the frequent necessity for it, as well as the comparative superiority of it over other kinds of operative interference.

The conclusions deduced from a number of cases, even numerically successful, would be more satisfactory if, instead of instituting comparison between the results of two kinds of severe operations (amputation and excision), the comparison was made between the cases which are the subjects of these operations on the one hand, and, on the other hand, those which are treated by position and constitutional treatment. The cases in which this last system of treatment fails, are those only in which either of the two operations in question can be justifiable. "It may be laid down as a general rule," observes Mr. Holmes, "that a large or important joint ought not to be excised while any reasonable prospect exists of a cure without operation." How often is it that such a prospect fails, and what are the circumstances, when it fails, which encourage the hope of restoration to health, or usefulness of limb, from excision of a joint? These are questions, we submit, well deserving farther and more extended investigation than they have yet received.

When the question of excision of the knee-joint was before the Medico-Chirurgical Society, between five and six years ago, Mr. Skey said, "it was a striking fact that these remarkable cases of excision came from comparatively few quarters, and were not spread over the whole of the profession: they were confined to a few persons, with whom the practice appeared to be the rule rather than the exception." It would seem that while one practitioner cures by "rest," another cures by excision, and a third finds a cure impossible,—or perhaps only improbable—and removes the disease altogether by amputation of a limb. Mr. Holmes's views, as he here puts them forth, that many cases may be cured, and that some, which he points out, must be submitted to excision, will lead the way to sounder and more justifiable conclusions.

Mr. Shaw's contribution to the present volume consists of a single essay, upon the diseases of the *Spine*. This should be read in connexion with that upon injuries of the same region in the former volume. The essay in this volume is confined to disease of the bodies of the vertebræ—the "Pott's disease" of former writers—bringing under notice the symptoms of incipient disease, the diagnosis from neuralgia or hysteric affections, the deformity of real disease, as opposed to mere weakness of ligaments, the formation of abscess, and the treatment to be followed. Upon all of these, Mr. Shaw's essay is full and discriminating. Although not one of the order of "specialists," Mr. Shaw's acquaintance with all matters bearing on the diseases of this region is evidently extensive and practical, and his teaching is instructive.

A short paper, by Dr. Brown-Séquard, upon the diseases of the *Nerves*, closes the series of diseases of the organs of locomotion and innervation. The writer describes the effects produced by irritation of the centripetal nerves, and the affections which may be caused by injury or by disease; and he gives some rules for diagnosis and treatment of diseases and injury of branches and trunks of nerves. Much, undoubtedly, of the physiology of the nervous system still remains unexplained. In the treatment of the diseases of the nervous system, it does not seem that our knowledge advances at the same rate as that of other parts of the system. Dr. Brown-Séquard remarks that "the rules for general treatment vary according to the kind of reflex affection to be treated."

The last essay in the present volume is a short paper by Mr. Coote, upon diseases of the *Tongue*, being the first of the series of Essays on diseases of the organs of digestion, which are to form part of the forthcoming volume. This essay comprises much practical matter within a small space. The description of the diseases, many of which are highly important, though it cannot be said that they are very numerous, are clear; and the treatment to be followed, operative or otherwise, is pointed out.

REVIEW III.

1. *Die vorzeitigen Athembewegungen: ein Beitrag zur Lehre von den Einwirkungen des Geburtsactes auf die Frucht.* Von Dr. HERMANN SCHWARTZ.—*Leipzig*, 1858. pp. 308.

On Premature Respiratory Movements: a Contribution to the Theory of the Action of the Process of Birth upon the Fœtus. By Dr. HERMANN SCHWARTZ.

2. *Ueber das Athmen der Kinder vor der Geburt in physiologischer, anatomischer, und forensischer Beziehung.* Von Dr. MAX BÖHR. ('Henke's Zeitschrift für die Staatsarzneikunde,' 1863. Erstes Heft. pp. 1—107.)

On the Respiration of Children before Birth in relation to Physiology, Anatomy, and Forensic Medicine. By Dr. MAX BÖHR.

3. *Beiträge zur Lehre von der Todesart der Kinder während der Geburt, mit bezug auf die Theorie von der Placentar-respiration.* Von Dr. C. HECKER. ('Verhandlungen der Gesellschaft für geburtshülfe.')—*Berlin*, 1853. pp. 145—196.

Contributions to the Theory of the Cause of Death of Infants during Birth, with reference to the Doctrine of Placental Respiration. By Dr. C. HECKER.

4. *Lehrbuch der Geburtshülfe.* Von Dr. A. F. HOHL.—*Leipzig*, 1855. pp. 1139.

Text-book of Midwifery. By Dr. A. F. HOHL.

5. *Zur Lehre vom Vorfall und von der Umschlingung der Nabelschnur unter der Geburt.* Von Dr. L. KRAHMER. ('Deutsche Klinik,' 1852, p. 289, *et seq.*)

On the Theory of the Effects produced by Prolapsus and by Loops of the Funis during Birth. By Dr. L. KRAHMER.

THE theory that the placenta is, during embryonic life, a respiratory organ—that the fœtal villi absorb not only nutritious matter, but also oxygen, from the maternal blood, by which they are bathed, has received among physiologists and obstetricians, in this country at any rate, a very general acceptance. But this agreement is by no means universal; there are some who, with Scanzoni and Kiwisch, refuse to admit the aëration of the fœtal blood; and there have been others who, granting this fundamental point, have attributed to other parts than the placenta the functions which are exercised by the lungs during extra-uterine life. Among the text-books of physiology in ordinary use in England, we may mention that of Kirkes,[1] in which the respiratory function of the placenta is—at least, by implication—denied. Moreover, those who admit it generally refrain from giving any reasons for the views which they hold; and it must be conceded that those arguments upon the subject which are to be found in 'Müller's Physiology' and elsewhere, are not very conclusive, being chiefly of a presumptive character. Of late, however, attention has been drawn to certain effects of interruption of the placental circulation, which had previously been only partially observed; and the result of these investigations has been to lend powerful aid to the theory that the placenta is the seat of an interchange of gases, essentially equivalent to respiration. It had long been known that, under certain circumstances, respiratory efforts are made by the fœtuses of animals, even while enclosed in the membranes, and that, as a result of these movements, liquor amnii and other fluids may find their way into the trachea and bronchi of the embryo; but now it is shown that such respiratory efforts are the immediate and necessary consequence of obstruction to the circulation in the placenta, and that when no fluid can enter the air-tubes, their occurrence may be recognised by another class of effects, of an equally characteristic kind. In this case blood is drawn into the pulmonary vessels; and if the obstruction continue, it exudes through the thin walls of the capillaries into the lung-tissue, forming ecchymoses, and the space created by the expansion of the thoracic parietes is in this way filled up.

Now, if these facts be correct, it seems impossible to deny the respiratory function of the placenta. These movements can be related to the cause which is found to produce them only as the result of a "*bésoin de respirer.*" This must be caused by the interruption of a process of aëration which had before been actively going on; and the conditions of the experiment are sufficient to show that this function was located in the placenta. But their physiological interest is not the only one which attaches to these observations. They have a wide bearing also in both obstetrical and medico-legal practice, and therefore we propose to bring before our readers the chief points which have been hitherto ascertained; and we have thought it desirable to preface our remarks by a brief *résumé* of the evidence in favour of the respiratory function of the placenta, which existed before these recent additions to it were made.

It may be remarked, in the first place, that no clear idea of placental respiration was possible till it was shown, about twenty years since, by Magnus and others, that gases are dissolved in the circulating fluid, and that the formation of carbonic acid occurs, not in the lungs, as had previously been supposed, but in the tissues generally. Till this was discovered, it was not known that the blood in the uterine arteries contains the oxygen, by which it is at least possible that the aëration of the fœtal blood may be effected.

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[1] Handbook of Physiology, fifth edition, p. 275.

It might, perhaps, be expected that, if this change occurs in the placenta, there should also be an alteration in the tint of the blood, which would be perceptible when the contents of the umbilical arteries and the umbilical veins were compared. The results obtained by experimenters upon this point have not been quite uniform. Jörg[1] states that he has seen a difference of colour in the vessels of the chorion of the horse. Herholdt[2] says that he found the blood in the umbilical vein redder than that in the umbilical arteries; and the same fact was observed also by Baudelocque,[3] Carus, Hérissaut, Diest, and others, though only in a slight degree.

On the other hand, the most eminent physiologists—and among them Haller, Hunter, Osiander, Scheel, Magendie, Bichât, and E. H. Weber—have altogether failed to detect any difference. On opening the abdomen of a pregnant sheep during life, J. Müller[4] thought that the tint of the blood in the umbilical artery and vein was not quite the same, and he says that others who were present agreed with him. He also observed that the blood in the umbilical vein coagulated more slowly than that derived from the artery; but he himself ascribes this to their not being collected at the same time. He does not state that he ever repeated this experiment on the living sheep; but in the cat, rabbit, and Guinea-pig, he uniformly failed to detect any difference in colour; and many observations made upon the uterus containing the embryos of sheep recently killed gave always the same negative result. Müller, therefore, appears to have quite made up his mind that no evidence of the respiratory function of the placenta is to be obtained from this source.[5] Schwartz[6] also arrived at the same conclusion from his experiments. Equally unsuccessful was the attempt which is stated to have been made by Müller to extract different gases from the two kinds of blood by chemical analysis. Schwartz has noticed, on cutting through the still pulsating funis of infants born in a state of asphyxia, that the blood in the umbilical arteries was dark in proportion to the degree of that condition; and in some instances was almost black. He made no attempt to ascertain the nature and quantity of the gases which the blood contained in these cases, being always absorbed in endeavouring to save the infant's life; but he thinks that the presence of carbonic acid would probably have been detected without difficulty.

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[1] Die Zeugung. Leipz., 1815, p. 273.

[2] Comment. über das Leben, &c., 1803, p. 64.

[3] Müller's Physiology, p. 317. (English edition, translated by Dr. Baly.)

[4] Loc. cit., p. 317.

[5] De Respiratione Fœtus Comment. Physiol. Lipsiæ, 1823, p. 163.

[6] Op. cit., p. 50.

The cause of this want of success, both in detecting a difference in colour in the blood coming to and going from the placenta, and in obtaining gases from those fluids, is not difficult to find. The quantity of oxygen contained in the fœtal blood is probably small, as suggested by Oken and Carus, and this would of course render its detection difficult. But the truth lies apparently in the remark of Schwartz, that perfectly normal fœtal blood cannot be obtained, and was never seen by any of the observers to whose investigations we have referred. The modern theory of placental respiration derives its chief support from the effects of its interruption; and these effects follow their cause so rapidly, that the condition of the blood is probably quite altered before observations on it can be made. All young animals taken from the uterus of their parent are already in a state of asphyxia, and therefore it is unlikely that we shall ever be able to obtain different gases by analysis of the blood contained in their umbilical arteries and veins, however delicate our processes of investigation may become.

In the absence, then, of direct evidence of a respiratory function of the placenta, we must fall back upon the presumptive and indirect. The provision that is made for aëration in the eggs of oviparous animals has been adduced with this view. Schwann[1] found by his experiments that when the eggs of birds are kept at the proper temperature in gases containing no oxygen, no embryos are produced, and the enlargement of the germinal membrane, its division into serous and mucous layers, and the development of the area pellucida, are the only formative changes which take place. It may, indeed, be answered to this argument, that there is a great difference between the assimilation of the albuminous substances stored up in the egg, which form the nutriment of the embryo of the fowl, and the direct absorption of materials which may have been recently oxygenized from the blood of a mammalian parent. The analogy is, however, probably worth something, and its value is increased by the remark of Müller, that the distinction between the oviparous and viviparous vertebrata cannot be an essential one, since, as is well known, genera are found in the same order among serpents and lizards, which differ in this respect.

Another argument for the necessity of some aërating process for the fœtal blood is drawn from the fact that products of oxidation are to be found in the embryo, which can hardly have been absorbed from the mother. Thus gelatin is easily obtained from the fœtal bones, but it cannot be detected in the maternal blood, and therefore it is at least probable that it is formed in the fœtus. Again, it is well known that urea and uric acid are contained in the fœtal urine, and that agglomerations of uric acid and urates may even block up the uriniferous tubules of the fœtal kidney. Now, there is little doubt that, under certain circumstances, urea may pass from the maternal to the fœtal blood. Braun[2] detected in the blood of a stillborn child, whose

[1] Müller's Archiv, 1835, p. 21.

[2] Chiari: Braun: Spaeth. Klin. d. Geburtshülfe, &c. Wien, 1855, p. 318.

mother suffered from uræmia, a large quantity of urea; and Hecker[1] found the same substance in the pleural fluid of an unripe child, whose mother was affected with some disease of the kidneys. But it is probable that this is quite exceptional. Schwartz[2] has examined the blood and the fluids contained in the pericardial and peritoneal cavities of stillborn children in ordinary cases, without being able to detect a trace of urea or uric acid. Now, if these substances are formed within the fœtal body, a supply of oxygen is necessary, and therefore something equivalent to respiration must occur.

The experiments of Von Bärensprung on the temperature of the embryos of animals are also adduced in favour of the same theory. Autenrieth[3] and Schütz measured the temperatures of the embryos of rabbits immediately after their removal from their mother, and found it only 93° Fah., while that of the mother was 99½° Fah.; they therefore concluded that the embryo has no independent power of generating heat. Others have drawn the same inference from the rapid fall of temperature in infants after birth; but Von Bärensprung[4] arrived at very different results from his more careful experiments. He first ascertained that in rabbits and dogs, while not in the pregnant state, the temperature of the pelvis and that of the uterine cavity are slightly lower than that of the upper part of the abdomen. He then examined the same parts in pregnant animals, introducing a thermometer into the abdominal cavity through a small incision; and he found that in them the temperature of the interior of the pelvis was constantly higher than that of the abdomen, the difference averaging nearly ½° Fah., while the temperatures of the interior of the uterus and of the embryo were slightly higher still. He then proceeded to measure the temperature of the human fœtus, by introducing a thermometer into the rectum immediately after birth; and the results which he arrived at varied very much in different cases. Sometimes the body of the young child was warmer than that of its mother; in other instances the two were of the same temperature; and in a third class of cases the temperature of the mother exceeded that of the infant. These observations are, however, of more value than might appear at first sight; for, as Von Bärensprung very justly remarks, since we know that all children must give off heat after birth at a greatly increased rate, in consequence of their exposure to the external air, the cases in which the fœtus had a higher temperature than the mother have far more weight in proving its power of producing heat than the others have in negativing it. It must be admitted, however, that all these considerations have but a very remote bearing upon the question with which we are immediately concerned, since there is nothing to show that the heat which Von Bärensprung observed is not generated in the uterus or in the placenta, rather than in the fœtus.

Schwartz also adduces the fact that the embryo possesses independent muscular and nervous activity, as an argument in favour of its also requiring aëration of its blood; but it appears to us that no such inference can be fairly drawn from these circumstances.

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[1] Virchow's Archiv, ix., p. 306.

[2] Op. cit., p. 64.

[3] Dissert. Sisteus Experimenta circa Calorem fœtus. Tübingen, 1799.

[4] Müller's Archiv, 1851, p. 126.

It will also be observed that these considerations tell in favour only of fœtal respiration in general, and afford no indication as to the part of the embryo in which this process has its seat. Those, indeed, who have maintained the doctrine that an interchange of gases is necessary to fœtal life have by no means always located this function in the placenta. Certain experiments of Béclard, to which we shall allude directly, led that physiologist and others to believe that the fœtus is constantly respiring liquor amnii during intra-uterine life, and that it absorbs oxygen from that fluid, just as fishes do from the water in which they are immersed. This idea was, however, overthrown by the experiments of J. Müller,[1] who found that tench and gold-fish, placed in the liquor amnii of the sheep, died in 35–45 minutes—living, in fact, no longer than if they were put into the same quantity of distilled water or oil. Again, some of those who oppose the theory of placental respiration, do so on the ground that the liver is the organ which, during uterine life, performs the functions of excretion which afterwards belong to the lungs. This view is well stated by Kirkes,[2] and it derives some support from the fact that the meconium contains a large quantity of carbon and hydrogen, the elimination of which substances would form a chief object of respiration. It is assumed by those who adopt this view that all the matters secreted by the liver during intra-uterine life accumulate in the intestines of the fœtus, and remain there till after birth; and also that they are purely of an excrementitious nature—an idea which receives support from the fact that the alimentary canal of the embryo is, so far as we know, in a state of functional inactivity. But Lehmann[3] found that the contents of the fœtal small intestine are very different from the substances which occupy the large intestine; and these results are certainly opposed to the view that the meconium is a mere collection of effete matter, waiting to be expelled from the embryo on the termination of uterine life; and the fact that the blood which is distributed to the liver comes in great part directly from the placenta, also renders it improbable that the bile is to be regarded as the principal agent in removing excrementitious matters from the fœtal blood.

We now pass on to consider the evidence in favour of the theory of placental respiration, which is derived from the effects of interruption to the circulation in that organ. This is principally of two kinds: the one derived from observations made during parturition in the human subject; the other from experiments on animals. But as a means of proof of the theoretical proposition, experiment is here, as in so many other cases, of far more value than observation, however carefully carried out. During birth, as is admitted by Schwartz, the infant is constantly exposed to irritations, to which the power of exciting the respiratory muscles might be attributed. Appearances undoubtedly due to previous attempts at respiration are frequently found in still-born children, whether or not any of the more obvious causes of interruption of the placental circulation have been known to occur. This fact, and the doubt as to the value to be attached to the varying quantities of blood which the thoracic organs are found to contain after death, render all observations of this kind unsatisfactory as independent evidence, though they are of considerable importance when used merely as illustrations of principles established upon other grounds. We shall therefore, for the present, confine ourselves to the consideration of the results arrived at by experiments upon animals.

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[1] De Resp. Fœtus Comment. Physiol. Lipsiæ, 1823, p. 193.

[2] Loc. cit.

[3] Handbuch d. Phys. Chem. Lipsiæ, 1854, p. 169.

Winslow[1] appears to have been the first who, having opened the uterus of pregnant cats and bitches during life, saw the embryos make distinct respiratory movements, though the umbilical cord was left untouched. Béclard[2] repeated these experiments with the same result: the embryos opened their mouths, their nostrils expanded, their chest-walls were raised; and these actions were repeated at pretty regular intervals, though they were less frequent than the extra-uterine respiratory movements of the same animals. He remarks that they exactly resembled the distant and deep inspiratory efforts made by infants in a state of partial asphyxia. Having tied the neck of the fœtus, so as to prevent the escape of fluid, he opened the air-tubes, and found in them a liquid exactly similar to liquor amnii; and the conjecture that it was liquor amnii was verified by another experiment, in which he injected ink into the amnial sac of a bitch through a small aperture, and afterwards found the respiratory passages of the embryo tinged black by that fluid. These were the facts which led Winslow, and afterwards Béclard, to think that the fœtus respires the liquor amnii during intra-uterine life.

This view was soon, however, contested by Mayer.[3] This observer removed the embryos in their membranes from a recently-killed cat, and found that repeated respiratory movements took place on the application of slight mechanical irritation to these embryos. And on post-mortem examination, several small pieces of the excrements contained in the liquor amnii were discovered in the air-tubes of three of the fœtuses. Mayer therefore suggested that the movements noticed by Winslow and Béclard were to be regarded, not as occurring during the whole of intra-uterine life, but as abnormal, and dependent probably upon some irritation acting upon the embryo, as, for example, obstruction to the uterine circulation, pressure on the funis, retardation of birth, &c. He also showed the incorrectness of the theory of his opponents by another experiment: he poured a green fluid into the respiratory tubes of a pregnant rabbit; and having killed the animal two hours afterwards, found that, though the green fluid was present in the liquor amnii, the maternal part of the placenta, and the stomach of the embryo, there was no trace of it in the air-tubes, lungs, or urinary bladder of the young animal. A modification of these experiments was performed by Nasse.[1] He incised the uterus in a pregnant bitch, which was at full term, and exposed the embryo lying in its membranes. He then compressed the aorta of the mother, so as to cut off the supply of blood to the uterus, and the fœtus soon began to open its mouth, and gasp for breath.

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[1] P. Scheel. Comment. de Liq. Amnii Usû, &c. Hafniæ, 1799. p. 10.

[2] Recherches qui semblent prouver que le Fœtus respire l'Eau contenue dans l'Amnion. Bull. de la Faculté de Méd. de Paris, 1813, p. 436.

[3] Salzburg. Med. Chir. Zeitung, 1817; ii. p. 431. Hufeland's Journal, Sept. 1841, p. 97.

These observations seem, however, to have been very generally forgotten; and, as in so many other instances, one of the most distinguished obstetricians of the present day, experimentizing in the same manner, has arrived at results in many respects similar to those to which we have referred. In 1850, Dr. Tyler Smith[2] made some investigations with a view to determine what particular reflex movements are manifested by the fœtus in uterô under irritation. He exposed the amnion in the uterus of a rabbit nearly ready to kindle, without injury to the attachment of the placenta, and without interfering with the circulation in the cord. When any part of the fœtus was irritated through the amnion, it drew up its limbs, but soon relapsed into the quiescent condition. The one effect which constantly occurred, whether the ears, tail, or extremities were pinched, was a movement of the head, as if in respiration. The mouth was opened and shut again once or twice, as in a kitten while drowning. No attempts at deglutition were made, and no liquor amnii appeared to be swallowed. The respiratory efforts affected the whole of the upper part of the body, but no actual inspiration or expiration occurred. It appeared to Dr. T. Smith that these imperfect contractions of the respiratory muscles must accompany every movement of the fœtus in uterô, and that these muscles are not called into play for the first time after birth, but have been already exercised previously.

It will have been noticed that these results, so uniform in the nature of the movements observed, present considerable differences as to the method of producing them; they have also received very different explanations at different times. Besides the theories of Béclard and Mayer, they were ascribed by Valentin to the absorption of oxygen on the part of the ovum, and by Krahmer to the cooling of the liquor amnii. Schwartz, therefore, determined to repeat these experiments under circumstances which should leave no doubt as to the cause of the phenomena observed. And it may be well to remark in this place that his investigations, while confirming the results obtained by the earlier experimenters, throw considerable doubt upon the value of the observation of Nasse, which we have just quoted, since he found that exposure of the fœtus by incisions into the uterus destroyed the circulation in the placenta so quickly that it was impossible to ascertain the effects caused by cutting off the supply of blood to that organ.

The animals which Schwartz selected for his experiments were rabbits: in them the placenta is always attached to the mesenteric aspect of the uterus; and it is therefore easy to avoid wounding it during the operation of exposing the embryo. The rabbits which he employed were all, as far as could be ascertained, in the last third of their period of gestation; the fœtuses were all sufficiently developed to be capable of making respiratory movements. It was only exceptionally that animals were used, which, by collecting straw and grass, showed that in them the end of gestation was impending. In performing these experiments, Schwartz first made an incision, as small as possible, in the linea alba; on drawing aside the edges of the wound the left uterus was seen, and often protruded at the opening. The parts of the organ in which ova were situated were indicated by projections on the surface; and the next point was to incise the walls of the uterus over one of the prominences, so as to expose only one ovum at a time. After all those in the left uterus had been separately examined, the right organ usually had to be drawn from the bottom of the abdominal cavity, in order to bring it into view. In some cases, the ova remained within the abdomen during the whole experiment; in others, they protruded, and were exposed to the cooling influence of the air; while in a third series of experiments, the rabbit was placed on its side, and the uterus and its contents were allowed to slide into a vessel of water kept at a constant temperature of 100°—104°F., which is that given by Von Bärensprung for the pelvic cavity of rabbits while big with young. In this case, the incision into the uterus was made under water.

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[1] Wagner's Handwörterbuch der Phys., i. 212.

[2] Manual of Obstetrics.

The effects observed under these various conditions were essentially the same in all the experiments, and presented only gradational differences, dependent chiefly upon the period of gestation at which the uterus was opened. When that organ is first exposed, slight peristaltic contractions are observed; these are less frequent, more limited, and weaker, the longer the distance from the normal period of expulsion of the ova. The bleeding from the cut surface of the uterus is at first considerable, but on enlarging the wound it almost entirely ceases; the uterine walls contract firmly; and before the section has much exceeded one half the circumference of the ovum this protrudes, so that no further manipulation is necessary to bring it thoroughly into view. The contraction of the uterus continues, till it forms a riband-like mass on the under surface of the ovum. When the fœtus is ripe, the placenta becomes separated, the seat of its attachment scarcely bleeding at all. When it is younger (up to 3½" long), the connexion with the mother remains uninjured.

The embryos are seen to move more or less actively in the yet unopened uterus. During the exposure of the ovum, these movements increase in some cases. The fœtus is not sufficiently clearly visible to enable their exact nature to be ascertained; but apparently they are not as yet of a respiratory character. Almost immediately after the protrusion of the ovum, an opening and closing of the mouth occur, accompanied, or quickly followed, by a rapid, quivering, inspiratory elevation of the ribs. These movements take place equally whether the placenta remains attached, or becomes separated from its bed; and there is no time to investigate the effect of other methods of interruption of the placental circulation, so completely and rapidly is it stopped by the contractions of the uterus. The respiratory actions are repeated at quite irregular intervals: at first comparatively strong, they gradually become more infrequent and weaker, and finally they cease altogether. The quickness of their appearance after the ovum is exposed, their force, their number, and the frequency of their repetition, vary, as might be expected, with the development of the embryo. The movements which were previously seen in the fœtal limbs begin to diminish as these regular actions come into play, and disappear altogether after the first two attempts at respiration. There is usually no trace of those painful contortions which are seen when rabbits a few days old are forcibly suffocated.

It is important to notice that these phenomena occur both in those experiments in which the ovum is beneath the surface of warm water, and in those in which it is exposed to the air. They show themselves also whether or no the funis be compressed, and whether the ovum be separated from the uterus, or left in connexion with it. Only once did they appear to become more frequent and stronger, and accompanied by more violent movements of the body, when the ovum, the embryo in which had already made some attempts at inspiration, was detached from the floor of the uterus. There was also rarely any difference between the movements of the embryos in the first and last ova, opened in the same horn of the uterus. Sometimes, in the last ova examined, the liquor amnii was mixed with excrement, and the respiratory movements were scanty and weak; rarely were they entirely deficient, or showed themselves only when the ovum was incised, and the fœtus exposed to the stimulus of the atmosphere.

The different embryos reacted differently upon external stimuli. If touched with a probe, or pinched gently through the membranes with the forceps, reflex contractions not infrequently occurred; but respiratory movements usually followed after an interval only, so that they could not be looked on as an effect of the irritation—results which are not in accordance with the experiments of Dr. Tyler Smith, to which we have already referred. When the ovum, held in warm water, was raised above the surface of the fluid, respiratory efforts occurred in some cases so instantaneously that Schwartz could not but ascribe them to the action of the cold upon the fœtus. On opening the ovum a quarter or half an hour after the cessation of the respiratory movements, a gaping of the mouth was sometimes noticed, or even a distinct respiration, at the moment when the embryo came into contact with the air, or when its skin was sharply pinched. How long the circulation continues in the fœtus under these circumstances Schwartz was not able exactly to ascertain. He observed in some cases pulsations of the arteries in the funis near the fœtal abdomen, ten minutes after the ovum had been exposed. When the embryos were examined within three quarters of an hour to an hour after the experiment was commenced, the heart was generally found still beating rhythmically; and even in younger fœtuses, in which this was not the case, contractions of the heart could usually be excited by mechanical irritation. The appearances found in the bodies of these embryos were not usually characteristic. The object being to ascertain the cause of these respiratory movements, and not to trace their effects, the fœtus was usually too young, and its respiratory apparatus was too yielding, for the movements to be attended with the results which we shall find to occur in the human embryo, when respiratory efforts are excited in it during birth, or at the end of the full term of gestation. The air-tubes of these fœtuses contained often a watery fluid, which was probably liquor amnii. The pulmonary vessels always contained blood, and the cavities of the heart, and especially its auricles, were filled with dark fluid blood; but the ecchymoses of the thoracic viscera were altogether wanting.

Schwartz also made a series of experiments, with the object of ascertaining whether the pulmonary vessels contain blood during intra-uterine life, or whether the circulation in them first commences with the functional activity of the lungs. It had been suggested by Veit, in the discussion which followed the paper of Hecker, read before the Obstetrical Society of Berlin, which stands at the head of this article, that it might be possible to test this question by cutting through the medulla oblongata of the fœtus immediately after laying it bare within the uterus, and before the respiratory movements began. This plan was not, however, adopted by Schwartz, both on account of the difficulty of performing the operation with sufficient rapidity and certainty, and because is was at least possible that the irritation of the nervous centres by the section might itself be the starting-point of the same movements. The method which he employed was to destroy the effect of the expansion of the thoracic parietes on the lungs by perforating the pleura on each side as soon as the chest of the embryo came into view. If this was not done too violently, the lungs receded before the knife, and remained uninjured. In this way he repeatedly succeeded in annulling the effect of the respiratory movements, which established themselves after the walls of the thorax had been pierced.

In all these experiments the trunks of the pulmonary veins and arteries, as well as their larger branches within the lungs, contained blood. The degree of fulness of the vessels varied, however, very much with the period of development of the embryo. When it was very young, the lungs were pale, and blood was present only in the main trunks of the arteries and veins. When the fœtus was more fully developed, the lungs were darker and of a reddish-brown colour, and the blood in the pulmonary vessels was much increased in quantity; but even then it could only be demonstrated in their larger branches; and there was a very marked difference between this condition of the lungs and the diffused congestion of these organs which is found in embryos in which the respiratory movements have had free play. These experiments, therefore, confirm the views of Veit, that the circulation in the pulmonary vessels begins gradually with advancing development, and is not suddenly called into existence by the establishment of respiration: but they do not allow any doubt to be entertained that the ecchymoses, and other signs of pulmonary engorgement, which are frequently found in still-born children, are to be regarded as evidence of a pathological state.

It cannot, we think, be denied, that the experiments which we have detailed go far to prove the reality of that process of aëration of the blood which we believe to have its seat in the placenta; and their value is so much the greater because the other evidence is, as we have seen, very imperfect. We have now to pass on to the next part of our subject—the question how far observation of the process of parturition in the human subject confirms the conclusions derived from these experiments on animals. But before doing so, we will pause to consider an objection which will probably have occurred to many of our readers. How, it may be urged, is it, if the contraction of the uterus which follows an incision into that organ is sufficient to cut off the circulation in the placenta, and to cause asphyxia of the fœtus, that this result does not occur in every parturition? It would be a natural conclusion from these experiments, that every child born, except where labour is extraordinarily short, must have already executed respiratory movements. Now, we must admit that no answer to this objection has been proposed which is perfectly satisfactory. It might be suggested that the uterine contractions during the process of birth are followed so quickly by relaxation, that the circulation in the placenta is restored before its disturbance has produced any serious effects; but this is hardly in accordance with the rapidity of the production of efforts to inspire, in the embryos experimented on by Béclard and Schwartz.[1] The latter observer supposes that the mode of action of the uterine contractions upon the circulation in the placenta is peculiar, and that it produces "a mechanical alteration in the distribution of the blood without impeding the process of aëration." This explanation, however, altogether fails to remove the difficulty, since the question is, why contractions of the uterus, which cause no danger to the child during birth, give rise to such remarkable effects when the organ has been cut open. It is probable, we think, that sufficient attention has not been paid to the difference in the effects which would be exerted by its contractions on the vessels in the walls of a hollow muscular organ like the uterus, in a full as compared with an empty condition. It is not at all improbable that the pains which occur while the fœtus is still in uterô produce far less compression of the vessels which lie in the uterine walls than those which follow the expulsion of the child. Now, when the uterus of an animal has been incised so completely as was done in Schwartz's experiments, it is able to contract to a degree that was impossible while it was, if we may be allowed the expression, *stretched over* the embryo; and its condition would resemble that of the uterus after rather than before birth. It was noticed by Schwartz that hæmorrhage, previously considerable, ceased at once as soon as the wound in the walls of the uterus was enlarged;—and this renders very probable the view which we have

[1] Op. cit., p. 118.

advanced, for it is at least likely that the organ would be stimulated to a certain amount of contraction when the incision into it was made. However this may be, there can be no doubt that in healthy parturition the child is born without having made any respiratory efforts, and that the first attempts at inspiration occur, as a rule, after the head is born in cephalic presentations, and during the expulsion of the fœtus in breech cases. The cause of the first respiration of the fœtus has not been completely ascertained. The experiments of Schwartz confirm the view that the contact of cold air acts as a powerful stimulus to the fœtus when first exposed to it; but it is probable that the interruption of the placental circulation, which must be caused by the uterine contractions as soon as the fœtus is born, is of itself sufficient to give rise to inspiratory efforts. It cannot be doubted that the supply of maternal blood to the placenta is in all cases cut off when the complete expulsion of the child has been effected; and therefore, as is remarked by Schwartz, it is quite useless to delay the ligature of the funis when the respiratory movements are imperfect in the newly-born infant, as is advised by many authorities.

But if the phenomena of normal parturition are not inconsistent with the views which we have been expounding, those which are observed in some forms of the deviations from natural labour give to this theory far more support, and receive from it their best explanation. Observations of this kind are recorded simultaneously with the experiments on animals to which we have already referred. Scheel found meconium and liquor amnii in the trachea and stomach, in some cases in which he made post-mortem examinations of stillborn children. Both Béclard and Winslow mention that they always found the air-tubes of infants born dead filled with liquor amnii. Glimpses of the truth were also perceived by Jörg;[1] but it was first clearly laid down by P. Dubois,[2] that when the circulation in the fœtal part of the placenta is interrupted by compression of the funis, lively fœtal movements take place, showing the discomfort which the child suffers; and that if the compression continue, respiratory efforts are excited. In this way he explained the rare occurrence of a vagitus uterinus, when the fœtus is so situated that air has access to its mouth and air-passages. Where this is not the case, he correctly observed that liquor amnii enters in its place. The expulsion of the meconium from the fœtal rectum was also noticed by him under the same circumstances, and ascribed to the influence of the active respiratory muscles; and he states that portions of this substance may be sucked into the air-passages with the liquor amnii. Bérard[3] treats the development of inspiratory actions in the embryo, when the circulation in the placenta is interrupted, as an established fact; and Jacquemier[4] and Cazeauz refer to premature activity of the muscles of respiration and deglutition the occasional presence of meconium in the fœtal stomach.

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[1] Handbuch der Geburtshülfe. p. 116. Leipzig, 1820.

[2] Mém de l'Acad. de Méd. de Paris, t. ii.

[3] Cours de Physiologie, t. iii. p. 523. Paris, 1851.

[4] Traité Théor. et Prat. de l'Art des Accouch., 3rd ed. p. 240. Paris, 1850.

Among the circumstances which interfered with the general acceptance of these views was the fact that the stimulus to the respiratory actions of extra-uterine life was believed to originate in the lungs, the pneumogastric nerves being regarded as the agents in exciting the centres to action, so far as the function of respiration is concerned. Now during fœtal life, when the lungs are inactive, no impression on these nerves rather than on others would be produced by impeding the process of aëration, and therefore the analogy between obstruction to the placental circulation and strangulation lost somewhat of its force. Thus an essential step was gained when it was shown by Volkmann[1] that the function of acting as incident nerves to respiration is by no means confined to the vagus, but belongs also to the sensitive nerves generally. He regards the regular action of the respiratory muscles as the result of the necessity for oxygen felt by the whole body. Vierordt[2] carried these views somewhat further, remarking that the substance of the nerve, like every other structure in the body, takes part in the change of gases between tissue and blood; and it is therefore only necessary to suppose that the nerve conveys to the centres the knowledge of the condition in which it is itself placed with reference to the supply of oxygen. All this is, it is true, theoretical; but it agrees well with the results of experiments on animals which, as is well known, show that the connexion of the pneumogastrics with the nervous centres is not necessary to the continuance of respiration; and it certainly renders more easy the understanding of the effects of interruption to the circulation in the placenta.

It was, however, Krahmer[3] who first collected all the facts up to that time observed, and combining them with observations of his own, wrought them into a homogeneous theory. To him belongs the merit of having drawn attention to the views of older observers, which had slipped into oblivion; and he is admitted by all the other writers whose works form the subject of this review, to be the original author of the modern theory of fœtal respiration. Starting from the physiological axiom, that the respiratory movements are essentially independent of the condition of the lungs, and are performed in obedience, indeed, to the call for oxygen, but without regard to the results which they effect, Krahmer proceeded to consider what these results would be, under the conditions which exist during intra-uterine life. The immediate effect of expansion of the thorax is, of course, if its parietes are sufficiently firm to exert the necessary force, to suck into the air-passages any liquid or gaseous substance which may have access to them. If the mouth be so placed that air can enter, this at once rushes in; and the vagitus uterinus must be supposed to have occurred under such circumstances as these. If the fœtus be surrounded by liquor amnii, this fluid, and with it any portions of meconium or of vernix caseosa which it may contain, are absorbed into the mouth, larynx, and trachea. But if these substances fail from any cause to enter the air-passages, another effect is produced, which was first fully explained by

[1] Müller's Archiv. 1841. [2] Wagner's Handwörterbuch d. Phys., ii. p. 912.

[3] Op. cit. Handbuch d. Gerichtl. Medicin. Halle, 1851.

Krahmer. The tendency to a vacuum within the thorax then leads to a rapid filling with blood of the pulmonary vessels, and of the capillaries of the heart and lung. There is often a dentritic appearance of the vessels on the surface of these organs, and their tissue acquires a dark-violet tint, and this may even increase, till the delicate walls of the vessels give way, and exudation into the tissues takes place, in the form of ecchymoses. These extravasations of blood are found chiefly on the surface of the lungs, and beneath the epicardium, near the base of the heart. They had already been noticed by Bernt,[1] as occurring in children who had given evidence of life after birth, but who had not breathed; and they had been by him obscurely attributed to the violent efforts of nature to overcome the obstruction to respiration. Bayard[2] and Casper[3] had also observed these punctiform ecchymoses of the pleura, the epicardium, and the great vessels, in infants who had been suffocated in bed or killed in other ways, and had indicated them as signs of death by asphyxia. They have been especially insisted on by Hecker,[4] who looks on them as by themselves sufficient to warrant the conclusion, that respiratory movements have been made before birth. He states that neither in adults nor in older children has anything exactly similar ever been observed. We think, however, that too much importance may be attached to these ecchymoses, when they are unsupported by other evidence. Hecker himself records two instances in which the cause of extravasations of blood is admitted by him to be doubtful. The first of these cases, is that of a woman who was subject to epileptic fits. After a severe rigor of an hour's duration, which came on during her pregnancy, the child's movements gradually ceased. When the fœtus was expelled, it was found to be imperfectly developed: the epidermis hung down in shreds from several parts of its body; a considerable quantity of bloody, dark-brown fluid existed in the cavities of the pleura, pericardium, and peritoneum. The lungs lay close to the spine, and were covered by many ecchymoses, as also was the heart near its base. The other was the case of a child which, when born, was not fully developed, weighing only 3¾ lbs. In the skin of the neck and breast, and also at other parts, were numerous small ecchymoses; similar ones existed also in the subcutaneous tissue and muscles. On the pericranium of the right parietal bone was a layer of black blood. The lungs, thymus gland, and heart were mottled by extravasations of blood, some of them of considerable extent; at the base of the heart they reached the size of a pea. In the lungs, they were not only situated on the surface, but existed also in the pulmonary tissue. The fact that in this case the ecchymoses were present, not only in the thoracic viscera, but also in the subcutaneous tissue, and in other parts where their occurrence is not explicable on the theory propounded by Krahmer, appears to us to justify considerable hesitation in the application of them, without corroboration from other facts, as proofs of respiratory efforts. It is also certain that they may be absent, when death has undoubtedly occurred from intra-uterine suffocation. Böhr found them wanting 18 times in 75 cases, or in 24 per cent. This observer appears to have been the first to notice the presence of ecchymoses of the thoracic viscera in children who, having been in a state of asphyxia during birth, and having made the usual respiratory movements, recovered, but died soon afterwards from some other cause. Thus a child extracted by him in consequence of a prolapse of the funis, was born asphyxiated, but was restored by the warm bath with cold affusion, only to die three hours later. In the body of this infant the petechial extravasations of blood were well marked, and were evidently the results of respiratory efforts made before birth. And there is no doubt that they might be found also in children who had survived birth much longer than this.

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[1] Verfahr. bei der Ausmittlung zweifelhaft. Todesart. Neugeb. Wien, 1826. pp. 57, 85.

[2] Ann. d.'Hyg., 1847.

[3] Gerichtl. Leichenöffnungen. Erst. Hundert. p. 84.

[4] Op. cit.

As might be expected, these ecchymoses are most numerous, and the coëxisting congestion of the pulmonary tissue is most decided, in those cases in which neither liquid nor air has been able to enter the respiratory passages, in order to occupy the space created by the expansion of the thorax. Now, during birth, it not infrequently happens that the mouth of the child is so situated in contact with its membranes, or with the walls of the vagina or uterus, that neither liquor amnii nor air has access to it. Under these circumstances, as we have seen, the ecchymoses are produced. But there is also another cause which may prevent fluid entering the air-passages, though the fœtus may be lying surrounded by liquor amnii. If the cartilages of the trachea and bronchi be not sufficiently resistent to keep the walls of these tubes open, in spite of the pressure exerted upon them, the only effect of respiratory efforts is, of course, to draw the sides of the air-passages into closer contact with each other. Before the twenty-fourth week of fœtal life, this appears actually to occur; and the proof of it is, that inspiratory efforts are in these young embryos equally ineffectual, whether they be performed before or after birth. Böhr relates a case which strikingly illustrates this fact. In a fœtus born prematurely in the fifth month, he saw distinct rhythmical respiratory movements, while it was lying between the thighs of its mother. These actions became stronger when the feebly-pulsating funis was tied. They continued about ten minutes. On cutting through the umbilical cord again, a little blood escaped; but gradually both circulation and respiration ceased. The autopsy was made three hours after death. Only one spot at the upper part of the middle lobe of the right lung, about the size of a pea, contained air, and floated in water. The other parts of the lungs were loaded with blood, and of a dark-brown colour, but were completely airless. Under the pleura of each lung, and on the thymus gland and aorta, were numerous punctiform ecchymoses. This case is certainly an instructive one, since it can hardly be doubted that the ordinary appearances found in still-born children were here generated by movements performed under the eye of the observer, after the fœtus was expelled from the maternal passages.

The presence of air in the lungs of embryos who have died before birth is a very exceptional occurrence. Schwartz only found it in four cases. Böhr's tables do, indeed, give twenty-three cases per cent., in which more or less of the pulmonary tissue contained air; but it then only occupied a very small portion of the lungs, and was found especially in the middle and upper lobes of the right lung, which are generally the first parts of the organ to become inflated, when the supply of air is scanty. Moreover, in all the cases of Schwartz, operative means were used to quicken delivery; and it is probable that this facilitated the access of air to the fœtal respiratory passages. In other cases, the last respiratory efforts of the dying fœtus occurred after its head had been expelled from the vagina; and a third source of fallacy arises from the attempts at artificial inflation of the chest, which are almost always made in these cases, with the hope of saving the child's life. In no one of the cases which form the basis of the tables given by Böhr, could all these conditions be proved to be absent; so that the numbers which he gives have but a very subordinate value. The vagitus uterinus appears never to have been heard by any modern observer; its occurrence, therefore, rests upon the same evidence which existed before these investigations were made.

We have already more than once alluded to the fact, that in many cases of intra-uterine asphyxia, liquor amnii, with or without meconium, is sucked into the air-tubes by the premature respiratory efforts. Portions of the vernix caseosa often accompany these substances. They are easily recognised under the microscope, being made up of epidermic scales from the cuticle of the child. Fine hairs, also, which are peculiar in having no medulla, and belonged to the lanugo, are frequently seen in the same way; and the presence of meconium, if doubtful, is indicated by the numerous crystals of cholesterine which it contains. These substances are often mixed with blood, apparently derived from the maternal vessels, and sucked in from the uterine cavity; and not infrequently the air-tubes also contain bits of the secretion of the cervical glands of that organ. According to the tables drawn up by Böhr, liquor amnii was found in the respiratory passages of 47 children out of 75 which were examined, or in 62⅔ per cent. An admixture of meconium was present in about half these cases.

The detection of these substances in the fœtal air-tubes often requires considerable skill and care. Schwartz tells us that, when he first began to make post-mortem examinations of still-born children, they were noticed but seldom, and only when their quantity was very large. But when his attention became directed to the subject, their presence was found to be almost constant. It is necessary to lay open cautiously the bronchial tubes before washing them; and in doubtful cases, he recommends slitting up the nose, and the careful examination of its chambers, as well as of the palate, pharynx, and buccal cavity. The trachea in its middle and lower part is generally empty, or contains but little of these substances, but below its bifurcation they are usually present, and they often extend even into the finest ramifications of the bronchial tubes. Besides occupying the air-passages, these "specific substances," as the Germans call them, are frequently found also in the upper part of the digestive canal. The œsophagus is usually free from them in its lower part; but they are often present in the stomach, accompanying the gelatinous mucus which it normally contains at birth. There can, therefore, be no doubt that movements of deglutition are often associated with the respiratory efforts in these cases. A remarkable instance is recorded by Dr. Fleischer,[1] in which the pharynx, œsophagus, and stomach, as well as the larynx, trachea, and bronchial tubes, to their finest divisions, all contained a viscid greenish-yellow fluid. The precise nature of this substance was not determined; but it was probably meconium, for the resemblance was exact both in their colour and in their consistence. The case is remarkable both on account of the great extent of the parts in which the foreign substance was found, and also in its consisting apparently of pure meconium, with little or no admixture of liquor amnii. A case is related by Liman,[2] in which the lungs and air-tubes contained neither of these substances, and yet a small quantity of a greenish fluid was found in the stomach, while the liquor amnii itself was stained by meconium. In this case, however, the lungs were strongly injected and ecchymosed, so that there can be no doubt that respiratory movements had taken place. The absence of the "specific substances" in the thoracic organs was probably due to some accidental cause. When deglutition occurs in the fœtus under these circumstances, it appears to be only a subordinate effect of the irritation of the medulla oblongata and pons Varolii, which gives rise to the inspiratory efforts.

The other post-mortem appearances found in these cases of fœtal asphyxia are of minor importance. The right side of the heart is often gorged with blood, the liver, kidneys, and spleen are congested, and the face is livid. This is, however, by no means invariably the case, for in some instances, in which death was undoubtedly caused by intra-uterine suffocation, and premature respiratory efforts had certainly occurred, the face is quite pale. This led to the separation of the cases of death during birth into two classes—the one of anæmia, the other of hyperæmia, or apoplexy. But, as in the similar cases of adults, the utmost pallor of the surface is often found to coincide with the most marked congestion of the lungs and viscera, so that this distinction is without foundation. Effusions of blood beneath the membranes of the brain, or in the substance of that organ, were not infrequently found as a complication in these cases, being, in fact, present in 28 per cent. of those collected by Böhr. It is probable that, as a rule, these intra-cranial effusions are caused by the compression of the fœtal head, especially when the labour is protracted; but it would appear that there are cases to which this explanation does not apply. They may, perhaps, result, as Böhr believes, from the propagation of the obstruction to the circulation backwards through the superior vena cava to the intra-cranial vessels; but it must be remembered that these effects are not likely to follow one another with the same regularity as in the adult, since there is an especial provision in the ductus arteriosus and foramen ovale of the fœtus, which might be expected to prevent any accumulation of blood in the right side of the heart, as a consequence of the congested condition of the lungs. Extravasations of blood have also been found in the abdominal cavity, in the anterior mediastinum, and within the duodenum.

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[1] Casper's Vierteljahrsschrift, 1863, vol. xxiii. Heft 1, p. 97.
[2] Ibid. 1860. (Case 17.)

The symptoms which indicate the occurrence of fœtal asphyxia during birth, while the child is yet alive, are fully discussed by Schwartz. The most positive evidence is of course afforded by the direct observation of the respiratory movements; but this can very rarely be done. It is maintained by Schultze,[1] that he ascertained their presence, in a case of prolapse of the funis, by auscultation of the abdomen of the mother, when a respiratory murmur was heard; this statement, however, certainly requires confirmation. In some cases, but only as an accidental occurrence, movements of the fœtus may be distinctly felt by the hand, or may be transmitted by the forceps during their application, which can be recognised as accompanying the inspiratory efforts. Another indication has been found in the diminished irritability of the fœtal limbs in these cases; but it is admitted by Schwartz that this scarcely ever gives any aid to the diagnosis. If during labour distinct movements of a presenting limb can be caused by gently tickling it, or if touching the mouth be followed by a contraction of the lips, then, according to our author, the absence of a state of fœtal asphyxia may be inferred. It will be remembered that, in his experiments, the reflex excitability of the limbs disappeared before the respiratory movements were established; though P. Dubois maintained the contrary. But from the failure to elicit contractions of the fœtal muscles, no conclusions can be drawn, for there may not be sufficient space to allow them free play; and the susceptibility to mechanical stimuli in different individuals varies greatly. Much more important indications are obtained by auscultation. Schwartz found that, at the termination of intra-uterine life, the average frequency of the fœtal pulsations is 144 in the minute, and that the heart's action remains unchanged during the whole of birth, with the exception of slight transitory modifications in its rhythm, caused by the uterine contractions. In most instances it is somewhat diminished in frequency during each pain, but this is often not the case; and when this alteration does occur, the organ returns to its former rhythm within five seconds after the uterus has again relaxed. Now, a condition of infantile asphyxia very generally diminishes the rapidity of the fœtal pulse, and also reduces its force. This affords a valuable aid to the diagnosis; but it is necessary that the exact number of the contractions of the heart of the embryo should have been ascertained before the interruption to the placental circulation occurs. In different fœtuses there are great differences in the rapidity of the heart's action. It is not sufficient to rely upon the average number of pulsations, since the variations are so great. A rhythm which would be perfectly normal in one embryo, would indicate surely impending danger in another. In prolapse of the funis, which, as we shall see, is in a large proportion of cases the cause of disturbance of the functions of the placenta during birth, the fœtal pulsations may often be felt to cease in the loop. This has always been regarded as a very unfavourable sign, so far as the child is concerned; but it usually occurs some time before the heart's action fails.

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[1] Deutsche Klinik, 1857, No. 28.

The expulsion of meconium and of urine is another circumstance which has been called in to aid in the diagnosis in these cases. It is well known that the time of the evacuation of these excretions varies under different circumstances. Schwartz has endeavoured to ascertain the law of these variations; and his conclusions are as follows:

1. When the fœtus is born in a state of asphyxia and recovers, and still more when it is born dead, the discharge of meconium and of urine occurs as a rule before birth.

2. Not only the urine, but also the fæces, are more frequently discharged within the maternal passages by girls than by boys. This coincides with the results obtained by Elsässer, who has also investigated this subject.

3. Neither the degree of development, nor the position of the fœtus, nor the varying dimensions of the soft parts through which the child has to pass, appear to exercise any constant influence, so far as the period of evacuation of the excretions is concerned.

The number of cases, however, from which these conclusions are derived, is so small, that we cannot attach any great value to them. The last one especially is in opposition to general experience, which teaches that, in breech cases, premature expulsion of the meconium is far more common than in cephalic presentations. It is true that fœtal asphyxia is itself more frequent in the former class of cases; but Böhr states that he has often seen the meconium discharged when the presenting breech has been delayed some time in the pelvis, though the child was born perfectly free from asphyxia. In cases where the head presents, he is inclined to attach considerable importance to the expulsion of meconium, as indicative of danger to the fœtus.

It must be borne in mind that the condition of the rectum affords no means of deciding whether partial expulsion of the meconium has already taken place or not. By passing a catheter into the anus, Schwartz often ascertained that the rectum of the newly-born child was full of this substance, though the soiling of the nates with it had been observed before birth. One of the effects of the expulsion of meconium is to stain the membranes and funis of a greenish colour, which may, under certain circumstances, afford aid to the diagnosis of the cause of death of the embryo. More or less of the fœtal excrement must of course have already been evacuated, before it can be sucked into the fœtal air-passages, as so frequently occurs. The fact that both the urine and fæces are often discharged by the fœtus before its death, diminishes greatly the value of the observation of Beale,[1] that casts of the uriniferous tubules are to be found in the liquor amnii. The case in which he discovered these bodies was that of a woman who died in the eighth month of gestation; but when her circulation failed, respiratory movements were probably excited in the embryo, and it may have been at this time that the urine was evacuated and mixed with the fluid in the amnion. This observation therefore affords no proof that the excretion of urine takes place normally during intra-uterine life.

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[1] Todd and Bowman: Physiology, vol. ii. p. 617.

The cause of the expulsion of the excrements in these cases is not clearly made out. The fact that the staining of the membranes, and the presence of meconium in the liquor amnii, are among the regular appearances found when the fœtus dies *in utero* before labour has commenced, appears to show that it is not due to the pressure exerted on the fœtus by the uterine contractions. The action of the fœtal diaphragm during the premature respiratory movements is also excluded by Schwartz, on the ground that the same cause does not produce this effect after birth. But it is not unlikely that a relaxation of the sphincter muscles occurs in these cases as a direct consequence of the state of asphyxia from which the embryo suffers; and if this be so, Schwartz's argument falls to the ground. He is himself disposed to compare the expulsion of meconium and urine in the asphyxiated fœtus to the evacuation of urine, semen, and fæces observed in adults who are hanged; and he attributes it to a contraction of the muscular walls of the rectum and bladder.

The causes of premature respiratory activity in the fœtus, with the pernicious consequences which we have seen to follow from it, comprise, of course, all those conditions which may interrupt the circulation in either of the two vascular systems which make up the placenta. They naturally group themselves in three classes—an arrangement which was, indeed, proposed as far back as the beginning of this century by Herholdt.[1] These are—

1. Obstruction to the passage of blood in the fœtal part of the placenta; compression of the funis.
2. Premature separation of the placenta; interrupted circulation in the maternal part of that organ.
3. Interrupted circulation in the maternal part of the placenta, from death of the mother.

It is chiefly to the first class of cases that Krahmer refers in his paper in the 'Deutsche Klinik,' which, with his other writings, forms, as we have said, the starting-point of the modern investigations on the subject of this review.

> "Up to the present time," he says, "all my experience points to the fact, that in cases of prolapse of the funis, the fœtus is stimulated to perform premature respiratory movements, and in consequence of the absence of air, dies suffocated *in utero*. During the past year I have examined the bodies of two fully-developed infants, who died during birth from this accident. The respiratory passages were found quite airless, but filled with a more or less viscid substance; the pulmonary vessels were loaded with partly fluid and partly coagulated blood; and the surface of the lungs was covered with ecchymoses. The portion of the funis which had been prolapsed was distinguishable at first sight from the rest of the cord."

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[1] Comment. über das Leben, vorzüglich der Frucht in Menschen. Leipzig, 1803, p. 82.

In six fatal cases of the same kind, related by Hecker, the ecchymoses on the heart and lungs were present in every instance, and in one, at least, liquor amnii had made its way into the air-tubes. Hohl[1] fully admits the conclusiveness of these observations, but thinks that it is not yet proved that death always results from suffocation in cases of this nature. He believes that, in some at least of them, the immediate cause of death is apoplexy. This opinion is, perhaps, supported by the admission of Böhr, already quoted, that effusion of blood within the cranium is a not infrequent pathological appearance in these cases.

It appears to be generally allowed by German obstetricians, that the twisting of a loop of the umbilical cord round part of the fœtal body may endanger its life. Two instances of this are related in Hecker's paper: in one, the funis was only loosely wound round the neck of the infant, but the gelatine of Wharton was entirely absent. The pains, also, were enormously powerful, and followed almost without intermission. The intra-thoracic effusions of blood were present in the body of the infant in this case; and the air-tubes contained a viscid yellowish substance, in which hairs were found when it was examined by the microscope. This case, however, proves nothing as to the danger caused by the winding of the normal funis round the fœtal neck, since its power of resistance to pressure must have been much impaired by the absence of the gelatine of Wharton. The other case is one in which labour lasted eleven hours, and in which twenty grains of secale were given, on account of the feebleness of the pains. The funis was wound twice round the neck of the infant, and fœtal asphyxia occurred before birth. Krahmer himself gives a case of this kind, in which delivery was effected by the forceps, in consequence of the slowness with which the labour went on. The embryo died during the operation, and the cord was found to be rolled tightly three times round its neck. While the forceps were being used, Krahmer suddenly perceived violent movements of the child, which soon ceased, and were not repeated. There was the usual evidence that the child had made respiratory efforts before birth; but it is impossible to say that these were not caused directly by the application of the instruments, though Krahmer thinks this very unlikely; and therefore no positive conclusion can be drawn from this case. Mayer[2] has collected 685 cases in which the funis was wound round the neck of the fœtus; in 18 of these only did the child die.

Directions are given, in works on obstetrics, that the funis should be at once removed from the infant's neck after birth, when it is wound round it, so as to free the trachea and large vessels from pressure. This shows that danger has hitherto been looked for from this cause, rather than from the compression of the funis. But it is stated that the same results follow when the umbilical cord tightly embraces a limb, as when the neck is the subject of the pressure; and this, if true, forms a powerful argument against the view which is generally accepted. Another cause of obstruction to the circulation in the funis is admitted by Hecker. In certain rare cases he ascribes the appearances recognised as indicative of fœtal asphyxia to pressure on the funis from abnormal positions of the child, or from morbid states of it, such as hydrocephalus; but this appears to be purely hypothetical.

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[1] Op. cit., p. 837.

[2] Hecker, op. cit.

The second class of cases consists of separation of the placenta during birth. Hecker records three instances of this occurrence, in which an examination of the fœtus was made. In one of them, though the embryo wanted six weeks of its full development, the extravasations within the thorax were well marked. The appearances found in the fœtus in cases of placenta prævia deserve further investigation.

We have only met with two cases of death of the mother, during advanced pregnancy or labour, in which the state of the fœtus appears to have been noticed. They are contained in Hecker's paper. In one of them, rupture of the uterus, which was rapidly fatal, occurred from the obstruction caused by a hydrocephalic condition of the fœtus. The child was removed by the Cæsarean section. The most evident signs of death by suffocation were observed on post-mortem examination. The other is a case related by Virchow, of a pregnant woman who died of cholera in 1848. In this case, also, the child was at once extracted through the abdominal walls, but was dead. Extravasations existed on the pericardium and the pleura pulmonalis, as well as in the coats of the intestines.

It only remains to consider what are the bearings of these views upon the points which arise in medico-legal practice. This is the principal object of Böhr's paper; and therefore we shall follow him especially in our remarks.

The first question is, whether the hydrostatic and other tests, in which the presence or absence of air in the lungs is used as a criterion of live birth, are in any way diminished in value by these observations. To this question an answer in the negative may with confidence be given, for in almost all forensic cases, the circumstances to which the entrance of air is ascribed in the instances given by Böhr, have, from the nature of the case, been absent.

The occurrence of the vagitus uterinus has always received its due amount of attention from writers on forensic medicine; and the theory which we have been unfolding rather lessens its importance by referring to definite causes a phenomenon which was previously unexplained. We have seen that, in all the cases of children which were dead at the time of birth, and in which even a small quantity of air was found in the lungs, its presence could always be accounted for on the ground that artificial inflation had been practised after birth, or that delivery had been effected by artificial means, or at least that, during the examinations made by the accoucheur, air had been enabled to get access to the fœtal air-passages. Now, the conditions under which birth has occurred in cases of suspected infanticide are almost always such, that these causes can be left out of consideration with perfect certainty. It is true, that it was not proved that the entrance of air into the lungs really arose in the way which we have supposed; and it must be admitted that cases have been related which seem to set at defiance the principles deduced from all other observation and experience. Such an instance is that recorded by Hecker,[1] in which the lungs of a child, born dead without operative interference, were found completely inflated with air. This case has caused Casper to modify somewhat the views which he held upon this subject. The only explanation of it which can be given is, that some one may possibly have practised artificial inflation on this infant while it lay in the dissecting-room; though Hecker appears himself to have rejected this supposition. These difficulties have, however, long been known to exist in the application of the hydrostatic test; and it may be safely asserted that the modern views do not in any way increase them.

It is, perhaps, possible that in some cases liquor amnii and meconium might be mistaken for fæces and urine when found in the fœtal air-passages, if there were reason to suspect that the child had been drowned in a privy or chamber-vessel. But these substances would easily be distinguished on careful examination by anyone who was alive to the fact, that meconium frequently enters the trachea and bronchi during birth. Moreover, the amount of air in the lungs would form a safe guide in most of these cases.

But the great value of these observations, looked at from a medico-legal point of view, lies in their affording evidence by which a large proportion of still-born children may be known to have died during birth, or at least to have been born with the cause of their death actively working upon them. Most of the cases of suspected infanticide, in which the child really died during birth from natural causes, would be at once cleared up by a careful post-mortem examination, in which attention was drawn to the appearances which we have been describing. There are, however, certain limitations to their value which must not be overlooked. The absence of petechial extravasations of blood on the thoracic viscera is no proof that death did not occur during birth; for though Liman[2] says that they are always present under these circumstances, this is shown by Böhr not to be the case. Again, the presence of the signs of respiration having taken place before birth does not absolutely prove that death occurred at the same time. We have seen that small quantities of air are often found to co-exist with evidence of fœtal asphyxia, and apparently result in many cases from the infant having made its last respiratory efforts after its expulsion from the mother, and then having died. Moreover, the sub-pleural ecchymoses are probably very persistent, and might be found in infants, in whom they had been generated, and who had afterwards recovered, and survived birth some hours. These cases are, however, very rare. Krahmer is probably right in considering the entrance of liquor amnii into the chest as far less injurious to the fœtus than these pulmonary congestions and hæmorrhages; and therefore recovery after they have occurred is probably not very common. Children undoubtedly often survive who have sucked liquor amnii into the air-tubes during birth, the fluid being expelled in the first few respirations which they make after being born; so that there remains no evidence of the premature inspiratory efforts having been made. It is therefore only in rare instances that a child which really lived after birth would be believed to be still-born on these grounds.

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[1] Virchow's Archiv, 1859, Band xvi. Heft 5 and 6, p. 535.

[2] Casper's Vierteljahrsschrift, Jan. 1861, Heft 1.

We have now, we believe, laid before our readers the chief points which have been ascertained by modern investigation with reference to the subject of the respiratory function of the placenta, and of the effects of its interruption. There are, no doubt, questions still undecided, and difficulties yet to be cleared up; but these have but little weight when opposed to the evidence from observation and experiment, which we have been unfolding; and there are, as we think, few doctrines in physiology against which there have not been urged graver objections than those which are employed against this.

REVIEW IV.

On the Causes of Sickness in the English Wars, and on the Means of Prevention. By E. A. PARKES, M.D., F.R.S., Professor of Military Hygiène in the Army Medical School. (From the 'Journal of the Royal United Service Institution,' vol. vi. p. 16.)

To our eye these few pages of history appear to rank amongst the most important that have ever been written. Instructive to those engaged in the practice and study of medicine, especially in its application to the health of armies, they are in an equal degree calculated to make the soldier reflect on those divisions of the military art which are most slurred over by writers, and which by mere tacticians are witlessly despised. Nay more, we shall say we would willingly see them pass into the hands of every man that can read and write, so important are they in their general aspect to the nation, so necessary is the diffusion of the lessons which they teach to effect change in the evils they are intended to reform.

The subject of the pamphlet before us is the health of armies considered historically, and in application to our military service. Short and condensed, it makes evident from recent examples how necessary is vigilance, how essential is resource, and how inevitable is disaster when each contents himself with merely doing his appointed share of work, not "looking on the things of others as well as on his own," or interesting himself in the final result.

In the briefest of all possible essays we see marshalled before us the few diseases whose frightful repetition and strong identity seem fated, as long as our nation lasts, to destroy our armies, to embitter our conquests, impoverish the blood of the nation, and neutralize the employment of our wealth.

Not only the devastating and disabling maladies which have left their mark upon our annals are here brought forward for instruction, with details full of interest and enumeration sufficiently complete, but the causes are severally indicated which, by a direct action, invariably produce the results of disease; so true is it that, understanding the conditions, you can with much certainty infer the disease, so also, knowing the disease, you can easily lay your hand upon the cause.

> "The rations issued at Carthagena in 1741," says our author, "are not mentioned in history; but the scorbutic dysentery soon succeeding scurvy, shows us as certainly as if the diet lists were before us, that hard salt beef and biscuit formed the miserable allowance, scarce deserving the name of food, which was issued to these men."

Nor may we suppose that modern times have excluded the most recognised and most glaring of these causes from operation. In the two first Caffre wars (p. 4) a large proportion of the men became scorbutic. Foregoing the advantages of civilization, it may be said that we brought ourselves to the level of savage life, and fought with blunted weapons and limbs that were maimed. A yet more modern instance lies before us.

> "In the first year of the Crimean war the diet was so insufficient, that any one accustomed to the subject would have been able at once, on being informed of the amount, to foresee the inevitable result; yet it was deemed sufficient to support the strength of men in the most trying and exhausting of wars."

From the avowal of M'Cleod, in his history of the surgery of that war, "scurvy influenced and complicated every wound . . . . it wrested more men from us than the conical ball." Again, he says, "It is useless now to inquire why that store of lime-juice which is proved to have lain at Balaklava during two months was not issued to the longing troops." We rejoice at his exoneration of our profession, but cannot acquiesce in so important a matter being wholly laid at the door of a subordinate department—the commissariat. It was but the story of Carthagena over again. The coast of the Black Sea offered abundance of green food and fruit, with wood for fuel, convenient to the use of those who would fetch it; "but perhaps," as Smollett has said, "the general was too much of a gentleman to ask a favour of this kind from his fellow-chief, who, on the other hand, would not derogate so much from his dignity as to offer such assistance unasked."

From such causes mainly, in the most glorious of enterprises, indeed, but in an early part of the Crimean war, perished that small, compact, and model army, of whose composition we may aver that, as regards manhood and perfection of form, the eye rests on nothing like them in the present day.

"Troops well cared for," according to our author, "*must* be healthy;" but the inveteracy with which these habits of mere neglect recur, lead us to fancy we hold in the sheets now before us some chapter torn from a treatise on human nature, intended to display its recklessness and heedlessness alike. Out of many examples, we shall pause a moment over the recent one of that Burmese war which occurred rather less than forty years ago:

> "In both cases," says our author, speaking of the Burmese expedition in 1824, and of the China war in 1840, "the cause of the immense mortality which ensued, or by far the most potent cause, was the food which was issued to the men. In both cases it was thought that men could be maintained, not only in health but in fighting condition, upon diet so bad that *no slaveholder in any part of the world, or in any age of the world, would have given it to his slaves,*

Wretched cattle, hastily purchased and driven to Calcutta, were there hastily salted; and on this wretched meat, and almost as wretched biscuit, the troops were kept, when within a few days' sail there was a land of wheat and of rice and of fresh vegetables without end.

"To the immense mortality in the Burmese war (1824) I need scarcely refer. In three or four months some of the regiments lost half their strength; in eleven months, 1·311 men died out of 2·716. The 13th Foot lost by disease 341 out of 608 men, or 56 per cent. So general was the scurvy that the surgeons were in the habit of examining the gums of the men before a skirmish to prevent any of the men having symptoms of the disease from advancing, as wounds received in that condition of body are most intractable. Malignant malarious fever and scorbutic dysentery were the great agents of destruction."

When one reads such things, it matters little to ask whether the disaster of Carthagena was in this or yonder century, in the past or yet to come. Details as we are now considering them seldom or never reach the general reader in such a form as to interest the nation; it is enough if in the history of a distant war, recounted in all likelihood by some officer of the service, a hint here and there occurs of fever and cholera.[1] Dysentery is invariably attributed to over-indulgence in fruit; short supplies and deficient commissariat conveyance are slightly mentioned; the fighting is all in all. The period, however, is not so far distant but that it survives in the traditions of the service; can it be true that some of the beef served out to the troops in Burmah had been in store since the Java campaign in 1811? There could then be but one opinion as to its nutritious qualities.[2] What is most certain is, that it was nauseated and kicked about by the men. We remember hearing from the lips of one officer of his regiment being in such a state that a double dram had to be served out before it could be marched less than a mile from Rangoon to the commissariat store.

"Twenty-six years later, in the expedition to China, the same tragedy was repeated without variation. The history of the Cameronians may be taken as a type of the fate of the whole force, although the amount of sickness was greater than that which prevailed in some other regiments. The Cameronians landed at Chusan, a splendid body of men 900 strong, on the 5th July, 1840. In the first week in August 500 were in hospital; towards the end of August less than 100 mustered on parade. Later in the season the *débris* of the regiment, under 200 men, were sent to Manilla to recruit, and of them but a fraction ever saw their colours again. The bloodiest battle would have been mockery to this. Doubtless they were to a certain extent in a malarious country, but the malaria was not sufficiently intense to cause so great a loss. Again the simple cause is to be found in the diet."[3]

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[1] Snodgrass' Burmese War.

[2] The head of the commissariat is, we believe, liable in money for the goods in store.

[3] During the last few months, facts have been prominently brought forward, which display in a striking degree the prophylactic influence of a sound dietary against the poison of malaria. We allude, in the first instance, to the getting in of the harvest in the Roman Campagna under better conditions of diet and shelter, without the usual occurrence of fever, as communicated during the past year to the Royal College of Physicians of London; and, in the next instance, to the circumstance of the total disappearance of ague from Scotland under improved condition of its inhabitants, as remarked upon by Dr. Christison in his address at the last meeting of the Social Science Association.

See also a recent work, Hind's Labrador, regarding the influence of diet in the production of night-blindness.

These instances, as it seems to us, are not without instruction. "As far as diseases are concerned," says our author, "the history of all our wars presents a remarkable sameness." It is to be apprehended that a tendency to "sameness," such as prevails in all organized bodies of men and nations, and nowhere so much as in armies, has occurred side by side with disaster, and stands even now at a point to require correction and vigilance to neutralize its consequences. A remarkable "sameness" certainly displays itself in the military character of the present epoch; our author does not fear to say, that—

"In the wars toward the end of the last century, and up to the long peace, the commissariat arrangements appear to have been inferior to those of eighty and sixty years before."

In our own days the last China war alone formed a brilliant exception. Under the guidance of good principles we might look for a yet greater improvement, or at least with equal precautions to an equal result.

"The first rule," says our author, "is to provide a diet in the highest degree strengthening and nutritious. It may seem an expensive matter to the state to provide a diet sufficiently *varied*, but it is not so."

The Great Frederick, in his published instruction to his generals, quoting another's words, says: "The first object in the establishment of an army ought to be making a provision for the belly;[1] that being the basis and foundation of all operations." His work includes many considerations for the health of troops. Directions are not only given for provisioning men, but horses too; he is careful that every company should be provided with a handmill; while he recommends brewers as objects of particular regard in the captured towns. So before the Russian campaign, under Napoleon I., the greatest interest was displayed by him in particulars as to the nourishment of his troops; these things are, however, much subject to failure, not only from the colossal scale of similar operations, but very much from a want of correlative intelligence and foresight in subordinate commands. Now, the leading idea, not too boldly expressed, in these pages, seems to us to be that the military art, or military economy, as regards an exact attention to hygienic principles, is really not far in advance of earlier ages. Habit tends to make us believe the increasing current of modern improvement to be such, that each art, by an inherent force, improves with advancing civilization, and perfects itself somewhat independently of the virtues and defects of those who practise it. Hygienic principles in war, under whatever denomination, have, however, always been of such extreme urgency, and so intimately connected with success, that every commander of genius and resource, whether in ancient or modern times, must have learnt them from example or precept, as well as from experience; and such themes must always have found a place in camp tradition, and in the memories of veteran soldiers; the art of war during the middle ages, and perhaps generally in ancient times, formed more exclusively than now the subject of conversation at home and at the convivial gathering; in our day, observation is directed to a more varied field; commerce, literature, science, fine arts, have distracted attention from the once absorbing topic of war, and it is not too much to say, that in modern civilized societies the tendency is to make that the property of a caste which, in fact, is highly wrought into the interest of all.

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[1] It was a favourite saying of Frederick, that "an army moved upon its stomach." It is paralleled by the reflection of Napoleon, that "the belly ruled the world;" and by the Arabic proverb, "The master of the caldron is master of the sword." The author of 'Anastasius' informs us, that in the battles between the Austrians and the Turks the great struggle was always round the large copper cooking-vessels of the latter, which the under-fed Austrian was as greedy to seize upon as the well-fed heathen was strenuous to defend. The argument for good diet was pithily put to us by a French soldier in these words: "Tel on vit, tel on est."

But of all causes which in later days have influenced the soldiers' health, none we believe have been of extent equal to the personal character of those leading spirits who mould the age in part or whole to their faculties. The bias of imitation is so much more considerable in man than the reasoning principle, that every age and people fluctuates in cycles of national character according as such prominent persons influence it. The wry neck of Alexander, and the round back of Napoleon, have found each in their time and nation an abundance of servile imitators. The latter of these men, though not naturally cruel in disposition, became so in act and deed through the operation of a fallacious philosophy. Neither he nor the Great Frederick ever fairly understood the sanctity of human life; with them it was very much a pawn to play with, a unit on the board; the exterior of the soldier under Napoleon was made flashy and imposing, but the constitutional vigour of his frame was ill-maintained; moreover, this spirit in the chief very largely pervaded the officers under his command. Wellington was not a man who, during his military career at least, cultivated or nourished to a marked extent the gentler and kindlier features of humanity. Moreover, in his day, the times were quick and urgent, and he, as their single prop,[1] was absorbed in the main result. A certain hardness and inflexibility of character, with a calculating head rather than a personal heroism; a hardness wrought, as it appears in some respects, into obtuseness, by a somewhat faulty and exceptional early training, left him in most essentials sagacious; in nicer points it is to be feared he was ever fated to be un-enlightened. These men were without an exact parallel in former times. It is open to conjecture that there may be several bad copies of them now. There is nothing, we may add, that more detracts from the character of the present ruler of the French than, with many blandishments and indulgences, the imperfect hygiene of the soldiers under his rule.[2]

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[1] We can find no other explanation or excuse for Pitt, whose neglect of the seamen's complaints previous to the mutiny at the Nore is properly characterized as a gross political blunder.

[2] One visit to a French barrack will confirm this. It has been reserved for the ready perception and simple conviction of an admirable woman to urge this matter with more propriety and force than anyone else could employ upon the attention of her consort. We refer to the Empress's inspired letter, or telegram, from Cadiz, in October last, *a propos* of a French transport with troops for Mexico, which had touched there. "Sire, on traite vos soldats pire que les nègres." As we write, we rejoice to hear that the wretched barrack accommodation at Avignon is at last undergoing change, with several particulars of improvement.

Notwithstanding the misconceptions to which modern historians have lent themselves, by which, however, it is pleasing to observe how few have been misled, we have only to turn to the pages of our Robert Jackson to see how different was the cast of character in an earlier time.

"Marlborough," says this prince of army surgeons, "was modest and humble, religious without ostentation, and full of sensibility to the human species."[1]

If a commander is like this, the subalterns take their cue from his behaviour. Humanity gives price to valour, which, as a single quality, is often the property of the brute. As regards Turenne, we know his troops named him by the tender epithet of father.

Partly to convince those who are too sickly in love with modern times and dates, we will quote from our author some of the details of the wars of Marlborough, and those soon after him :

> "In the wars of Marlborough, those in Flanders and Germany, in 1742 and in 1760, the men were better fed than in many later campaigns; salt meat seems happily to have been little used. The colonels of the regiments appear to have been the chief purveyors: each colonel contracted with butchers, who drove with the army herds both of sheep and oxen for slaughter. Fresh meat, at any rate, was thus procured, and we know from the writings of Donald Munro, that in 1760, the army surgeons strongly insisted on the issue of bread and fresh vegetables; fruits also seem largely used, and in this way the ravages of scurvy appear to have been almost prevented. The wars in Flanders in 1742, and in Germany in 1760, have been very carefully recorded; perhaps more so than any other war, with the exception of the Russian war of 1854-5. In both these wars, at certain periods, the men were exposed greatly to inclemencies of weather. It was then seen, that if men are well fed and can be kept dry, they can bear great cold. The winter of 1742 was extremely severe, and in April, when the troops commenced their march, there were extraordinary snows for seventeen days. The troops marched through these storms, but were every night received into warm houses. Out of the 16,000 men, not twenty were lost. Again, in the German war in 1760, some regiments made a winter campaign on the borders of the Lower Rhine; they were exposed to great inclemencies of weather, to great hardships, and to extreme cold, yet they were very healthy, much more so than the troops left in the fixed camp at Warburg, who, it may be supposed, must have been in the possession of much greater comforts. This was owing to their good food and good clothing. At that time, 1760, every soldier wore a *flannel waistcoat*, a custom which has now unfortunately disappeared. This custom was commenced by gifts from the Quakers to the army, 1745-6, and it was found to be attended with the greatest possible benefit. In 1760, the Government issued warm clothing of this description, and in addition there was a very large private subscription in England, and blankets, greatcoats, underclothing, shoes, stockings, &c., were given to the men. The men's blankets were carried on horses, and were wrapped in waterproof clothing. Each company had its own horses, which kept up with the men on the march."

Here we have, in effect, what is far from carried out in the present day—viz., the underclothing of the soldier carefully attended to, one of the most important particulars of his personal economy. This, with bodily cleanliness, is of more real importance than personal appearance on parade. In combating the effects of cold, the experience of the American War of Independence is appealed to, to show that the daily use of spirits is always hurtful, and how material hot liquids are to the comfort and health of the soldier:[1]

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[1] Jackson on Armies.

> "The same fact, that men can bear great exposure to cold if properly fed and clothed, was established in the American War of Independence. Some of the Rangers were out during the winter, and escaped entirely the diseases produced by cold. They attributed their immunity in a great measure to the use of hot ginger tea. Every man carried a piece of ginger in his pocket, and would on no account be without it. With this they made hot tea, and they found this much more comforting than spirits, which appear to have been in a great measure disused among them. Hot infusions of garlic and infusions of horseradish were also used for this purpose, until the more common employment of hot tea and coffee supplied us with the means which may, perhaps, be considered even better adapted to protect the body against exposure to cold."

The yearly pressure of a blind economy enforced upon Administrations, and a want of burning sympathy of men of influence from the army ranks, have combined to leave the "condition of the soldier" question so much in arrear, that above all other questions it seems clogged with unaccountable difficulty in retrieving ground that has been lost; the inferior and disadvantageous condition of the soldier, received into acceptance by power of custom, is strangely now at variance with the military adage "that every soldier is a gentleman."

From matter, all of which is of the highest value, we shall select two points for observation and remark. One of them addresses itself to commanders of forces; the second to governments. The first regards the site of malarious and infected localities; the second concerns the enlistment of men. In treating of the malarious districts, our author mentions the fact, in comments on the Walcheren expedition, that as early as the year 1747 Sir John Pringle had already described the unhealthiness of Walcheren and South Beveland. So great was the sickness that in many corps six-sevenths of the men were in hospital at that time. Our author resumes:

> "This ought to have ever prevented the expedition in August, 1809, from being undertaken; but, as if it was not warning enough, we have it mentioned that the English force, in 1809, found a Dutch regiment there which in three years lost 715 out of 800 men; this was the type of its own fate. On the 14th of September, seven weeks after leaving England, out of 15,000 men 10,000 were in hospital."

We also find an instance recorded of a regiment encamping, against medical advice, on the marshy grounds of the Pedee river in America during the War of Independence. Caught as in a trap, they could not, from mere feebleness, remove their sick. Those of the men who were strong enough to leave the banks of the river quickly improved on the march, while "of those embarked in boats few were heard of again."

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[1] Severe and prolonged cold subjects the frame to disease, and we might say to pestilence, in a degree perhaps equal to extremes of heat; when rigorous, it is far from having that strengthening effect upon the frame that is commonly supposed. In the terrible cold of the expedition to North Holland, in 1799, when the cold was more intense than in the Russian campaign in 1812, the contagious fever was accompanied by maniacal symptoms resembling those described in the plague of Athens. A severe winter frequently precedes the Oriental plague. Thee is no more fatal mistake than the constant strain in the endeavouring to harden the soldier, who, as a rule, has far less vital power than his officer.

As regards old camping-grounds the following: In the expedition to Egypt from India, in 1801, the army, after excessive hardships amid burning heat, reached the Nile. They descended that river for four hundred miles, and landed at Ghiza.

> "They found there the 89th Regiment very sickly; scarcely fifty men mustered on parade. This should have been considered the touchstone by which the sanitary condition of Ghiza was to be judged. However, the army was there disembarked, being then, to use the words of Sir James McGrigor, 'uncommonly healthy.' In less than a week they sent into the hospital ten per cent. of their force; in three weeks there were a thousand sick out of the eight thousand men; in four weeks there were twelve hundred sick; then the army moved to Rosetta. We must therefore consider that no less than fifteen per cent. of that force had in that short space of time been in the hospital, and one-fourth of the duty-men, in all probability, must have passed through the hospital. The diseases produced in this short time were attended with very slight mortality. They were chiefly fevers which appear not to have been of malarious origin, but chiefly of that kind—yet little investigated or understood—which are known by the name of the bilious remittent or the bilious typhoïd, and the bilious relapsing fevers of the Mediterranean and Egypt. There were also slight dysenteries and some ophthalmia, but no plague till afterwards, when the army got to Rosetta. It was supposed at one time that the diseases produced at Ghiza were owing to the marshes in the neighbourhood; but this is rendered improbable—first, from the fact that, as far as can be known from the accounts of the diseases, which are very short and imperfect, the malarious taint did not form any grand element in these diseases; and secondly, from the following fact—Ghiza for months had been an encamping-ground of a succession of bodies of troops, Turks, Mamelukes, French, and then English. The whole country was covered with putrid effluvia. At a subsequent period, when time had been given for the dissipation and complete decomposition and elimination of all these putrid remains, Ghiza was again occupied, and was found to be healthy; therefore it would appear certain that the condition of the camp was the cause which led to the great amount of sickness in the expeditionary force."

We shall now refer to a second vital error which is of singular persistence in our nation. We allude to those sudden enlistments which, under some impatience of circumstances not duly provided for, have been had recourse to, against all the teachings of experience, each time much upon the same plan.

This fault is only stigmatized in a note appended to our author's text, and perhaps on that account we shall give to it more prominence.[1] The dirty famine-struck recruit is indeed of doubtful value on his own account, and he is often a source of contagious disorder to the soldiers.[2] A consideration of the nature of the contagious fevers which affected the British troops about the close of the last and early part of this century show them to have partaken largely of the nature of synocha, or what is now called relapsing or famine fever. They seemingly flowed from this source of enlistment and spread widely and persistently. The plan of allowing candidates for commissions in the army to scrape together a set of boyish raggamuffins[1] has repeatedly been had recourse to, and even as late as the Crimean war. We will quote what Robert Jackson has said on this subject:[2]

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[1] See account of the British Legion in Spain, p. 12.

[2] Des enfans qui encombrent les routes et les hôpitaux.

> "It was believed, not without evidence, that the seeds of contagious fever were introduced into many regiments of the line in 1795 by recruits from independent companies. Under the operation of a judicious system of economical regulation, the progress of a disease thus introduced might have been checked—the disease itself banished. Good regulations did not exist at that time; the value of a soldier was estimated in a false scale, and from that false estimate measures originated which threatened to disorganize the army. Rank and command were more certainly obtained by the money of a schoolboy and the activity of a crimp than by actual knowledge of military duty and hard-earned experience in the fields of war. Money had purchased one step, and with money another had to be purchased.
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> "The chances were watched with eagerness, and the soldiers which were the subject of the traffic passed from hand to hand with the same indifference as counters at a gaming-table. This practice has fortunately given way to a better order of things. The materials of our army are not only better in their own nature, but the economical regulations are better digested and more rigidly practised than they were at the period alluded to; yet, notwithstanding these advantages, sickness has been sometimes great among the troops on service since the years 1794 and 1795, and with all the light that might have been derived from experience, it is only of recent date that the practice of accumulating the military sick into military hospitals—a *practice more destructive of human life than the most sanguinary battles in the field*—has been annulled, or only resorted to under necessities."

We bring this extract the more willingly under the eyes of our readers, as some tendency has lately been displayed to return to the system of general hospitals or hospital depôts, in the teeth of old experience. The general absence of relapsing fever from England since 1855, and confidence in improved sanitary arrangements, may strengthen the purpose of those advocating such views. The abandonment of the sick depôts in favour of the regimental system is calculated by Jackson to have checked the mortality which was "prodigiously great" in the early part of the Peninsular War. He estimates that this reform alone gave ten thousand firelocks to the general in command, and that the annual saving of human life to the army amounted to not fewer than five thousand souls. "I would not err, perhaps," he says, "if I said one-third more."

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[1] In the armies of the Roman Republic, according to Polybius, the military service was only accorded to those among the citizens who were two grades removed from pauperism. The qualification was in an income of 4000 asses. The proletarians had but 1500. The *a capite censi* were paupers. Any slave who had intruded himself into the ranks of the army, even though honourably scarred, was infallibly punished with death. In the reconstruction society by Charlemagne, the extent of landed possession was taken as the basis of personal military service and contribution. The army was made to consist wholly of proprietors of the soil. In this way the scum of armies, fostered by his predecessors, was purged out.—See Barginet, Hist. du Gouvernement Féodal. If, as we have heard, the "right sort," in our day, is not forthcoming, the cause must be found in the social and moral conditions of the service. Honestas enim idoneum militem reddit.

[2] Jackson on Contagious Fever, 1819.

In short, it turned the tide of victory.

For striking accounts of spotted typhus and other scourges we refer to the author's pages and to those of Dr. Murchison; the survey, though exquisitely painful,[1] is necessary to make one fully apprehend, and in a lively manner, what war actually is.[2] Jackson, in his 'Treatise on Fever,' invokes the attention of statesmen and generals, as well as physicians, who superintend the medical concerns of armies, but laments that the "words of a physician are ordinarily spoken to the winds." He fathers the prevalence of contagious fever in his day "on the carelessness, indifference, error, prejudice, or self-conceit of the great, who will not condescend to be instructed by the humble;" but this is not without exception, and there are names enshrined in his pages which shine bright even now.

> "The 93rd," says Jackson, "was sickly when it arrived at Spike Island. It embarked with a long list of sick, and with presentiment of increase; but the sickness, instead of increasing, diminished during detention in the harbour, and more remarkably still on the passage to Barbadoes. I do not assert positively that this improvement arose from the management of the officer who was entrusted with the command; but, whatever the actual cause of the improvement, the unwearied diligence and affectionate care of Lieut.-Colonel Gamnel to the concerns of the soldier, on every occasion where his conduct fell under my notice, as they impressed me with a high respect for his character at the time, so they have left with me a strong feeling of veneration for his memory. Besides the 92rd, the 29th Light Dragoons arrived at Cove in a sickly state. The disease prevailed in both the transports in which the corps was embarked at the time it arrived at the rendezvous at Cove: it was banished from the one in which the commanding officer, Lieut.-Colonel Hay himself, was; it continued in the other. The fact is striking. I was disposed to ascribe it at the time to the judicious care and attention of Lieut.-Colonel Hay, who was then esteemed, and apparently with good cause, a man of sound judgment and superior knowledge in matters which relate to military economy."

On the whole, it may be considered probable that from the superior thoughtfulness of individual character in our nation, and it is to be hoped from the predominance of a reigning humanity founded on Christian principles (though we would not be thought to ignore their influence in other countries), our armies have been in a less degree than those of foreign states subject to the results of hygienic neglect. Such errors as those of Carthagena, and Walcheren, and the early Crimean campaign, have been followed by a quick rebound of the national heart; and the military administration have repeatedly found it necessary to render an account of its past proceedings to the nation in minute particulars of regulation and discipline.

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[1] According to Dr. Murchison, one half of the French in the Crimea attacked with typhus fever died. See his work on Fever, p. 225.

[2] We may remark of spotted typhus, the type of European pestilence in the present epoch, that it is so allied to true plague that, in the history of that disease, it is found commonly to precede it; and this also becomes spotted typhus in its decline. By the ablest physicians they are scarcely distinguishable, and only by that fugitive symptom the bubo; they are birds of the same nest, and only not inseparable. Fortunately for mankind, the co-operation of several causes seems necessary to create plague. What strikes the medical reader with some surprise is, that fevers of such extent and intensity as our author describes, in no case passed into true plague. The cases of spotted fever lately noticed at Philadelphia seem clearly referable to an army source, and, though few, point to a painful future. See American Quarterly, July, 1863.

To what purpose, it may be asked, do we ransack history for elucidation of truths which are plain and incontrovertible? It is that they require to be received in their fullest force; it is the so-called practical men,—by which we mean men punctual in duty, but little fertile in expedient, less wealthy in mental gifts—that we must accuse of maldirection in great operations of the field. In vain shall the medical officer seek to make an impression on the chief whose mental gifts are those of a *sabreur;* electric in the onset, rigid in discipline, but afterwards unprepared to cope with the slow occasions of destructive decay or with the assaults of malarious and zymotic poison.

> "Previously to the battle of Dettingen," says our author, "the men had been extremely healthy, after the battle they were exposed to wet and cold for two or three days; the consequence was that an attack of dysentery occurred, and was so general that half the army was affected by it. Had this occurred a few days previously, it is by no means improbable that the strenuous exertions which alone won the battle of Dettingen would have been impossible."

The enumeration of causes and of remedial agencies, and the multiplication of instances, though worthy of note, are far from being all that is required for the instruction of one in command. To secure the soldier from deprivation, and from perishing or suffering through inclemencies of the air and seasons, requires at once vigilance and contrivance. Military hygiene, except in its main features, is not invariably a simple affair.[1] In our nation alone, under imperfect moral teaching, the term genius[2] has tended to become synonymous with eccentricity; and ability or adaptability displayed in various things is rather counted as disabling for trust in service, social and public; an enlarged capacity, however, and that fertility in resource which is fed by observation and comparison, is precisely what we hope for and what we require in the conduct of our future campaigns; the variety of operations incident to our service renders it the most suggestive of any; it is not impossible that some variation of type in military character may react upon the traditions of the early part of the century. It is, we hope, no treason to say that in the latter end of the wars of Napoleon and Wellington a class of men came to the front in whom a smart attention to technical detail and flattering alertness in obedience superseded all speculative and all intelligent conception of duty.[1] Hence the superior considerations of health and subsistence fell into abeyance, or into a second line of importance. It is with a conviction that a due attention to hygienic laws will stamp a new era on our country's greatness in arms, and supply very much to make our strength secure, that we instance that fulness of resource and freedom from class prejudice which comes of reading, conversation, and the searching into many things, as being most necessary to the accomplished commander of troops. The explanation of his disasters are otherwise likely to appear strange to him, as in the extract before us :—

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[1] The first quality of a commander, says Marshal Saxe, is courage; the second is genius, which should be strong and fertile in expedients; the third is health. 'Reveries and Memories,' by Marshal Saxe.

War is a trade for the ignorant, and a science for men of genius. Chevalier Folard, Sd.

[2] We may also affirm that military hygiene is a progressive science.

> "And yet, when asked by the officers in command what extraordinary cause could be assigned for an amount of sickness and mortality truly alarming, something of incredulity has stolen over the countenances of my hearers while the natural causes above described were detailed; so little are we inclined to believe that great effects can be produced by the action of common causes."

The commander should be so far advanced in the study of the laws of health as in some sort to associate himself with the physician in the cares of his charge. There will then be no jealousies either from head-quarters or in joint operations, and the character of the medical officer in proportion to his earnestness and ability, will carry more weight with it than it does now.[2]

We shall press this subject closer by bringing to recollection so humble an instance as that of Captain Cook : a familiar exemplar in the memory of our people. Emerging from a letterless boyhood, this man acquired repute as astronomer, mariner, and geographer; he was useful and clever in medical matters, and we may add not unskilled as an experimenter; we may add, also, as botanist and brewer. It is strange how a little knowledge thrives with some and rusts in the hands of many. The fatal sickness which seized on his crew in the Indian Archipelago, in returning from his first voyage round the world, opened Cook's mind to the importance of the sea scurvy—a disease then held to be incurable; and from that time he studied it with attention, until he had arrived at a masterly treatment of the disease. A better proof of which could not be given than the fact that in his second voyage, out of two crews, amounting to 197 men, during an absence of three years he lost but one man by sickness. His stores of "salted cabbage, sour crout, marmalade of carrots, portable soup, inspissated juice of wort and beer, mustard, etc.," do not alone account for this. Attention to nice points of hygiene and to physiological views combined to give the result. It is not an ordinary man who would have set his sailors to work in New Zealand to brew beer from a tree resembling the American spruce, knowing that with his inspissated juice of wort and molasses it would make a very wholesome beverage, and correct the deficiency of esculent vegetables, of which the country appeared to be destitute.

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[1] It is well known that the latter part of Frederick the Great's rule favoured the same cast of character, and, by strangling thought and reflection, acted perniciously on the spirit of the times, and on the fortunes of his nation. This was amply experienced in the Prussian army, whose tactics we were then borrowing, and at the time something of that spirit found its way over here.

[2] We may instance Major-General Nott as an officer who, without any brilliant fortune in the field, owed a most triumphant success to the skill and foresight with which he conducted his commissariat arrangements. We must remember, also, that he knew how to exempt a conscientious medical officer in a censure which included all the other officers of a regiment. See Life of Nott.

With such ingenious minds, association affords the highest degree of pleasure; and let us insist that it was in no small measure by availing himself of the intelligence of our profession that he was able to perfect his views of hygiene.

In the present day and time we are so far still in fæcê Romuli, that we may say with an author already quoted, when lamenting that hospital mortality does not occur so much from disease in its own simple nature as from artificial aggravations through misapprehensions of causes and conditions—

> "It is not difficult for the physician to say what may be done for prevention; but he can rarely do more than say that he has not authority to act. The medical art is a science, but it is a science of difficult attainment. If attained, it is perhaps only in the evening of a long life spent in the daily search of it; and as not of easy discovery, it cannot well be supposed that a commissary of war or a military commandant of hospitals should be possessed of it. Yet to such the health-concerns of armies, and the control of army medical officers, appear to be committed, in most of the military services of the greater Powers of Europe."

In the British service, though there is much of the old spirit remaining, much that is retrograde in tendency, we may allow that the description only generally applies. At the same time, we will not disguise from ourselves that it is to the superior spirit of the nobles and privileged of our land that our profession has to look for support in the ranks of the army. The value set upon us by the military class will ever be too low. The soldier is too much in the habit of referring all things to himself, and the gentler arts and virtues seldom grow kindly under his dictation.

In our reflections on the sad results of deficiencies in military hygiene, we would not lead it to be inferred that the best advice, offered with the most steady, uncompromising courage, has invariably been at the service of our commanders. Imperfect views may occasionally intervene; there may be a relapse from the lessons of experience in the minds of our profession. If Jackson's instructions in his chapter on the use of transports had been fully borne in mind, troops affected with cholera would hardly have been sent out to sea for a change, as they were from Varna. On its coming on to blow, and the hatches fastened down, death dealt his blows doubly and trebly quick. This pained us at the time, and has dwelt on our memory till now. We shall never fail honestly to lay open to view any principle or practice prejudicial to humanity. The attention that has lately been given to these subjects, supported by two most worthy names, has not yet died out. Born of urgent circumstances, it has displayed as yet more of the instability of fashion than of the robustness of enduring strength. We may point to the publication of the 'Minutes of the Commission for investigating the Sanitary Condition of the Troops in India' (a notice of which will be found in our last number, p. 181), one of the most favourable steps in this direction on which we have to congratulate ourselves, while it has revealed a source of weakness to our state and a scandal to our civilization which is hardly credible.[1]

It is almost distinctive of that field of disease which associates itself with war, that it is but in a minor degree subject to operations of cure, preventive means constituting the greater part of the science. This leads directly to the consideration whether ancient civilization was at any time more advanced than ours in precaution against such disasters. Our author seems to be of the opinion that such was the case. He makes, indeed, an *argumentum ad verecundiam* to the Horse Guards, founded on the immunity which the Romans enjoyed in their vast undertakings. With us every war, however glorious in effort and success, is pregnant of lessons in camp disorder.[2] In their history it is quite different. Notices of a similar kind are most infrequent. None would be more averse than ourselves to place this subject on a false basis. The inhumanity of man to man in the heathen world is what we have practically little power of conceiving in the present day.[3] Indeed, we read marvellously little of the wounded in the relation of battles in ancient history. We can just now remember it but once in Arrian, where Alexander is mentioned as visiting the wounded after the battle of Arbela, although hurt in the thigh himself.[4] This conqueror is said to have owed the health of his troops in great measure to changing his camp frequently. His great losses in returning from the Indus to Persia were due to excessive heat and want of water. But to resume, the comparison of the ancient with modern times is full of instruction, and it affords a great lesson in the startling contrast shown between commercial states and those of an original military bent. In one case, we meet with the constant recurrence of camp disease; with the others it seems very rare, and especially so with the Romans.

To treat of Greek history, in the first place, we pass over the siege of Troy, and shall first quote Herodotus, viii. chap. 115, to show that the Persian army, in its return from the invasion of Greece, was seized with pestilence and dysentery. We must note the pestilence of Athens, when all the city became a crowded camp under the external pressure of war. This is described by Hippocrates and Thucydides. The latter relates how (probably hoping to relieve the pressure of population) an expedition of 4000 men was despatched to assist in the siege of Potidæa, under the command of Hagnon, and that they carried the infection with them, to the great detriment of those engaged in the siege; so that Hagnon was compelled to re-embark after forty days with a loss of 1700 men of the troops he had brought to Potidæa.

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[1] In a few words, it may be stated that we hold India on the terms of losing five British regiments by sickness there annually, at the lowest computation.

We would not lead it to be inferred, however, that reports any more than reviews are sure guarantees for progress and reform. They at least may constitute some of the conditions of each. There are responsibilities other than those of public writers; and commissioners who never yet, according to our experience, have played more than a subordinate part in effecting social change.

[2] The sudden conquest of the Havannah in the early part of George the Third's reign was the most lucrative, as well as the most heroic of enterprises; its plunder equalled national subsidy. During the siege the want of water, and total want of good provisions, caused disease among our troops to rage like a fearful pestilence. There perished Bathurst, the much-loved physician, Johnson's co-partner in the 'Adventurer,' and its editor. "May my country," wrote Johnson, "never be cursed by such another conquest."

[3] See Niebuhr's Lectures on the History of Rome. Lecture 13. Vol. iv.

[4] Arrian's History, chap. xii.

In Rome, also, one at least of their early pestilences[1] (291 U.C.) was due to the accumulation of men in the city under the ravages of the enemy, with the aggravation of much cattle collected there, which at least in the pestilence of Athens they were free from, the Athenians having sent their cattle to Eubœa and the islands. In neither of these instances did the pestilence extend to the neighbouring country. Under such circumstances we may consider these cities as unhealthy camps.[2] But generally we will affirm that as regards ancient history the chief sanitary interest centres in Syracuse and its sieges. There the Carthaginians especially were most unfortunate, and not there alone. Carthage, it is to be remembered, is that state whose civil constitution, according to the opinion of Aristotle, was the best in existence, and as near as possible to perfection. In the year 395 B.C. the Carthaginians were engaged with their allies in the siege of Syracuse, which alone remained to be subdued of all Sicily. We shall relate what befel their army:

> "But as to the Carthaginians after they had ruined the suburbs and rifled and plundered the temples of Ceres and Proserpine, a plague seized upon their army; and the more to increase and sharpen the vengeance of the gods upon them, both the time of the year and the multitude of men thronging together greatly contributed to the enhancement and aggravation of their misery, for the summer was hotter than ordinary, and the place itself was the great occasion that the distemper raged above all bounds; for the Athenians in that *very same place*, not long before, were in multitudes swept away by the plague, for it was a marshy and spungy ground. In the beginning of the distemper, before the sun rose through the coldness of the air that came off from the water, their bodies would fall a shaking and a trembling; but about noon, being so closely pent up together, they were choked with the heat. The infection was brought

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[1] Those great pestilences which have formed eras in the world's history we will not, by a forced construction, attribute to human agency, although doubtless, in some degree subject to alleviation from human aid and sympathy. These we entirely attribute to cosmical influences. Such afflicted the Roman State both during the Republic and the Empire, and have occurred to as great an extent during the Christian era. There are parts of the world which, from volcanic or other earth-born influences, seem ever smouldering with disease.

[2] The conditions which are common to a town and an encampment cannot be too often urged on the military man. The same laws of hygiene apply to both. By studying causes as they tell upon home life and city life, he will best learn to deal with disease in campaigns. Every cause of blood-deterioration lays the frame open to epidemic disorder; and once it is established, the most robust are often those who become the subjects of contagion the earliest and most frequently.

in among them by the south wind, which swept them away in heaps, and for a while they buried them; but the number of dead increasing to that degree that those who attended the sick were likewise cut off, none durst approach the infected, and (besides the want of attendance) the distemper seemed to be incurable. For, first, catarrhs and swellings of the throat were caused by the stench of the bodies that lay unburied, and the putrefaction of the soil. Then followed fevers, pains in the back, heaviness in the loins, dysenteries, and blotches and boils over all the body. Thus many were tormented by this plague; others were struck mad, and ran about the camp like wild beasts and beat every one they met. All the help of the physicians was vain, both by the violence of the distemper and the sudden despatch it made of many; for in the midst of great pains and horrible torments they died commonly on the *fifth* or at most the *sixth* day, so that they who died by the war were accounted happy by all; and it was further observable that all that attended on the sick died of the same distemper; and that which aggravated the misery was that none were willing to come near to the distressed and languishing persons in order to administer to them any sort of help; for not only strangers, but even brothers and dear and familiar friends and acquaintances, were forced for fear of the affection to avoid and forsake one another."[1]

The Carthaginians are said to have been witnesses of the carcasses of 150,000 of their own men dead of the plague. This loss was rapidly followed by a crisis in their affairs, from which the state and city were saved as by a miracle. And thus, the historian adds, "was a great turn and change in Carthaginian affairs, by which one may learn that whoever they be that beyond measure exalt themselves, may come in a short time to be convinced how weak and inconsiderable they are."

We shall now borrow from Livy[2] his account of the siege of Syracuse by the Romans under Marcellus, when it was defended by a Carthaginian fleet and army, 212 B.C.:

"It was autumn, and the places where they lay (both armies) were in their nature unwholesome, but much more so on the outside of the city than within, and the heat was so intense as to impair the health of almost every person in both the camps. At first the insalubrity of the season and soil produced both sicknesses and death; afterwards, the attendance on the sick and the handling of them spread the contagion far and wide, insomuch that all who were seized by it either died neglected and forsaken, or also infecting such as ventured to take care of them, these were carried off also. Scarcely anything was seen but funerals, and both day and night lamentations from every side rang in their ears. At last, habituated to these scenes of woe, they contracted such savageness, that so far from attending the deceased with tears and sorrowings they would not even carry them out and inter them, so that they lay scattered over the ground in the view of all, and who were in constant expectation of a similar fate. Thus the dead contributed to the destruction of the sick, and the sick to that of the healthy by the apprehensions which they excited, and by the noisome stench of their bodies, whilst some, wishing rather to die by the sword, singly assailed the enemy's posts. But the distemper raged with much greater fury in the Carthaginian camp than in that of the Romans, for the latter by lying so long before Syracuse were become more hardened against the air and the rains. Of the enemy's troops the Sicilians, as soon as they saw that the spreading of the distemper was owing to an unhealthy situation, left it and retired to the several cities in the neighbourhood which were of their party; but the Carthaginians, who had no place of retreat, together with their commanders Hippocrates and Himilco, *perished to a man*. Marcellus, when he perceived the violence of the disorder increasing, had removed his troops into the city, where, being comfortably lodged and sheltered from the inclemency of the air, their impaired constitutions were soon restored; nevertheless great numbers of Roman soldiers were swept away by this pestilence."

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[1] Diodorus Siculus, xiv. 70.

[2] Book xxv. chap. 25.

Not only in Sicily, but in Spain and in Africa, do we read of pestilence afflicting the armies of the Carthaginian state; and it is singular what infrequent mention of it occurs in the annals of the standing armies of the Roman republic. If in the early days of the Republic the Roman soldier was badly fed, he seems at least always to have had wheat,[1] and we know, after the second Punic War, a meat ration. In later times he was both well paid and fed. With the Romans, we are informed, the military position was secondary to perfection and regularity of their encampment. There were ædiles of the camp, and cleanliness was enforced by severest discipline. We are ignorant of their medical appointments, but under the Empire, and later, we know that the professors of medicine had a social rank even above what obtains in the present day.

The army sent by Augustus Cæsar to the conquest and exploration of Arabia affords the fullest instance of loss and suffering to Roman troops from causes other than those of conflict, and we shall remark upon it on that account. The general, Ælius Gallus, seems to have laid his plans badly in the first instance, and was all along under the influence of a traitorous guidance. He had to operate through a difficult, unknown country never before or since subdued, with dry parching heat and great want of water, which made itself felt to a great extent (as it had sometimes to be carried on the backs of camels), and interfered greatly with his progress and success; consequently his losses were immense. He was obliged to stay an entire summer and winter at Leuce Come, in A. Nabatæa, to recruit his troops from disease. This we may conjecture, from the words of Strabo, consisted mainly of the nature of scurvy, though described by him as peculiar to the East. Count de Joinville, in later times, attributed the same disease to Eastern influences, while with us, in Western Europe, it has always been considered a disease of the North.[2]

The names which Strabo has transmitted to us, of diseases in these troops, are stomacacce, a disease affecting the mouth, and sclerotyrbe, a kind of paralysis, according to him, in the legs, attributed to the water and the plants partaken of by the soldiers as their cause. The notices of disease in the annals of the Roman army are unaccountably slight, and deficient in particularity. We may read sometimes of an epidemic and heavy rains impeding operations, as in 518 A.C. against the Gallic tribes beyond the Po. In the campaign of Cæsar against Pompey, mention is made of autumnal fever, which affected largely the troops of the former. It appears to have been the common fever of the south of Italy, in that time of year when, according to the poet, a quartan fever is looked forward to as a "blessing."[1]

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[1] Barley was reserved as a punishment for the Roman soldier, and leguminous food was somewhat abhorrent to him, but occasionally resorted to. A double ration of wheat, or a ration and a half, was an instrument of reward. Hence the words duplicarii, duplares, sesqui-plares, to distinguish soldiers who received them. Papirius Cursor (435 U.C.), a most distinguished general, and evidently a pet of the historian, seems to have been a strong fellow. Livy mentions that he was the best runner of his day, and that he never spared his men. As germane to our matter, we notice that from constitution or practice, he was a famous eater (cibi vinique capacissimus).

[2] In the most northern parts of Europe, besides lupus and leprosy, which linger on the coasts, there still exists a wide-spread and almost general "evil" among the populations, to which it would be difficult to assign any other so comprehensive a term, and which results from bad food, damp and cold operating on successive generations.

In the pages of Pliny scurvy would again appear to be indicated, under the same names as in the account from Strabo. Might we condescend to more modern times, we should find far more particularity in the Comte de Joinville's memoirs of his campaign in Egypt, during the crusades in the 13th century, with Louis IX. of France:

> "You must know," he says, "we eat no flesh the whole of Lent, but eel-pouts, which is a gluttonous fish, and feeds on dead bodies.[2] From this cause and from the bad air of the country, for it rarely rains a drop, the whole army was the subject of a shocking disorder, which dried the flesh of our legs to the bones, and our skins became as black as the ground, or like an old boot that had lain long in a coffer. In addition to this miserable disorder, those affected by it had another sore complaint in the mouth from eating such fish, that rotted the gums and caused a stinking breath. Very few escaped death that were thus attacked, and the surest symptom of its being fatal was a bleeding at the nose; when that took place, none recovered."

This gives us a description of dry gangrene such as might reach our breakfast table any morning in news from the American army through the New York papers, as well as of scurvy in its most virulent form.

In closing this article we may say, if the present happily existing interest attaching to sanitary questions ever rise to the height and breadth of popular diffusion, so that each man should come to know the interest he has in studying and arriving at truth in these questions —the soldier no less than the officer carrying with him the traditions of health, and chiding the neglect of precautions against disease—it will be owing, we think, in the first place, to the liberal and fearless expression of the medical profession, both within and beyond the walls of the service. We have always ourselves been willing, without prejudice to the truth or public necessity, to give an opportunity for such expression—a most necessary benefit to the military profession, in which *esprit du corps* and uniformity of habit is too apt to enclose and narrow that breadth of view which is required to contend with the varying obstinacies of nature, and well calculated, as regards their medical department, to stimulate sagacity and to nourish zeal.

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[1] Sperantibus quartanam. Juv. Sat. iv. 57.

[2] In fact, these fish were taken in the Thanis, a neighbouring canal, in which Christian and paynim bodies, in prodigious numbers, found a ready sepulchre.

REVIEW V.

Traité des Maladies à Urines Albumineuses et Sucrées; ou de l'Albuminurie et du Diabète Sucré dans leurs Rapports avec les Maladies. Par le Docteur J. ABEILLE.—*Paris*, 1863. 8vo.

A Treatise on Diseases attended with Albuminous and Saccharine Urine; or on Albuminuria and Diabetes Mellitus considered in their Relation to Disease. By Dr. J. ABEILLE.

THE passage across the channel of a large French book on Albuminuria and Bright's Disease, and its appearance on the horizon of medical literature, must give rise to a feeling of curiosity and pleasant rivalry in the minds of English medical men, such as would be experienced by the British public generally on beholding a newly-constructed French man-of-war lying in one of our ports. With an inward consciousness that we have hitherto won for ourselves a sort of priority in this field of research, we yet welcome heartily the new-comer, in the full expectation of finding not only a complete elucidation of all that labour and science have previously made known to us, but some advance, some discovery, by which to profit and improve ourselves. The work in question is an elaborate compilation, carefully arranged, and giving evidence of very considerable clinical experience; but, to the German and English reader at least, it must be regarded as an imperfect exponent of this great subject in one most important particular—the morbid anatomy of the kidney, and more especially those minute changes which the microscope alone can reveal.

The question of albuminuria is evidently that which has most occupied the attention of the author; six hundred and forty pages being devoted to its consideration, while only eighty are allotted to the discussion of glycosuria. We therefore propose to examine his views on the former more at length, and to give but a short sketch of the latter. The book opens with a long chapter on the *history* of albuminuria. Fordyce, in the year 1768, seems to have been the first to have noticed coagulable lymph and chyle in the urine. Cruikshank, in 1798, classified all cases of dropsy under two great heads—those with and those without albuminuria. Wells, Blackall, and Howship paved the way for Bright in 1827. Rayer, in 1840, made an important contribution in his work on diseases of the kidney; he was the first to show clearly that albuminuria is present in many diseases in which no lesion of the kidney is detectable. Frerichs, Johnson, and Todd all receive at his hands their due meed of praise. He claims for himself the discovery of albuminuria in hospital gangrene, pyæmia, diphtheria, and all the so-called septic or zymotic diseases. A description of *the anatomy of the healthy kidney* follows, in which he quotes largely the opinions of Isaacs,[1] from whose paper all the woodcuts are copied. In writing of the circulation in the kidney, he adopts, we are glad to see, the arrangement of the bloodvessels which has lately been discovered by Virchow, on which turns a very important point in the morbid anatomy of the kidney. Johnson, and others who have followed him, have accepted the description given by Bowman, and have thus, as seems to us, not correctly interpreted a peculiarly congested condition of the medullary substance repeatedly met with in kidneys whose cortex in one or more of its constituents is in an advanced state of disease. Bowman asserts, that all the arterial blood in the kidney, with the exception of a few branches to the capsule, passes through the Malpighian tufts and the capillaries surrounding the tubules of the cortex, before it leaves the organ through the branches of the renal vein; and that the efferent vessels from the larger Malpighian bodies near the bases of the cones take a course towards the pelvis of the kidney between the straight tubes; or that, in other words, the vasa recta of the medulla carry blood which has already passed through the cortical part of the kidney. This is directly denied by Virchow, who has convinced himself that, at the bases of the pyramids, some of the branches of the renal artery pass up straight to the surface of the kidney, giving off in their course twigs which carry the Malpighian tufts; but that, from other branches which run obliquely through the bases of the pyramids, there spring *branches which, taking first a horizontal course, or describing short curves with their convexity turned upwards, bend suddenly down at a right or even acute angle, and break up after a short course, into tufts* (*Schöpfe*), *which run down between and encompass the lower bundles of the tubes.* Thus is explained how, when through disease either of the bloodvessels or tubules of the cortex, the circulation in that part is obstructed, a kind of collateral flux into the healthy vessels of the medulla must follow, and the pyramid is found gorged and dark-red, offering a curious contrast to the pale cortex. This appears to be more reasonable than the explanation given by Johnson of the phenomenon in question: "That the veins in the medullary cone are gorged probably in consequence of retarded circulation, produced by pressure from the swollen cortical substance lying between the cones." Were this pressure really the cause, it must act equably on all the vessels of the pyramid; and inasmuch as, according to his view of the circulation, the blood passing through the pyramid has already traversed the cortex, the stasis would tend rather in the direction of the cortex, or the blood would be pressed back from the vessels of the pyramid into those of the cortex. Virchow goes so far as to state that albumen can thus be separated from the blood beyond or apart from (*jenseits*) the proper secreting substance of the kidney by transuding through the distended bloodvessels of the pyramid. We have dwelt upon this point somewhat at length, because it is one that has not as yet been sufficiently propagated in England, but which bears in a most interesting manner on the question of albuminuria.

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[1] Trans. of New York Acad. of Med., 1857.

A chapter on the *Chemical Composition of the Urine* contains nothing worthy of particular mention. He next proceeds to show that *albumen may be found in the urine in health*—*a*, when an excess has been taken in the food or injected into the blood; *b*, as a result of nervous influence [Bernard states, that by thrusting a needle into the floor of the fourth ventricle, at a point higher than that which gives rise to glycosuria, albuminuria is produced]; *c*, from excess of water in the blood [Magendie produced albuminuria at will by injecting water into the blood of animals]; *d*, from pressure of the blood on the walls of the capillaries. He quotes, in point, the experiments of Robinson and Frerichs, who have caused at will albuminuria in animals, either by tying the renal veins, or by placing a ligature on the aorta below the origin of the renal arteries and removing one of the kidneys so as to determine intense hyperæmia of the other.

In the fourth chapter, on the *Pathology of the Urine in Diseases attended with Albuminuria*, he enumerates the various tests in vogue for the detection of albumen, preference being given to heat and nitric acid. He has repeated the experiments made by Mialhe and Pressat on the one side, and Becquerel and Vernois on the other, to determine whether albumen, as it appears in the urine in disease of the kidney, the white of egg, and albumen of the serum of the blood, are all soluble in excess of the precipitant when thrown down by nitric acid, and he finds that they are thus soluble, the excess of acid necessary to produce this effect being much greater in the case of the two latter than of the former.

A short account of the *microscopical examination of the urine* contains nothing worthy of mention, and is followed by the fifth chapter on the *Pathological Anatomy of the Kidney*, including—*a*, the anatomical lesions met with constantly in Bright's disease; *b*, lesions other than (*autres que*) those of Bright's disease, which sometimes follow albuminuria, and are present either alone or coincidently with the lesions peculiar to Bright's disease; *c*, alterations of the blood that can be regarded as anatomical lesions capable of giving rise to albuminuria.

This and the preceding chapter we regard as the feeblest in the book, and as offering a remarkable contrast to many of those that follow.

(*a*.) The views or doctrines of the three great schools—French, German, and English—with their several champions—Rayer, Frerichs, Johnson—are examined in succession. That Rayer should have grouped all his six divisions under one head—"albuminous nephritis" —is not to be wondered at, so shackled was he by the dogmas of the school of his day; but that your German, after passing many a long day and many a weary night in examining bits of kidney under the microscope, should, after all, find himself compelled to refer the various changes of structure that he has discovered to one common source, and to consider them all to be different stages of one morbid process, is, indeed, he thinks, surprising. "Rayer talked to us of albuminous nephritis, the Germans now speak of *desquamative nephritis*."

We always believed that the term "desquamative nephritis" emanated from the English school, and was first employed by Johnson, but the author seems determined to give the Germans even more than their due. We turned therefore to p. 25 of Frerichs' book on the Kidney, which the author quotes throughout as the fairest representative of German doctrines on this subject, where we find—"Todd and Johnson recognise in the kidney of scarlet fever a *desquamation of epithelium of the kidney*, but the retention of the fibrinous coagula in the tubules at the commencement of the process, and the changes in the texture of the kidney, are opposed to this view." The description given by Johnson in his last publication,[1] is that in which evidently he believes; but he grieves that even the Englishman should still bow his neck to the yoke of inflammation. "Inflammation is not the fundamental fact in Bright's disease either acute or chronic." The termination *itis* raises his indignation. We own that, after reading this hit at the poor German pathologist, we looked with interest to an original chapter from the author on the morbid anatomy of the kidney, which should place the French school, with him for its leader, foremost in the field; but, alas! we looked in vain.

(*b.*) In this category are ranged lesions of the bloodvessels of the kidney, and products of syphilis. The changes in the bloodvessels described by Johnson are alluded to, but, strange to say, no description whatever is given of the amyloïd degeneration—a question that has assumed such importance in the morbid anatomy of this organ. This form of disease is still spoken of by him as the "waxy or bacon kidney;" nay, he even goes so far in one part of the book as to talk of a "bacon diathesis"—"*diathèse lardacée.*" Syphilis is allowed to exercise an influence in the development of Bright's disease, and he quotes a case related by Virchow to show the connexion that exists between syphilitic affections of other organs, especially the liver, and those of the kidney. Obliteration of the renal vein by thrombus is considered by some to be a cause that will give rise to albuminuria; he quotes some cases supposed to be in proof of this, but shows that in all there was some degeneration of the kidney, independent of the thrombus, which would account for the albumen.

(*c.*) The presence of albumen in the urine in certain cases, where there is no apparent change in the kidney to account for the phenomenon, is attributed to an alteration in the blood. As this question will be again treated of, we will leave it for the present.

In the last three chapters, which bring to a close the first part of the book, the subject of albuminuria is treated at great length; and it is here that, in our opinion, the interest of the book begins. Albuminuria is to be regarded merely as "a symptom of an organic or functional lesion of the kidney, dependent on pathological conditions most numerous and dissimilar." The various doctrines concerning it propounded by different pathologists may be grouped together under two great heads:—1. That of Bright and his followers, who regard it in the light of a functional disturbance causing by its persistence renal lesion. 2. That of the German school (Frerichs), which considers the renal lesion to be the constant cause of which albuminuria is the effect, the slightest and most transitory attack of albuminuria being in all cases referrible to hyperæmia of the kidney. The first of these will not hold. It does not follow that because, as will presently be shown, albuminuria may exist without any recognisable lesion whatever, it is therefore never caused by renal disease, any more than that because paralysis may exist without any recognisable lesion of the nervous centres it is therefore never dependent on disease of those centres. Bright's reasoning is false. The second is, he thinks, equally unsatisfactory. He cannot here resist another hit at Frerichs, whom he considers to be more of a schoolman than a good clinician, more of a theorist than a rigorous observer; but to whom he fully atones in the next page for these unkind and, we think, untrue words, by dignifying him with the title of "le plus fort et le plus avancé des micographes Allemands," which is perhaps even more than the Berlin professor would ask for at his hands, though he might lay claim to the title of "good clinician and rigorous observer." The observations of numerous pathologists who have studied more particularly the morbid anatomy of the kidney, among whom are quoted Wilks and Basham, show that there is no necessary relation between renal congestion, desquamation of the epithelium of the kidney, and albuminuria; that both desquamation and albuminuria may be present without any lesion of the kidney, and that albuminuria may occur independently of either of the other two conditions. He concludes that albuminuria may and often does exist without any structural change in the kidney appreciable to the microscope. The question of the possibility of its occurrence without even congestion or hyperæmia—a much more difficult one to dispose of—is then treated at considerable length. He argues that in simple nephritis it is not present in the period of congestion; that hyperæmia of the kidney is met with in all cases of scarlatina at a certain period—the period of eruption—although in one-half of all cases there is no albumen in the urine, and in others, where it does appear, the period of eruption has passed by, the hyperæmia is past and gone, and the patient may be regarded as convalescent. With regard to this part of his argument, we would draw attention to an apparent contradiction of the above at p. 371, where he writes—"In 21 cases of albuminuria in scarlatina, I have observed 8 times the appearance of this phenomenon, *between the first and the sixth day of the eruption*, 13 times after the sixth day and during the period of desquamation." Thus, on his own showing, in more than a third of all cases of albuminuria in scarlatina, the phenomenon occurs at the commencement of the disease, in the eruptive stage, when, as he himself declares, the kidney is most hyperæmic. This apparently weakens, nay, nullifies his argument, even if no account be taken of the fact that congestion of the kidney *is* present in all cases of albuminuria occurring even in the later stages of scarlatina.

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[1] Med. Chir. Trans., 1859.

He then continues—"In the eruptive stage of measles and smallpox the kidneys are extremely congested, but albuminuria is rarely present; in heart disease there is often, 'tis true, albumen in the urine, but in many cases, where the venous stasis is extreme, it is altogether absent." But should not this be put in another form? May one not ask the question, To what do you attribute the presence of albumen in the urine in those cases of heart disease in which no alteration in the structure of the kidney can be detected, but in which there is great venous congestion? For an answer to this, we turn to p. 353, where, after speaking of such cases which have come under his observation, and endeavouring to explain them, he alludes to the experiments of Robinson and Frerichs, already mentioned, in which the *increased pressure* of the blood gives rise to escape of albumen into the urine, and then adds—"It is certain that advanced organic disease of the heart, especially when accompanied by cirrhosis of the liver, can play an analogous part in the production of albuminuria." He therefore allows that congestion of the kidney can give rise to albuminuria in certain cases, although at p. 136, with which we are occupied, he writes—"Who in good faith will believe that renal congestion can of itself give rise to albuminuria?" Again—"The kidneys are extremely congested in asphyxia, yet there is no albuminuria." But surely the most bigoted disciple of the congestion theory would not maintain that a rush of blood to the kidney from a sudden check offered to the circulation can in two or three minutes, at the most, squeeze out, as it were, the serum of the blood through the distended capillaries of the kidney; he would, we are confident, petition for a little longer time than this to let the congestion do its work, and corroborate his doctrine.

We have dwelt thus at length on this very important and interesting point in pathology, because we think that the author is not warranted in drawing the conclusion at which he arrives, that renal hyperæmia is of itself powerless to produce albuminuria without hæmorrhage, and that there must be a morbid condition of the blood to account for this phenomenon. Surely the same pressure of the blood that will cause rupture of the capillaries of the kidney and albuminuria, as a consequence of hæmaturia, will also give rise to simple albuminuria without the presence of blood, just as happens when a ligature is placed on the renal vein; nor does it here seem necessary to call in an additional power—a *deus ex machinâ*—in the form of an "altered condition of the blood."

Albuminuria is considered under the two great heads of *transitory* and *persistent* albuminuria. By transitory he understands that which is found for a time only in very many diseases, which is independent of any renal lesion whatever, and which must be attributed to an altered condition of the blood. Under the head of persistent he classifies all chronic cases, where there is some change in the structure of the kidney to account for this phenomenon, as albuminuria in scarlatina, in the so-called Bright's disease, and in certain diseases of the genito-urinary tract. This classification seems on the whole to be a very good one, although it must be allowed that in many cases of scarlatina, albuminuria is no more persistent than in measles, typhus, &c.

Transitory Albuminuria.—In *neuralgia:* He states that he has met with it in cases of facial and frontal neuralgia, and alludes to Bernard's experiments, already quoted. In *phthisis:* Here it may depend on Bright's disease, acute or chronic, but it may also be present without any lesion of the kidney whatever; it is sometimes also due to tubercular disease of the kidney. In *simple uncomplicated pneumonia*, it is present in rather more than a twelfth of all cases, coming on usually between the eighth and sixteenth day, nearly always accompanied with lithates in considerable quantity, and therefore to be regarded as critical. In grey hepatization or *chronic pneumonia*, he has found it once in 7, in typhoïd pneumonia once in 11 cases. This average is not high enough, according to the observations of Parkes and Finger, the former of whom found it in 42·6 per cent., the latter in 45·4 per cent. of all cases. Again, Heller and Parkes are of opinion that it is not in the period of resolution, but during the height of the disease, that it occurs. In *capillary bronchitis* he found it in 4 out of 57 cases in which the urine was carefully analysed. In *erysipelas* and *hospital gangrene*, albuminuria was present in 15 out of 105 cases; it is seldom met with in the milder, but generally in the severe forms of these diseases. He combats the opinion of Begbie, that it appears during the period of desquamation or commencing convalescence; he has always found it appear in the early period, between the first and the sixth day, and disappear with convalescence; he cannot, therefore, consider it to be due to desquamation of the kidney epithelium—an opinion entertained by Begbie. The fact of the occasional occurrence of erysipelas in connexion with Bright's disease is commented on, but he regards it as no more than a consequence of an exaggerated distension of the skin by the serous effusion, for he has seen it just as often in anasarca from heart disease. In *pyæmia*, it was present in 3 cases that he watched. In *typhus* and *typhoïd fever:* In genuine typhus it is always present, appearing at an earlier period than in typhoïd; he found it in 12 of 95 cases of typhoïd, where it appeared between the eighth and twenty-fourth day—as a rule, before the fourteenth [according to Murchison, never before the sixteenth]. He makes the extraordinary assertion that "a constant character of albuminous urine in typhoïd fever is its alkalinity." The fallacy contained in this statement, which is contrary to the opinion of all the best observers, must depend on one of two causes—either that the urine was not tested directly after it was passed, or that it had been partially retained in the bladder from extreme feebleness, in his cases, and had undergone partial decomposition. It must be remembered, as is shown by Parkes, that the urea here tends rapidly to decompose, so that urine, which is acid when passed, will become alkaline in two or three hours. *Puerperal fever:* In all well-marked cases that he has met with, albuminuria has been constantly present. *Diphtheria:* The nature of the epidemic exercises a marked influence on the frequency of its occurrence, just as in scarlatina; thus, in some it is present in as many as two-thirds of all cases, in others only in one-third. It frequently causes lesion of the structure of the kidney, analogous to that met with in Bright's disease. *Ague:* (*a*) Where there was only one simple attack, he has found it only in 3 out of 150 cases; (*b*) where there were repeated attacks, in 8 out of 48; (*c*) pernicious fevers: it is here a constant phenomenon, coming on during the first paroxysm, and then abating or continuing till it increases in intensity in the second attack. During the paroxysm the urine is high-coloured and natural, but becomes acid after the administration of quinine. *Cerebro-spinal meningitis:* In 3 out of 7 cases. Two post-mortem examinations disclosed no alteration of the kidneys; it may here perhaps be the result of some nervous influence. *Choleriform diarrhœa:* In about a half of all cases that he has examined, but less intense and lasting a shorter time than in the following. *Cholera:* In 17 out of 20 cases; he has found that, contrary to what is generally supposed, the urine during the period of reaction is not so highly albuminous as during the cold stage. Here the secretion of urine is so scanty that it must always be drawn off with the catheter, and the small quantity thus obtained is always highly albuminous. He comes here again into collision with Frerichs, who considers the albuminuria to be dependent on a true lesion of the kidney. This he absolutely denies; nor will the intense hyperæmia of the kidney, which he allows to be present, suffice. "The *quid divinum* which causes the cholera must be regarded as the general cause, the alteration of the blood as the proximate cause of the albuminuria." *Epilepsy:* He draws a distinction between epilepsy and eclampsia. The term eclampsia is preserved in the translation in the same sense as he uses it—viz., as applied to convulsions in infancy, in the puerperal state, and in uræmia, in contradistinction to the convulsions of epilepsy and hysteria. He believes that in the former albuminuria is never present, in the latter nearly always, so that its presence or absence may serve as a means of diagnosis. *Rubeola:* In not more than a ninth of all cases; quantity small; very temporary. *Variola:* In 1 out of 17 cases, but this one was confluent; he therefore supposes that it may be present in many cases of confluent. (We find that Parkes gives as an average 2 in 5; Becquerel, 1 in 11.) *Morbus cordis:* He believes that, in certain cases of organic disease of the heart, there may be prolonged an intense albuminuria without any special lesion of the kidneys; and we have already alluded to the fact that he here adopts as most plausible the congestion theory, quoting the experiments of Robinson. Both albuminuria and dropsy may be present without any trace of Bright's disease; and therefore, for the differential diagnosis, examine the following points: 1. Has the disease of the heart existed to your knowledge before the appearance of albuminuria? Inquire into the history of previous scarlatina, &c.; examine the urine microscopically for casts. 2. If you find concomitant symptoms of heart and kidney disease, the heart will have been probably the first of the two organs affected. 3. Has albuminuria clearly been present before any signs of heart disease showed themselves? Bright's disease may with tolerable certainty be diagnosed. *Cancerous cachexia:* That the cachexia, *per se,* may give rise to the albuminuria, as in ague. He has found it present in 2 of

6 cases; but as no post-mortem examination was made in either of the two, the statement is merely conjectural.

Chapter VIII., *Persistent Albuminuria.*—Under this heading he places albuminuria in scarlatina, which, though it is not always persistent, and may occasionally disappear spontaneously without leaving behind it any structural lesion of the kidney, yet for the most part lasts, and is attended with structural disease; secondly, all varieties of the so-called Bright's disease. *Albuminuria in Scarlatina:* The pith of his assertion and arguments may be given in a few words: That albuminuria is present in scarlatina in more than one-third and rather less than one-half of all cases, lasting on an average seven to eight days. That of 26 cases where it was present, in 10 it appeared between the first and sixth day of the eruption, in 16 after the sixth day, during the period of desquamation. That the changes which take place in the epithelium of the kidney, and which, in the great majority of cases, are the immediate cause of the albuminuria, are exactly analogous to those in the epidermis. That when this general eruption bursts forth at once and simultaneously on the skin and mucous membranes, the lining membrane of the uriniferous tubules is affected, and there is consequently albuminuria at an early period in the disease; but that if it involve the epidermis alone in the first instance, and then spread more gradually to the internal organs, the renal affection and the albuminuria are of late occurrence. That an alteration of the structure of the kidney, as indicated by desquamation of epithelium cells, is present in a greater or less degree in all cases of scarlatina; and that whereas albuminuria is only present in about a half, the probability is that the alteration must reach a certain extent and intensity to give rise to the phenomenon. Finally, he concludes with these words: "It follows no less clearly that the alteration of the blood is not sufficient to account for the albuminuria, because it is present in all cases of scarlatina, whereas albuminuria is only present in rather less than a half of all cases." (p. 380). But what an argument is this! In measles, in small-pox, in pyæmia, in puerperal fever, &c., is there not, on his own showing, an alteration of the blood present in all cases, while albuminuria is much less often a symptom than in scarlatina? If, then, he considers "alteration of the blood" to be a sufficient cause to account for the albuminuria, rare though it be, in those other diseases, *a fortiori*, should he make use of it in scarlatina, where albuminuria is so much more frequent? But no! At last he has touched a solid, he has discovered a lesion. No more of humoral doctrines! Away with the "*quid divinum!*" Bring forward the German pathologist with his knife and microscope, and hear now what he has to say to you!

Relations of Dropsy and Scarlatina.—He notices the comparatively great frequency of dropsy in England, and its rarity in France. Of the 26 cases of albuminuria already mentioned, 8 only showed any trace of serous effusion. Its first appearance is nearly always between the sixteenth and twenty-sixth day of the disease, at or after the period of desquamation. He refuses to accept the theory of the

English school, and Johnson in particular, that it is attributable to cold. Its frequent occurrence in some epidemics, and its scarcity in others, point much rather to the conclusion that it is directly dependent on the nature of the poison than on any external and casual agencies,—that it is directly influenced by the malignity or intensity of the scarlatina. He shrewdly observes that Graves and his followers, who argue for the contrary, do not consider that the scarlatina maligna destroys life, in the majority of cases, before the dropsy has a chance of appearing; and that therefore the result of 10 cases of malignant, of whom on an average 6 die before the twentieth day, the day on which the dropsy appears, cannot be compared with that of the same number of simple cases, all of whom live. If, then, the patient lives, the chances of dropsy are greater in proportion to the malignity of the attack. He treats with great clearness the question of the mechanism by which it is produced, showing how there are necessary two or three factors, the concomitant action of which is required—a loss of the albumen of the blood, a suppression of the secretion of the kidney, an impairment of the functions of the skin;—any one of these acting alone is insufficient. The elements of the sweat thrown back upon the blood find no way of escape by the kidney, and the blood becomes surcharged with water, while it parts with its albumen.

A short history is given of *uræmia*, and the different theories proposed at different times to account for the phenomenon are mentioned and criticised. How a peculiar set of symptoms were supposed to be due to retention of urea in the blood, and were grouped together under one happy denomination, "uræmia." How Frerichs upset this by injecting urea into the blood of animals, and so proving that, *per se*, urea was no poison, but that carbonate of ammonia, a product of its decomposition, was the real poison at work, as might be demonstrated by examining chemically the breath of those suffering from the so-called uræmic symptoms. How this theory was in its turn controverted by many writers, but by none more ably than by Schottin, who shows that carbonate of ammonia may always be detected in the breath where the mouth is foul from sordes, carious teeth, &c.—consequently, in many fever-patients, and in cases of tonsillitis; that carbonate of ammonia is not the only substance which, injected into the blood, gives rise to convulsions and coma, but that other salts, as the saline sulphates, produce exactly similar effects; that, contrary to what was observed by Frerichs, he could only once detect carbonate of ammonia in the breath out of 16 cases of Bright's disease examined, and in that one case the mouth was loaded with sordes. Hence the author thinks that we must fall back on the supposition that the symptoms are due not to the retention of any one element of the urine in particular, but rather of several, the separation of which from the blood is essential to its purity.

In treating of the question of *coma*, he dwells upon an acute form in uræmia, which is not preceded by convulsions, but kills suddenly and rapidly, like apoplexy, with which it is often confounded. But he asserts, even in these cases, slight premonitory symptoms—

torpidity, dulness of the intellect—will be observed if looked for. *Pain in the head* is a very common symptom, sometimes diffused, sometimes in form of hemicrania. He dwells on the importance of analysing the urine in chronic headache. *Amaurosis:* The condition of the eye which gives rise to this is very fully discussed. Diplopia is rare at the commencement, generally present at an advanced stage; muscæ are common; cornea and sclerotica generally sound; pupils often dilated equally or unequally; iris normal; media of eye, as a rule, transparent; irregular black patches, over which pass varicose vessels of the retina, are seen on the retina towards its periphery, being collections of choroïdal pigment; optic papilla in part or generally hyperæmic, itself and the retina for some distance round it appearing turbid. In chronic cases there are seen, especially in the neighbourhood of the papilla and limbus luteus, small ecchymoses and yellowish patches, isolated or in groups, which are probably extravasations that have undergone fatty degeneration. The following microscopical changes are described by Charcot:—choroïd sound; retina thickened, and containing nebular patches, ecchymoses, and yellow spots; nerve-cells more or less the seat of fatty degeneration, molecular granules being scattered or in groups between the degenerate cells.

Passing over a few pages on the characters of the urine and on other symptoms of Bright's disease in scarlatina, which contain nothing more than is already known on the subject, we come to the chapter on *Prognosis of Albuminuria in Scarlatina*, wherein we read that it is important to ascertain whether the albumen first appeared in the eruptive or desquamative stage; if in the former, the chances for the patient are much more favourable. The quantity of albumen present, its persistence, the presence or absence of fever and vomiting, and, above all, of dropsy, are weighty points for consideration. Not only is dropsy rare in France, but also rarely fatal. We look also naturally with much curiosity to his plan of *treatment*, and we find that it differs little from that in vogue among us. General blood-letting is condemned, cupping at the loins to be recommended. We are told to act on the skin, and to purge, but to leave the kidneys at rest. Tannic acid has completely failed in his hands.

One hundred and thirty pages are then devoted to the consideration of *Bright's disease properly so called*, which he defines in the most arbitrary manner as "a disease characterized by the presence necessarily of three phenomena—renal lesion, presence of albumen in the urine, dropsy more or less extensive." Without stopping to question the correctness of this definition, to which we do not think that Bright himself would have subscribed, we go on to examine briefly any points worthy of observation in his description, and such as differ from the views commonly received in this country. It may be prefaced that he recognises an *acute* and *chronic* form of Bright's disease—the former corresponding very nearly with the acute desquamative nephritis of Johnson, the latter including the varieties of disease described by Johnson in his later publication. The acute form is by no means uncommon in infants and children, independent of scarlatina. Cold is an occasional but less common cause than is supposed by the English. In the symptomatology he insists on œdema in some part of the body as a necessary symptom in all cases, either acute or chronic. The acute form, as a rule, terminates favourably, but, even after a seemingly radical cure has been effected, may return; also that occasionally it may be prolonged into the chronic form. Among the complications he alludes to the condition of the stomach and intestines, to the frequency of inflammation of the mucous membrane in the great *cul-de-sac* of the former, and in the last part of the ileum in the latter. Small ulcers may here be met with in cases where diarrhœa has been a marked symptom, but, on careful examination, are generally found to be tubercular. The form that attacks children is sometimes very insidious, and very difficult of diagnosis, the most prominent symptom being in many cases bronchitis, which resists all treatment. In his system of *treatment* he differs little from the majority of English practitioners. He upholds bloodletting in the acute stage, and agrees with Johnson in recommending antimony among other remedies. Gamboge is very highly spoken of in doses of 4 or 5 grs., increased gradually to 15; it is a purgative that can be employed even in very feeble subjects. We are glad to see that he speaks in favour of diuretics, especially digitalis, but only at a particular stage in the disease—viz., after the congestion has been relieved, and before advanced disorganization of the kidney has set in. Their effect must be carefully watched, and if diuresis does not speedily follow, they are to be discontinued. Strange to say, he condemns the use of the hot-air and vapour bath, "because," he says, "carried along by routine, I have, like the rest, abused this remedy, and given as many as thirty *vapour baths* without obtaining any other result than that of exhausting and prostrating my patients." He does not, however, speak of his experience of the hot-air bath, which he would not, we think, condemn so sweepingly had he made proper trial of it. The water cure is, on the contrary, lauded to the skies. Of all the preparations of iron, the perchloride has answered best in his hands; it is particularly serviceable in cases where vomiting and diarrhœa are prominent symptoms. Notes of 26 cases terminate this section, including sucklings of eleven and eighteen months, and children of various ages, from three to ten, in all of whom the dropsy was independent of scarlatina. A complete omission of post-mortem examination detracts materially from their value.

A short *résumé* of some very interesting pages on *albuminuria in pregnancy* will bring to a conclusion this first part of the book. As regards frequency of occurrence, Blot, in a series of observations made in the 'Clinique,' detected albuminuria once in every 5 pregnant women. The author states that the average in his own private practice was 1 in 10—the greater frequency in the former series being attributed to the comparatively feeble condition and poorer circumstances of women in a hospital. It commences, as a rule, during pregnancy, not, as is generally stated, during labour; also it ceases in most cases soon after labour is over. Primiparæ are more especially subject to it. Three important questions must be taken into account: 1. *Is albuminuria in pregnancy always attended with renal lesion?* After examining the statements on the one hand of those who consider it due to a chloro-anæmic condition of the blood (theory of Simpson), on the other of those who regard it as the result of renal lesion (Rayer), he concludes that, though lesion of the kidney is much more common than was supposed in connexion with albuminuria of pregnancy, there still remain a number of cases in which, even after the most careful microscopical examination, no changes of structure can be detected. In such cases pregnancy must be regarded as the cause, either by the obstruction offered to the circulation, or by causing an alteration in the blood. 2. *Is eclampsia always attended with albuminuria?* Not always; it may be present without any trace of albuminuria. Depaul quotes 5 such cases; Dubois, Leuret, Imbert-Goubeyre, each give 1. The author relates in full the notes of a case in which eclampsia appeared in the eighth month, and again during labour, though no trace of albuminuria or œdema could be discovered during the whole period of pregnancy. Depaul and others suppose that the albuminuria is an immediate consequence of the convulsions; this he cuts short by remarking that the observations on which the supposition is based are inexact, in that the urine was not carefully tested during pregnancy before the attack came on. On the other hand, albuminuria is often present without eclampsia; thus, according to statistics of Cazeaux, eclampsia is not met with more than once in 484 pregnant women, whereas the author has shown already that albuminuria may be found once in 10. 3. *Is eclampsia in connexion with albuminuria always a manifestation of Bright's disease?* Not always. Signs of Bright's disease are often to be detected, but in not more than a half of those who die of the eclampsia. He draws this conclusion relying more particularly on the result of 44 cases, given at length by Braun, of Vienna, in an elaborate paper on the subject.

The influence of eclampsia in the mother on the fœtus is very marked. Supposing that the child reaches its full term after the mother has been attacked in the course of pregnancy, it will very probably be born dead. The average of deaths is here reckoned by Braun at 40 per cent. When a woman dies of eclampsia before delivery, the child extracted is nearly always dead, or dies quickly. If eclampsia is severe enough to bring on premature labour, the fœtus is generally dead, or dies soon after birth (average of deaths, 64 per cent.). The differential diagnosis of eclampsia from Bright's disease and simple eclampsia not depending on disease of the kidney is quite possible, by paying attention particularly to the presence or absence of albuminuria for some time before the attack, and to its persistence or rapid disappearance after delivery:

> "The frequent concomitance of dropsy with albuminuria in pregnancy is no positive proof of the existence of renal lesion; for the constitution of the blood of pregnant women disposes them to dropsy; and if both phenomena disappear with the birth of the child, it is certain that neither the one nor the other was connected with Bright's disease."

Treatment of the Albuminuria.—If a woman's life is in danger from the accidents to which Bright's disease may give rise in pregnancy, how far are we warranted in inducing premature labour? (*a.*) If we are sure that the fœtus is dead, to save the mother. (*b.*) If the woman is so far advanced in pregnancy that the child may be fairly supposed to be capable of living, to save mother and child. (*c.*) When the fœtus, though alive, is not capable of life out of the uterus, and when the mother is about to die, the child may perhaps be sacrificed to save the mother. This last is a very difficult question, as the decision turns on whether or not there is a fair probability of saving the life of the mother. *Of eclampsia:* He quotes Braun's opinions at length. Treatment may be divided into medical and obstetrical. Chloroform is most valuable, but should be administered before the paroxysm is fully developed, when rigidity of the muscles, slight twitches or spasms, and a peculiar restlessness indicate its approach. It should be continued till sleep supervenes, but should not be given when the eclampsia is fully developed. Braun is opposed to the practice of bleeding, but the author regards it as a valuable remedy in many cases. Obstetrical interference is warranted in certain conditions. If labour is already advanced when the attack commences, finish it as quickly as possible with the forceps or by turning, according to the position of the child. If there is slight dilatation of the os, rupture the membranes, and, if labour does not rapidly progress, dilate the os with the fingers. If there is no sign of approaching labour, it must be induced prematurely. Three different methods to be employed are then given; but as the opinions of Braun published some years ago are here quoted, we have thought right to give a short analysis of some papers published this year, under his direction, by Kühn, his first assistant, from observations made in the lying-in department of the hospital at Vienna.—'Cf. Wiener Mediz. Wochenschrift,' No. ix., Feb., 1863.

Twenty cases of induction of premature labour or abortion have occurred there during the last three years, the various methods adopted being *puncture of the membranes* with the English elastic catheter or Simpson's sound, four times; *intra-uterine catheterism* with the elastic catheter or catgut bougie, twelve times; *combination of puncture of membranes with catheterism,* twice; *intra-uterine injection* with the apparatus of Lazarewitsch, twice. The causes which necessitated the operation were as follows: Contracted pelvis, nine times; uræmic poisoning, four times; suffocation from dyspnœa in Bright's disease, twice; tetanic spasms, once; pneumonia, once; acute tuberculosis, once; chronic tuberculosis, once; chronic bronchitis, once. In 23rd week of pregnancy, 1; in 24th, 1; in 29th, 1; in 30th, 3; in 32nd, 6; in 34th, 2; in 35th, 2; in 36th, 3; in 37th, 1. In 13 cases child born alive, but 6 of 13 died shortly; in 7 child born dead. Of the women, 8 recovered perfectly, and were dismissed; 1 died after eight weeks of phthisis; 1 recovered from the operation, but was attacked with acute inflammation of knee, and transferred to a surgical ward; 10 died after labour, 4 of puerperal fever, 4 of Bright's disease, 1 acute tuberculosis, 1 hæmorrhage.

Of the different methods the preference is given to catheterism, especially in those cases where no dilatation whatever of the os is to be detected. The catheter is introduced with comparative ease through the long, soft, narrow cervix, and, after the stilette is withdrawn, pushed up on either side towards the fundus of the uterus, between it and the membranes, and left lying there. In the 12 cases where it was employed, the first pains supervened, on an average, five and a half hours, the child was born, on an average, thirty hours after its introduction. The operation was in no single instance followed by hæmorrhage; but, it must be remarked, care was taken, as far as was possible, to avoid the placenta, the position of which was calculated from a preliminary and careful estimation of the position of the fœtus. Next in favour comes puncture of the membranes with some blunt instrument. This plan is rather to be recommended in cases where the cervix is shortened, or where the os is dilated enough to admit of the introduction of the finger; thus, in cases of contracted pelvis where the head is presenting. It may be also generally employed for the production of abortion. The intra-uterine injection seems to be longer in its operation than the catheter. He advises that the opening through which the water is injected should be at the end of the catheter, and not at its side, in order that the stream of water may penetrate farther; also that the caoutchouc bladder of Lazarewitsch has the disadvantage of becoming softened by the warm water, and losing much of its elasticity and power of injecting, so that it is not to be preferred to the common enema syringe.

A very short sketch of Part II., on "Glycosuria, or Diabetes Mellitus," is all that space will permit. This is the less to be regretted, in that a full review of this subject, in the October number of this Review for 1862, contains a much clearer and more complete exposition of the physiology and pathology of sugar than could possibly be given in an analysis of these eighty pages, however full it might be.

There is a remarkable analogy, he observes, between albuminuria and glycosuria. Thus, the presence of sugar, as of albumen, in the urine may be either transitory or persistent: in the one case, a symptom *per se* of little or no import, occurring in connexion with certain diseases, as epilepsy and hysteria, blows on the head, a sudden and severe hindrance to respiration, certain diseases of the liver, stomach, and lymphatic glands; myelitis: in the other, a symptom in like manner, but directly indicative of some serious and fatal constitutional disorder, the *point de départ* of which, though still *sub judicê*, will, it is to be hoped, as certainly be determined by future investigators, as was the kidney by our own Bright, in the history of albuminuria. For the detection of sugar in the urine, Barreswill's solution is highly spoken of, but it must always be fresh. Names mentioned in connexion with theories to account for diabetes are those of Bouchardat—formation of an excess of sugar in digestion and introduction into the blood; Bernard; Mialhe—deficient alkalinity of the blood, which renders impossible the destruction of glucose in the economy; Raynoso—an impairment in the functions of respiration, whence follows an incomplete combustion of glucose. The names and doctrines of Prout and Pavey are unnoticed. He inclines to Bernard's view. With regard to the vexed question of the quantity of urea and uric acid excreted, he believes that, if the whole quantity of urine passed in the twenty-four hours be taken for observation, the proportion of urea and uric acid in the urine will be much as in health. The diseases of the eye in connexion with diabetes are examined at some length. A slighter form of amblyopia appears in the early stages of the disease, which may disappear rapidly and even suddenly, but sometimes persists and becomes serious. A severe form, only met with in confirmed disease, affects, like the former, both eyes, but for the most part unequally. The ophthalmoscope shows the optic papilla to have a nacreous appearance, to be smaller than natural, sometimes excavated, with its border irregular; the vessels appear tortuous. Jaeger and Desmarres have found the retina the seat of hæmorrhages and fatty degeneration. The name of our countryman France is of course quoted in connexion with the diabetic cataract. The experiments of Richardson for the production of artificial cataract by introducing substances into the blood which, either by a direct action on the lens or simply by increasing the density of the aqueous humour, render the lens opaque, are criticized. Lecorché, who has made the same experiments, but has never succeeded in producing such a result, not even in cases where he has been positive that sugar was present in the humours of the eye, attributes the cataract to the "general deterioration of the organism," not to any special action of the sugar on the lens.

After examining various plans of *treatment*, and giving especial preference to that of Bouchardat, he ends by recommending absolute restriction to nitrogenous and fatty food with gluten bread; regular exercise, or, if possible, gymnastics; water cure; dry warm air; employment of alkalies, Vichy water; frequent use of mild purgatives; administration of quinine and iron.

REVIEW VI.

1. *On the Parasitic Affections of the Skin.* By T. M'CALL ANDERSON, M.D.—*London*, 1861. 8vo.
2. *Skin Diseases of Parasitic Origin; their Nature and Treatment.* By W. TILBURY FOX, M.D. London. pp. 210.
3. *On Ringworm, Scall-head, Baldness, and other Parasitical Diseases of the Head and Face.* By GEORGE ROSS, M.D. London. pp. 138.
4. *A Guide to the Treatment of the Diseases of the Skin; with Suggestions for their Prevention.* By THOMAS HUNT, F.R.C.P. London.

IN all countries, and at all times, skin-diseases have been the object of peculiar abhorrence. In earlier ages, those who suffered from them were held to have sinned in some special manner, and to be the immediate victims of divine punishment. This strong feeling was doubtless in great part due to the hideous deformity of some of these affections.

But the sanitary precautions taken to prevent the spreading of the malady show that besides this there was a second cause—namely, a belief that these diseases were highly infectious. The strict enactments of the Mosaic law in the case of lepers are well known. The leper was shut out from the congregation until such time as the priest should have pronounced him clean; and similar laws existed in other communities than the Jewish. For instance, we learn from Herodotus[1] that this was the case in Persia:

> "Should any citizen have a leprosy or white eruption he is not allowed to enter into the city nor to have intercourse with other Persians; and they say that he suffers because he has sinned against the Sun. And should it be a foreigner who is attacked by one of these diseases, in many places they go so far as even to expel him from the country."

What the real nature was of these two affections, which were thus dealt with, we have now no means exactly to determine. Probably they were vague terms, under which many totally distinct eruptions were confounded; for diagnosis was young and imperfect in the day of which Herodotus is speaking; and we can hardly doubt that, under this Persian law, many a poor sufferer must have been driven out whose presence in the city would in nowise have compromised the health of his fellows. It is, indeed, only very recently that we have learnt to distinguish a contagious disease of the skin from a non-contagious one with any trustworthy degree of certainty. Before the microscope came into use such a diagnosis was impossible; and even now, when the distinction has been established, be a practitioner of medicine as experienced as he may, cases will come before him in which he must have recourse to that instrument before he can arrive at a satisfactory conclusion. It is almost entirely to foreign dermatologists that we owe our present knowledge; and pre-eminent among the rest stand the well-known names of Bazin and Hardy from the French school, and of Hebra and Kuchenmeister from the German. Many English writers follow in their steps, and their various treatises do but embody the results arrived at by these foreigners, with more or less slight modifications and additions of their own. The general conclusion in which all these modern researches end may be briefly stated; it is this: that all contagious diseases of the skin—at least, all that exist in this part of the world—owe their contagious property to the presence either of animal or of vegetable parasites. There is one, and only one, exception to this universal law, and that is, in the case of certain syphilitic eruptions. But even as regards this one exception there is no absolute unanimity of opinion. Some authorities still refuse to admit their contagious nature.

Contagious skin-diseases, then, are with this one doubtful exception, diseases of parasitic origin: and the first question concerning them which suggests itself is, whence do these parasites come? In what way can we come to be infested by them? The simplest and most manifest method of contagion is the direct one. The parasite animal, or vegetable, as the case may be, is transferred directly from the body of a person already infected to the body of a sound one. A man, for instance, in good health, sleeps with another suffering from the itch, and the acarus passes over in its nocturnal wanderings from one body to the other; or a child uses the brush of another child on whose head are some favus crusts; and in so doing transfers to its own scalp some of the *Achorion Schoenleinii*, which develops in its new site, and produces favus crusts similar to those from which it came. But it is by no means necessary that the transmission should be thus direct. The spores of the various fungi which live on the human body are of microscopic size, and excessively light. As they reach maturity, they are detached from the mother plant, and many of them, carried away by currents of air, float about for a definite period in the atmosphere. Most of these, we may hope, are eventually deposited in positions where they can do no harm; but those few which, by a fortunate chance, light on a suitable spot, say the skin or scalp of a child, and find there the necessary conditions for their development, fix on their new site, and, growing there, become in turn a source of contagion to others.

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[1] i. 138.

If any of our readers should doubt as to the power of the atmosphere to convey the fungus-spores from place to place in the way we have described, we would refer him to M. Pasteur's excellent article in the 'Comptes Rendus,' where he will find abundant proof of its possibility. M. Pasteur made use of an ingenious plan to collect the particles which float about in the air. In a long tube he placed a plug of gun-cotton; a continuous stream of air was made to pass through this tube, and the cotton acting as a mechanical filter, arrested the particles on their way. The plug was then removed, and dissolved in a mixture of ether and alcohol; as the cotton vanished, the contained particles dropped to the bottom of the fluid, and when this had evaporated, were examined with the microscope. The dust thus obtained was found to contain sundry inorganic substances. But besides these there were numerous small bodies, which could in no way be distinguished from the ova and spores of low organisms, and whose nature was most convincingly demonstrated by their developing into fungi and infusoria when placed in various solutions.[1]

There is no doubt, then, that the germs of parasites may be transmitted not only immediately, but in this indirect manner. In either case, the source from which the germ is derived need not of any necessity be a human one. The same parasites which infest the skin of man are found also on the integuments of other animals; the itch-insect, for instance, is common enough on sheep. The *tricophyton*, to which ringworm and sundry other affections are due, has been often seen on horses and other animals. Bazin notices a case in which the fungus of porrigo favosa was traced back from a child to a mouse, through a cat, which had first eaten the diseased mouse, and then been petted by the child. And there is reason to suppose that even the vegetable world may serve as a nidus for our enemies. At any rate, Gruby is said to have inoculated the bark of an oak-tree in full vegetation with the Achorion Schoenleinii, and to have seen a favus cup develope itself on the spot, identical with that which grows on the head of infants.[1]

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[1] Our readers will remember that we made allusion to Pasteur's experiments regarding spontaneous generation in our January number, see p. 170.

There are some authorities who maintain that, besides coming from these sources, the parasitic fungi may be generated spontaneously on the parts of the skin where they are found. They adduce as arguments in favour of this view the undoubted facts, that these fungi are found almost invariably only on persons of a certain constitution, or who have been weakened by disease or want of food, and also that when found, their growth may be modified or entirely arrested by internal remedies. This is not the place to discuss the general question of spontaneous generation. This theory, admitted by Aristotle, revived in our own country by Needham, and since his time bandied about with various fortune by numerous inquirers, has been most strongly contested by M. Pasteur. But without entering on this intricate question, we may say a few words on the insufficiency of the arguments just mentioned. It is true that, as a rule, but not as a universal one, these parasites are only found on persons of feeble health; and it is true also that their growth may be interfered with by internal remedies. But these facts only show what every one would expect *à priori*, that the parasitic germs require a suitable soil for their development. This is true not only of fungi but of all plants, and of all animals. Every organism, be it what it may, requires certain external conditions adapted to its wants. Place any seeds you please in a fitting soil and under proper conditions, and they will grow and flourish. Place them in an unfitting soil and they will perish. The skin of some persons, notably of enfeebled ones, affords a fitting soil for the fungi in question. When the germs light there they develope. The skin of others, for some unknown reasons, does *not* possess the requisite properties. The spores or germs which chance to fall there fall to no purpose.

So also with respect to the effects of drugs administered internally. Arsenic and other medicines have, we know, even when administered in health, a certain influence upon the skin. They alter in some way or other its nutrition. Whatever hindrance then they offer to the growth of a parasite, may be fairly attributed to the change which they have wrought in its nidus. You have made the soil an unsuitable one. It is as though you had poured some poisonous fluid into the ground on which a plant was growing. The difference, in fact, between the use of internal and of external remedies in these cases is simply this: the external remedies attack the fungus itself; the internal attack the soil, which is essential to its growth. If a drug can be found which does this effectually, it must be equally efficacious with one which attacks the fungus itself. The difficulty is to find such a substance. This inability of a parasite to grow on one skin while it flourishes on another, extends not only to the fungi but to our animal pests. In illustration of this we may quote an observation cited by Dr. Fox (p. 18): "M. Delafond has shown that in a healthy sheep it was possible to put a great number of acari on the skin without communicating the itch; while, on the contrary, the development of the cutaneous affection was rapid if the animal had been weakened by a bad regimen."

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[1] Anderson, p. 23 (quoted from Devergie).

Parasites, then, whether vegetable or animal, affect some skins in preference to others. This being so, the question at once arises, on what does this preference depend? This cannot, as yet, be answered with precision. Still there are certain general conditions which may be mentioned as indubitably favourable to their development.

In the first place, the skin of children affords a more suitable soil than does that of adults. No one who has had experience of children's hospitals will for a moment doubt this assertion. The vegetable parasites, though they often occur at a more mature age, are infinitely more abundant in youth.

Mr. Hutchinson has tried to explain this comparative immunity of grown-up persons in the case of common ringworm of the scalp. The fungus which causes this disease is found not only on the epidermis, but within the bulbs and shafts of the hairs. Mr. Hutchinson attributes the frequency of its occurrence in children to the greater softness of the cortex of their hair. As they grow older the cortex hardens, and presents a physical hindrance to the entrance of the fungus. But as Dr. Fox well remarks, this explanation is insufficient, and for this simple reason: the fungus does not make its way in through the hard cortex; it grows down into the hair-follicle, reaches the papilla, and gets into the interior of the hair through its soft growing part; and this part is equally soft in old and young.

Next to youth, among favourable conditions comes weakness, and especially the weakness of a scrofulous constitution. Sound and vigorous subjects *may* be attacked by parasites; but they are attacked much less frequently than the sickly, and, moreover, the affection, when established, holds to them with inferior tenacity, and is more easily subdued. Such at least is the result of our own experience. In this respect there is a great similarity between external and internal parasites. The common threadworm is no difficult enemy to deal with in an otherwise healthy subject. But in a scrofulous child, let what drugs it may be given, as fast as they are expelled new ones take their place, and this with a pertinacity which often forms the despair of the medical attendant.

A third condition which greatly favours the development of parasites is dirtiness. Hebra lays great stress on this as a cause of favus, and Dr. Anderson holds it to be of all predisposers the most powerful. It is not only that the dirt furnishes a good soil for the parasite, but as a dirty skin is one that is left undisturbed, the fungus finds there the repose which experience shows is necessary, or almost so, for its growth. Hebra gives an example from every-day life, which admirably illustrates this point. He remarks, that if an inkbottle be left for some time undisturbed, there is soon to be seen on its surface a development of fungous matter, whereas, if the inkbottle be kept in constant use, nothing of the kind occurs. It is this which in great part explains the immunity from parasitic eruptions enjoyed by the wealthier classes. They are cleanly in their habits, and the germs are no sooner deposited than they are brushed or washed away.

Youth, debility, dirtiness are then all conditions which favour parasitic development. Besides these there are some others, as, for instance, the existence of a non-specific eruption. The germ, when it lights on a skin already diseased, finds, as it were, the ground ploughed up and prepared for its reception, and the morbid secretions of the part supply it with the best pabulum. Cases of this kind have misled some dermatologists into the opinion, that the presence of a parasite is only an accidental phenomenon, and has no share in the production of skin disease. They see an eruption exist for a length of time in a simple state, and then some fungus makes its appearance. If they succeed in destroying the fungus they find that the eruption remains uncured, and they naturally infer that the existence or the absence of the cryptogam is of no importance. But in such a case they have been dealing not with a single malady, but with a combination of two, engrafted one on the other, and by destroying the parasite they have cured the second, while they leave the first and the non-specific one untouched. This predilection of parasites for diseased skins is well illustrated by an experiment of C. Bernard:

> "When frogs have been kept long in captivity their health declines and ulcerations arise around the mouth and nose; the nervous system being in this case considerably depressed, the animal is of course found to resist much longer the action of strychnia and similar poisons, while parasitical affections spread with fearful rapidity. Frogs are subject to the growth of parasitical fungi, which, after a certain lapse of time, occasion the animal's death. Now, if a healthy frog be placed in a jar containing others affected with the above-mentioned disease, the new comer sets contagion at defiance; while if another frog affected with ulcerations in the vicinity of the natural orifices is introduced into the jar, the parasitical vegetation covers it at once."[1]

In Dr. Fox's opinion, it is by no means necessary that an eruption should already actually exist. It is sufficient that a tendency to non-specific eruptions should be present. "It would appear," he says, "from all considerations, that the non-specific eruptive crasis is that which supplies the necessary and only fit soil for fungous growth; the crasis, or *tendency to eruption*, because by no means need there be any actual manifestation present, in the form of eruption, which is the culmination of the peculiar blood state." (p. 19.) It is by this hypothesis that Dr. Fox explains the frequency of these parasitic growths in the young and in the scrofulous; such subjects being, as is well known, especially liable not only to parasitic, but to non-parasitic skin disease.

What has been said hitherto has reference to parasitic eruptions as a class. It remains to speak of them individually. Parasites, as already has been stated, are either animal or vegetable. The maladies caused by these latter are much more numerous and much more difficult to overcome than those caused by the former; but as to the number of parasitic fungi themselves, there is great diversity of opinion. Dr. Anderson, following Hardy, reckons them as four: 1. The *Achorion Schoenleinii*, which is the fungus of favus; 2. The *Tricophyton*, to which three forms of eruption are due—namely, herpes circinatus, sycosis, and herpes tonsurans; 3. The *Microsporon Audouini*, which produces alopecia areata, or, as it is more usually called, tinea decalvans; 4. *Microsporon furfur*, to which is ascribed pityriasis versicolor. On the other hand, Hebra altogether denies the parasitic nature of sycosis, and attributes all the other above-mentioned eruptions to one and the same fungus. In this latter opinion Dr. Fox coincides. The arguments adduced in favour of the identity or non-identity of the fungi are of two kinds. One set are based on the frequency with which the various eruptions are found to co-exist on the same patient; the other on the microscopic characters of the spores and other constituents of the fungus growths. The instances are exceedingly numerous in which herpes circinatus and herpes tonsurans have been seen side by side on one skin. Sometimes a patch of the latter, occupying the edge of the hairy scalp, is seen to terminate in the skin below in an unmistakable patch of the former. So also not rarely an infant is found affected with herpes tonsurans, while the hands of the mother present rings of herpes circinatus. So also with sycosis and herpes circinatus. Their co-existence in one individual, or in members of the same family, has been noticed with sufficient frequency to establish a conviction in the minds of many dermatologists that they are virtually one and the same malady. We agree, therefore, with Dr. Anderson and Dr. Fox in placing these three eruptions under one head; but with respect to the *Achorion Schoenleinii* there is by no means an equal degree of certainty. It has, it is true, been seen side by side with the *Tricophyton*, and such cases are represented in the admirable plates which are now being reproduced by the Sydenham Society from those of Hebra; but the question is not whether the two *can* co-exist, but whether this co-existence is sufficiently frequent to convince us of their identity. This, in our opinion, has not yet been established. As to the second class of arguments—those, that is, which are based on the microscopic appearance of the spores and mycelium—we have little to say. These elements are excessively small, and the difficulties in the way of examining them carefully are confessedly great. Moreover, the element of mutability which, as Hooker says, pervades the whole vegetable kingdom, exists to the fullest extent in these humble and little specialized organisms. So that we agree with Dr. Fox in thinking that very little reliance is to be placed on minute differences of size and form as criteria for the separation of these cryptogamous plants into distinct species. After all, the question is one which concerns the botanist more than the medical man. Be the fungi one or many, it is the same thing to the practitioner. He can afford to look on the question with indifference, and for this simple reason—

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[1] Medical Times, Feb. 25th, 1860.

the same drug which will destroy one parasitic fungus will equally destroy all others. The difficulty is not to find a drug which can do this—there are several equally efficacious—but to find means of bringing the drug and the parasite into intimate contact. When this is effected—be the fungus a *tricophyton*, an *achorion*, or a *microsporon* —its destruction is ensured, and with its destruction that of the eruption which it occasions.

In some cases this necessary contact of drug and parasite may be brought about with the greatest ease. In others it is a matter of considerable difficulty. When the cryptogam is confined to the superficial elements of the skin, it is sufficient to rub one of the many parasiticidal ointments or lotions on the affected part, and in a few days the malady is at an end. Such is the case in that very common disease of childhood, herpes circinatus, and such also in another not uncommon ailment, pityriasis versicolor. These affect, as a rule, parts where there are very few hairs, and the fungus is consequently limited to the scaly cells of the epidermis. If a few of these be placed under the microscope, and a drop of potash solution added, the spores may be detected in abundance. But the task is a much harder one when the parasite fixes on some hairy part of the skin—the scalp or the chin. It is then no longer confined to the epidermis. It makes its way into the follicles, penetrates the bulb of each hair, and is found occupying the centre of its shaft. In this state of things the simple application of an ointment or of a lotion is seldom of much avail. All this treatment can do is to destroy such fungi as exist in the scales or coat outside of the hairs; but those which are within escape, and, as a rule, the disease only comes to an end when the hairs have perished. We do not say that simple external applications are never successful even in these cases. We have ourselves seen patches of herpes tonsurans yield to the application of a glass rod dipped in strong sulphuric acid, and Dr. Jenner speaks highly of the results obtained with sulphur ointment, to an ounce of which forty grains of ammonio-chloride of mercury have been added; but we say that no reliance can be placed on this method of proceeding. In the majority of cases it is unsuccessful, and to ensure a cure we must have recourse to stronger measures. The only safe method is to pull out the hairs from the diseased spot one by one, and at the same time to apply a suitable parasiticidal ointment. This is the principle of the treatment which has long been pursued in Paris by certain empirics; but the plan which they adopted for the extraction of the hair was unnecessarily painful. Strips of some form of pitch-plaster were applied to the head, and after a time stripped off, the hairs, of course, being torn out at the same time. Besides the atrocious pain, there is another evil attending this plan. Many hairs escape, and are left to form new centres for the spread of the eruption. The method at present in use in the hospital of St. Louis is in every way preferable. It is more complete, and causes much less pain; in fact, if carefully conducted, it hardly causes any. The hairs are pulled out with forceps made for the purpose. Only one or two are removed at each tug, and if the skin be very sensitive only a very small extent of surface is cleared at one sitting. The sensibility may also, according to Bazin, be diminished by applying oil of cade to the surface; but Hardy, as also Dr. Anderson, state that they have found very little profit in this application.[1] It is not enough to extract the hairs; at the same time a parasiticidal lotion must be employed, and there are none better than those which contain corrosive sublimate. Hardy uses one containing a grain to the ounce. Dr. Anderson recommends one of double that strength. Each dermatologist has, in fact, his own pet application. The hairs, it should be mentioned, ought, for security, to be removed from a rather more extensive surface than is apparently diseased; and the external applications should be continued for some time after the depilation is complete, and the malady seemingly at an end. The extraction of the hairs is not an easy business. It is true that, the bulbs being diseased, the hairs hold slightly by their roots; but, then, they are excessively brittle, and frequently give way in the attempts to pull them out. Dr. Jenner even goes so far as to say that, in the case of herpes tonsurans, extraction is impossible; but in this he is certainly mistaken. We have ourselves seen the operation performed, and can vouch for its being feasible; but it must be admitted that it requires considerable dexterity and carefulness; so that it has been found advisable at St. Louis to entrust this function to men specially trained for the purpose. It is only in herpes tonsurans that any very great difficulty occurs in the process of depilation. In favus and in sycosis the hairs may be much more readily extracted, as they are by no means so brittle as in the former affection. Patients suffering from the remaining form of parasitic disease—Alopecia areata, or, as it is more generally styled, Tinea decalvans—rarely come into the medical man's hands until the hair has already disappeared from the diseased part, leaving round bare patches on the scalp. The hair may then be removed for a short distance round each patch, while some blistering fluid is applied to the spot itself. At the same time as these local remedies are in use the general health must not be neglected. The subjects of parasitic disease are, as before mentioned, often scrofulous or debilitated, and cod-liver oil and tonics are of great service in altering their constitutional condition and rendering the soil unsuitable for the development of fungi.

Before we leave the subject of parasitic fungi, we may mention that there is one skin affection to which neither Dr. Anderson nor Dr. Fox allude, which is in all probability also due to the presence of a fungus. This is the *Molluscum contagiosum* of Bateman; the *Acne varioliforme* of Bazin. Bateman had already pointed out the contagious nature of this disease, and had named it in accordance with his opinion; but pathologists had entertained great doubts of his accuracy, until M. Caillault again called their attention to the subject, and established by fresh observations the correctness of Bateman's designation.[2] Other observers have since then come to the same conclusion.[1] So that no doubt now exists that molluscum is really contagious. This fact is in itself an *à priori* reason for suspecting it to be due to parasitic growth; and what analogy would lead us to expect has been confirmed by microscopical research. M. Hardy has discovered the spores of a cryptogamous plant in the sebaceous contents of the shining semi-lucid tumours which characterize this affection.

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[1] Hardy, p. 155; Anderson, p. 38.

[2] Archives de Médecine, 1851; Fourth Series, t. xxvii.

We turn now from the vegetable to the animal parasites. Of these there are some half-dozen that prey on the human skin. There are the common flea and bug, each of which by its bite produces a characteristic but transitory eruption. Then there is the little red spider, or harvest-bug, which also produces a slight skin affection. Fourthly, there is the *Acarus folliculorum*, a curious little animal which infests the sebaceous glands, and is especially common in persons who suffer from acne punctata. Not that the acne is to be attributed to the acarus. In this case the parasite is blameless, and the proof is, that acne frequently occurs without there being any parasite present; and on the other hand, that the parasite is often to be found in glands which are perfectly healthy. But besides these which we have enumerated, there are two others which are more likely to require medical attention—the louse and the itch-insect, or, as it should rather be styled, the itch-spider, inasmuch as it possesses eight and not six legs. Of the former of these there are three varieties found on man—the pediculus capitis, pediculus corporis, and pediculus pubis, each variety taking its name from its special locality. It is, as Dr. Anderson remarks:

> "Very curious that these three species should live in such close proximity to one another, and yet in no case leave their own preserves for the purpose of poaching on that of their neighbours. One can understand why one insect should prefer hairy and another non-hairy parts, but not why one pediculus should find its appropriate soil on the hair of the head alone, while the Pediculus pubis attacks all hairy parts except the head."

Pediculi, of whichever kind, may exist on the body without causing any eruption whatsoever; but when they are present in large numbers they usually, by their irritation, give rise either to an eczema or a prurigo. It would be an error, however, to ascribe to pediculi every case of eczema in which they are found. They are as often the consequence as the cause of this and similar eruptions. The moist and diseased surface seems to offer some special attraction to them, and in the morbid secretions they find the food and other conditions most favourable to their rapid multiplication. For this reason it is that they are so constantly found to complicate favus and the impetigo of children's heads. It is, moreover, in these same cases, where they are engrafted on a previous eruption, that they are most difficult to deal with. The matted hair and the glutinous crusts protect them against the action of external remedies, and at the same time furnish a secure lodging-place for the ova. Under such circumstances the hair must be cut short, the crusts removed with poultices, and staphysagria applied either in powder or mixed with lard. By these means, and by a sedulous attention to cleanliness, it is seldom that success is not obtained, be the pediculi as numerous as they may.

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[1] Hardy, Malad. de la Peau, 2nd Part, p. 98.

There remains for our consideration the *Acarus scabiei.* The history of this creature is a curious one. So long back as in the days of Averroës, the Arabian, the itch was ascribed to the presence of an animalcule. Later writers repeated the statement, without, however, pretending, so far as we are aware, to have themselves seen the parasite which they described. Linnæus probably had not seen it, or he would not have described it as identical with the mite found in flour. He had noticed that the itch frequently attacks the buttocks of young children, and he attributed this to the application of flour to those parts by nurses. The real explanation, no doubt, is, that the hands of the nurse are, in carrying the child, in direct contact with its buttocks; and the hands are, as every one knows, the most frequent seat of the disease in adults. At the beginning of this century, medical men seem, as a rule, to have come to regard the itch-insect as a myth, when in 1812 M. Galés, who had been apothecary at St. Louis, demonstrated its existence in the presence of numerous medical men and students. The most distinguished entomologists and savans examined the specimens, and acquiesced. The treatise published by M. Galés had the honour of being "couronné" by the Institut; and the question of the existence of the itch-insect seemed solved for ever. But after seventeen years had elapsed, M. Raspail came forward and accused Galés of imposture. He showed that the insect which Galés had shown and pictured was nothing else than a cheese-mite! Galés had concealed this under his nail; then opened a pustule or vesicle with a lancet, and producing the animalcule, professed to have removed it from the patient. It was not till 1834 that the *Acarus scabiei* was established as a real existing animal. It was discovered by Renucci, in Corsica; and now that its position has been once clearly pointed out, is not very difficult to find. Galés, the impostor, had pretended to find it in the vesicles or pustules. This is in itself a sufficient proof of his falsehood. The acarus is not lodged in the vesicle or pustule, but at the end of a little furrow which it hollows out under the epidermis. The vesicle or papule, as the case may be, is due to the irritation caused by this operation. The canals or furrows are usually of a serpentine form, and can be most readily detected either on the hands or, in the male, on the penis. The search for them will be made easier by first carefully cleaning the skin. It is only when the cuniculus is found that we can be quite certain that the case is one of scabies; and it must be confessed that this cannot always be done, especially when the disease is only of short standing. Still, in such cases, the seat of the eruption and other attendant circumstances usually afford sufficient data for a practical judgment. Should any doubt remain, the case should be treated as one of scabies, and the effect of remedies, as Hippocrates said, will show the nature of the disease.

It would appear that it is only the female acarus that is lodged in a cuniculus or furrow. The male roves at large on the external surface. His spouse, so soon as she is impregnated, seeks out a suitable spot—

one, that is, where the epidermis is soft and delicate—and burrowing in, spends the rest of her days imbedded out of sight; for once in her "sillon" or "cuniculus," and egress is impossible. The long, stiff hairs which beset her posterior extremity effectually prevent all retrograde motion. In this retreat, then, she lives and lays her eggs, penetrating a little deeper as each ovum is deposited. These are in time hatched, and the larval acari make their escape from the furrow by its open end, and become the companions of their sires upon the free surface of the skin. It is doubtless by the passage of these larvæ from one person to another that the disease is transmitted. The mature female, as we have seen, is safely lodged under the epidermis, and cannot possibly make her way from one body to another. The adult male might pass over, but clearly by himself would be of little importance. It is to the migration of the young acari, or of the impregnated female before she has made her burrow, that the contagion is due. Luckily for doctors, this migration requires more than a mere momentary contact of a healthy with a diseased subject, otherwise each time they examined a patient they would be liable to infection. But speaking generally, scabies is only caught when the contact has been much more prolonged than this. In nineteen out of twenty cases, it is caught by sleeping with a person already affected, or by wearing his clothes; and this it is which, according to Hardy, accounts for the much greater frequency of this disease in winter than in summer.[1] In cold weather the labouring classes herd together, and sleep several in one bed; whereas in summer they isolate themselves, as far as their means will allow. For eighty cases which present themselves each winter morning, on an average, at St. Louis, only some ten are seen on a summer day. Moreover, it is those parts which are most exposed to prolonged or intimate contact which are usually first affected. We have before mentioned that in infants and young children the buttocks are, as a rule, earliest attacked. Doubtless this is due to the contact of the nurse's hand; and according to Hardy, the disease in adult men commences most frequently on the skin of the penis, and is thence transmitted by scratching to the fingers.

The treatment of scabies is very simple. The method most ordinarily used in this country is merely to apply sulphur ointment to the skin; and in time this plan, if used with perseverance, ordinarily succeeds; but it requires time, for the female, buried in her furrow, is out of reach of the ointment. Any plan which would first open her burrow, and so expose her to the action of external remedies, would clearly be much more efficacious. This is done at St. Louis, and the old tedious process has now been reduced to the short space of one hour and a half. Both from what we have seen in the French Hospital and from our own practice, we can vouch that this short time is sufficient for the cure of an ordinary case of scabies. During the first half-hour, the patient is rubbed well all over his body with good black soap. The second half-hour is spent in a warm bath. This, with the previous soaping, removes much of the scaly epidermis, and lays open the cuniculi. During the third half-hour the whole body is rubbed over with sulphur-ointment, to which subcarbonate of potash has been added in the proportion of half a drachm to the ounce. The patient must not wash the ointment off till the next morning, by which time he will be effectually cured. Of course, if he puts on the same garments he wore before, he may again be infected. To prevent this, the clothes should be exposed to sulphur vapour, or placed in air or water heated to 150° Fah. By these means both acari and ova will be destroyed.

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[1] Hardy, 2nd Part, p. 197.

In these brief remarks on the parasites which infest the human skin, we have purposely avoided all discussion of the anatomical lesions to which they give rise; these and the parasites themselves will be found fully described in the books which head this article. Of these books there are only two which will be found of much service by the medical man or the student: these are the treatises of Dr. Anderson and of Dr. Fox. Dr. Anderson's book in especial is a clear and well-written statement of all that has at present been made out on the subject; and the student will find there the views of Bazin and Hardy put before him in his own language. In Dr. Fox's treatise he will also find an account of the entophytes, or parasites which are found on the mucous surface. Of Dr. Ross's short treatise, a very small part, notwithstanding its title, has reference to parasitic disease. The greater part is occupied by an appendix on the constitutional relations of diseases of the skin, and by a certain number of cases related in a dramatic form. For instance—

Dr. R. How long have you had this?

Mr. D. I have had it occasionally before this, but never so bad.

Dr. R. How is your health?

Mr. D. I am all out of sorts; I have lost my appetite, &c. &c.

Mr. Hunt's 'Guide to the Treatment of Diseases of the Skin' is addressed rather to the unprofessional public than to the medical world; at least so we judge from the general style of the work, and from a notice begging patients not to take arsenical preparations without medical advice. As regards parasitic diseases, Mr. Hunt's views are by no means in accordance with those we have brought forward. Mr. Hunt is a follower in his classification of Willan, and thinks scabies more nearly allied to smallpox than to scallhead. He believes that the presence of a parasite, animal or vegetable, is a pure accident, and that no eruption was ever cured by destroying the parasite (p. 214). We cannot say that the case of favus, which he brings forward, convinces us. Mr. Hunt professes, in this instance, to have destroyed the parasite without curing the disease. How was its destruction effected? Mr. Hunt tells us that it was destroyed *easily*, "by applying oil of linseed, which soon detached the crusts; but the health remaining impaired, the vegetation grew again most rapidly." We should have been much surprised if it had not. The oil which removed the crusts had no more power to destroy the fungus in the hair-follicles and bulbs than the same quantity of water. Mr. Hunt does not even believe that scabies is due to the presence of an acarus, but ascribes it simply to dirtiness. Some of the arguments by which Mr. Hunt supports this opinion are so curious as to deserve notice. Mr. Hunt believes that the acarus is *not* the cause of scabies, because one of the pupils at St. Louis failed, in Mr. Hunt's opinion, to prove that it is. This pupil, M. Gras, among other experiments, made the following:—He placed on his skin seven living acari; after a time their introduction was followed "by some vesicular appearances."[1] One would have thought this was evidence, so far as it went, that acari can cause vesicular eruptions. But not so Mr. Hunt; and for this reason: "but the acari were then poisoned, instead of having the chance given them to quit their quarters for a new subject. The other experiments were equally inconclusive"!!

In bringing this article to a close, we think it due to Mr. Erasmus Wilson to state that it was written before his communication, published in our last number, had reached us. Mr. Wilson is not a supporter of the views we have set before our readers. He admits that this modern theory is a captivating one, and that it is not only popular in France and Germany, but gradually making way in England also; but he sees no valid reason for abandoning the conclusions at which he arrived some twenty years or more ago. The pathological forms which are now by all other writers regarded as the spores and filaments of cryptogamous plants, have, according to Mr. Wilson, no right to this vegetable character, but are nothing more than altered epidermis cells:

> "Favous matter and the mucedinales of the phytodermata are organic matter arrested in development at the lowest degree of life, the function of reproduction; the sporules are growing organic substance, aborted epidermic granules; the filamentary portion fully-formed organic substance, beyond which there is no further growth, the highest and perfected form of development."[2]

We have ourselves not the slightest doubt of the fungoïd character of the forms and elements in question. We would refer, however, such of our readers as may wish to see the opposite side advocated, to Mr. Wilson's communication, where they will find his theory stated at length.

REVIEW VII.

A Treatise on Hygiene, with Special Reference to Military Service. By WILLIAM A. HAMMOND, M.D., Surgeon-General United States Army, &c.—*Philadelphia*, 1863. pp. 604.

IN a preceding part of this number of our Review we have noticed Dr. Hammond's 'Physiological Memoirs' with the praise due to them. We little thought at the time that there would so soon be laid before us a work by the same author, bearing the title given above. Whether we consider the important office Dr. Hammond filled, or the peculiarity of the period when he composed it—not a time of peace and leisure, but, as we all too well know, of civil war in all its intensity—

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[1] Hunt, p. 137.

[2] P. 204 of our last number.

we cannot but express a feeling of surprise that he should have undertaken and completed it. Surely it is a remarkable example of energy, and, whatever opinion may be formed of its execution, we cannot refuse our respect and admiration towards a man capable of such exertion. He himself informs his readers that he gave to its completion the time he should otherwise have given to rest, and who can doubt it?

Republics have been notorious for ingratitude; and if it be true, as reported, that Dr. Hammond has been deprived of his high appointment of Surgeon-General to the United States Army, we shall have, we cannot but think, another example of the kind in this his dismissal. Were we to offer a conjecture as to the cause, we should attribute it to no worthy motives, rather to those of the baser kind—either to party feeling or to political jobbery, with which President Lincoln's Administration is charged, or to envy and jealousy, creative of clamour against an individual selected for his merit, and suddenly placed over the heads of old aspirants for office—men, probably, with little regard for science, addicted to routine, devoted to antiquated practices, and who must have looked with horror on calomel and tartarized antimony being excluded, as we learn from a circular of the Surgeon-General, from the regular lists of army medicines.

Of a work on so large and miscellaneous a subject as the one before us, and of so great a length, our notice must be comparatively brief. To criticize it with any minuteness would require more space than we can afford to give it. Of its general character thus much we can say: that it displays much ability, extensive reading, and original research. Dr. Hammond's previous career well fitted him for undertaking it. The years he passed in the public service as an army medical officer afforded him the requisite practical experience, and his after labours, as Professor of Anatomy and Physiology in the University of Maryland—where he so honourably distinguished himself—specially qualified him for a work of this kind, and has made the accomplishment of it, we may presume, comparatively easy. The manner in which the treatise is written bears evident marks of the different courses of life which its author has led. It may be viewed as consisting of two parts—one, the first, occupying about one-half of its pages, confined chiefly to general hygiene, which may be read with interest and benefit by all who wish for instruction on the all-important subject of sanitary science; the other, the remaining moiety, on the application of the science to military life in all its various and trying circumstances. This portion we cannot too strongly recommend to the army surgeon; it contains so much valuable information founded on the amplest data, and given in a manner equally clear and demonstrative.

When we take into account the interest of the work, the variety of matter it contains, the many branches of science from whence the author has derived his materials; and further, when we keep in mind the short time in which the volume has been composed, we ought not to be surprised if we meet with passages to which we cannot give our assent, and which are open to serious objection; but we must confess some little surprise in encountering others in which the author, who generally is so well informed, should have made statements not in accordance, as it seems to us, with the exact science of the day. We shall advert to some of these.

In Chapter I. of the third section, he describes atmospheric air as "a compound gas of variable density surrounding the globe, and dissolved in the water which constitutes a part of its matter." Surely this definition is very faulty. A mixture of gases, which it is, is different from a compound gas, and though water in vapour is a part of the mixture, it is not the solvent.

In the same chapter, when treating of the influence of atmospheric vapour on certain physiological processes, he states that "the quantity of urine excreted is greater in dry than in damp weather, for then the watery particles are given off in greater amount by the emunctories of the skin and of the lungs." Had the reverse been said it would have been correct, in accordance with experience and theory. We had, at first reading, supposed there was a typographical error, but we do not find it in the list of errata; and on re-perusal, taking in what preceded and what followed, we must refer it to the author's incuria.

Of the sirocco of the Mediterranean he says: "Its effects upon animal life are extremely depressing, and even plants droop and wither under its action." The latter part of this sentence is not borne out by fact, and the first is somewhat too strongly expressed. The sirocco being a moist as well as a warm wind, it is commonly felt as depressing, though we have known persons who liked it; but, owing to the same quality it has no parching, withering effects on plants. During harvest-time it is approved by the Maltese farmer, as it prevents the ripe grain from being shed—an effect he dreads from a wind of the opposite character, the south-west, remarkable for its high temperature and extreme dryness; and is at all times approved by ladies, who are more devoted to the guitar than to their ringlets.

In the Chapter on Light he states that "the tadpole, which under normal circumstances developes into the frog, when subjected to darkness does not undergo the transformation with the same degree of promptness, and may even be thereby entirely prevented from becoming a perfect reptile," founding his statements on Dr. William Edwards' experiments, confirmed by his own, unaware, as it seems, of those of Mr. Higgenbottom and of Dr. John Davy, proving that light alone has no positive effect on its development, which is chiefly influenced by the degree of heat and the quantity of food.

Treating of water for drinking, and of infusoria so commonly occurring in drinking-water, he says: "It may be said with truth that water in which infusoria do not exist is not the best fitted for ingestion; for so universally do they make this fluid their habitat, that their absence is *primâ facie* evidence that something is wrong in the water in which they are not found." Now, if this be true of the water in the United States, it certainly is not so of water in Europe. In spring-water of greatest purity we have sought for them in vain.

In the same chapter, when treating of the purification of water by filtration, he says, and truly, that "many substances held in solution can be thus removed from it," adding that "even the saline constituents of sea-water are separated by a filtration through a stratum of sand thirty feet thick," which we more than doubt. We have read statements of the kind in the narratives of old travellers; but we are acquainted with no experiments affording proof that such is the fact. In mines, the workings of which have extended under the sea, we have not found the droppings from the roof of the galleries sweetened by the transit; nor do we understand how water that is capable of extracting alkali from glass should be liable to have the soluble salts existing in the sea separated by sand, whatever the depth of its stratum.

In the Chapter on Compound Aliments, he states that "the white meat of birds owes its colour to the fact that it is in a state of fatty degeneration." We wish he had given proof of this. We have always understood that mere colour is an accident of muscle, and that if a survey of different classes of animals be made, the muscular fibre will more commonly be found colourless than coloured. To come to particular instances, it would be singular indeed were the pectoral muscles of the partridge and pheasant, and the femoral of the woodcock, so constantly white, in the state of change asserted. Even in the pectoral muscles of the barn-door fowl, whenever we have examined them, we have found them excellent examples of striated muscle, without any indication of degenerative change. Whether in America these muscles are more subject to disease, we are ignorant. Once we remember, in the West Indies, having to help a turkey, the breast of which was almost all fat, and a lady Creole of the island, who sat beside us, gave us to understand that the occurrence there was not uncommon.

In his Chapter on Food, when considering it in relation to climate, he justly remarks, that "the amount of food ingested by the inhabitants of warm climates is less than that taken by the residents of cold ones," adding: "The East Indian lives on a little rice, whilst the Greenlander eats several pounds of fat meat daily." All those who have had experience in India will smile at the notion of this scant Hindoo fare. It is true that somewhat less food is required in a tropical than in a temperate climate, and still less than within the frigid zone; but there is much exaggeration in the idea that a low nutritive diet, such as rice, and small in quantity, will suffice a native of the tropics. Besides rice, of which the East Indian consumes a considerable quantity, he uses ghee and pulse, and other nourishing vegetables. The ration of the British soldier in India is even more liberal than when he is serving at home.

These are a few of the errors, as they appear to us, which we have met with in the careful perusal which we have given to Dr. Hammond's work. Should this review ever meet the eye of the author, we hope he will reconsider the passages and correct them in a second edition, which we do not doubt will soon be called for. The book for its sterling merits deserves to be an authority; any blemishes must weaken it as such. We have more pleasure in directing attention to the excellences of the volume and to some of the original observations of the author by which it is enriched. We shall pass over certain passages on which we may hold a difference of opinion—in a work of such extent almost unavoidable, there being so many subjects involved respecting which men of science and of the highest rank still differ, such as the unity of the human race, the nature of malaria, the contagion of certain diseases, the influences of climate, &c.

We have said that the first portion of the work *chiefly* relates to general hygiene; we lay emphasis on chiefly, inasmuch as even in this part the author's main intent is kept in view; indeed, the first chapter is as special—and it is a very able one—as any of the latter. Its subject is the examination of recruits. Urging the necessity of a careful examination of men for military service, and the consequences of neglect of it, he refers to his own experience, which is interesting as illustrating the character of fresh and hastily-made levies and of volunteer armies. He says:

> "The present rebellion has opened our eyes to the evils flowing from indiscriminate enrolment of men unfit from physical infirmities to undergo the hardships incident to a soldier's life. Thousands of incapacitated men were in the early stages of the war allowed to enter the army, to be discharged after a few weeks' service, most of which had been passed in the hospital. Many did not march five miles before breaking down, and not a few never shouldered a musket during the whole time of their service. In a hospital under my charge, containing six hundred beds, I discovered at one time, on inspection, fifty-two cases of inguinal hernia in men who had undergone but an insignificant amount of exposure to hardships. Cases of chronic ulcers, varicose veins, epilepsy, and other conditions unfitting men for military life, came frequently under my notice. The recruits were either not inspected at all by a medical officer, or else the examination was so loosely conducted as to amount to a farce. I know of several regiments in which the medical inspection was performed by the surgeon walking down the line and looking at the men as they stood in the ranks. Not long since a case was reported to me by an intelligent surgeon in which the colonel of the regiment to be inspected refused to allow the men to be stripped to undergo examination."

Adverting to weight as of importance in estimating the powers of a soldier, Dr. Hammond points out a difference deserving of being known and kept in mind between the American and European:

> "For corresponding heights," he states, "American soldiers are not so heavy as those of European armies. The former do not grow laterally to the same extent as the last-mentioned, and hence their deficiency in weight—a defect, however, which, while it makes them less capable of enduring long-continued fatigue without succumbing to its influence, renders them more able to perform labours in which activity and rapidity of movement are necessary."

In the chapter on "The Accidental or Non-essential Constituents of the Atmosphere," the author offers some excellent remarks. Relative to carbonic acid, one of these constituents, we think he proves, though we can hardly admit that it is a non-essential, that, *per se*, it may be breathed with impunity when diluted with a sufficient quantity of oxygen and free from the vitiating influence of organic effluvia:

> "I confined," he says, "a sparrow under a large bell-glass having two openings. Through one of these I introduced every hour 1000 cubic inches of an atmosphere containing 45 parts of oxygen, 30 of nitrogen, and 25 of carbonic acid, allowing the vitiated air in which the animal had respired to escape. At the end of twelve hours the bird was in as good a condition as at the commencement of the experiment, and when the bell-glass was raised it flew away as if nothing had happened to it. A mouse subjected to a similar experiment also suffered no inconvenience."

Malaria has the attention given to it which its importance deserves. Of the two principal theories which have been advanced to account for it, Dr. Hammond seems to favour most the one that attributes its production, not to gaseous emanations from the decomposition of vegetable matter, but to the action of minute poisonous fungi floating in the atmosphere, and inhaled in respiration. In support of this hypothesis he gives an account of some very interesting observations made by Dr. Salisbury, of Ohio, on the production of measles by fungi arising from straw in a state of partial decomposition. The use of such straw in bedding was found to excite the disease amongst the troops; and fungi taken from the straw, applied by inoculation, was ascertained equally to cause it, and to afford protection from an attack of the malady when measles were prevailing in camp epidemically.

In the chapter on Temperature, Dr. Hammond, when describing the effects of cold as a morbid agent, adduces his own case in corroboration of the reported instances of its benumbing influence:

> "On one occasion," he says, "I was nearly overcome by an intense desire to sleep produced by a sudden change in temperature by which the thermometer fell in about two hours from 52° to 22° Fah. I was crossing the mountain ridge between Ceboletta and Covero in New Mexico, and if I had had much further to go I should probably have succumbed. As it was, I reached a rancho in time to be relieved, though it was several minutes before I could speak. The sensations experienced were far from being disagreeable, and with all these there was a feeling of recklessness of consequences that made it a matter of indifference whether life was preserved or not."

It seems happily ordered, we may remark, that whilst one degree of cold is so stupifying, a less degree, as if forewarning, occasions painful sensations. We well remember in ascending Etna, a boy, a young muleteer, entering the Casa Inglese, in the loftier region of the mountain, crying under the influence of pinching cold, and how a small glass of spirits had the immediate effect of changing his doleful to a cheerful state, shown by his laughter. It is on occasions of this kind that the stimulus of spirits, used in great moderation, by exciting a more vigorous circulation, and by that means warming the extremities and probably the nervous centres, is so beneficial. We believe that the members of the Alpine Club are well indoctrinated in this matter.

As perhaps might be expected from what is already well ascertained relative to the influence of the seasons on the health of the people of the United States, the Federal troops during the last two years have suffered less from disease in winter than in summer. The army returns referred to by Dr. Hammond show this in a striking manner, and are remarkably contrasted with the returns of our army during the war in the Crimea; but it is to be kept in mind, that whilst our troops during the first winter were all but starved, the Federals have been most amply provisioned.

In the chapter on Climate the author discusses at some length the influence which it exercises on phthisis. He confirms, from his own experience, a statement referred to in a former volume of our Review when noticing the medical statistics of the United States' army, that a very dry atmosphere is most conducive to the prevention of the disease, and to its cure in its incipient stage, and this though the cold may be great in winter and the temperature high in summer. The following is his experience of a dry climate, of equably low temperature, which as bracing and tonic he prefers to one of a uniformly high temperature. He says:

> "From my own observations, I am able entirely to confirm the deductions arrived at by Dr. Coolidge. I have known several persons afflicted with phthisis pass the winter at Mackinac with very decided advantage. The climate there is cold and dry. The mean temperature of winter, as deduced from observations made during twenty-four years, is 20·03°, and for the whole year, 40·65°. The main quantity of rain for the same season (snow being melted and measured as water) is but 3·31 inches, and for the whole year but 23·87 inches, less than the average at any other military station of the United States, except those situated on the prairies west of the Mississippi and in New Mexico, and some parts of California and Oregon. Under the influence of the climate of Mackinac, both in summer and winter, I have witnessed all the symptoms of phthisis become ameliorated or entirely disappear, at the same time that the body improved in condition and strength. Of course, in cases in which the disease was far advanced, the same favourable results were not to be expected; but even here a very marked improvement was manifested."

He adds:

> "In a service of three years in New Mexico, during which period I served at eight different stations, ranging from the extreme northern part of that territory, I saw but three cases of phthisis, and these were in persons recently arrived from the United States. Inflammation of the lungs is also very infrequent, as are likewise pleurisy and bronchitis."

He quotes Surgeon J. F. Hammond's experience as similar. He had never seen a case of phthisis in New Mexico, with the exception of two, in officers who were labouring under the disease at the time of their arrival, and they in its dry and equable atmosphere improved there.

When treating of acclimatization, there are amongst other valuable remarks of the author some respecting diet in relation to climate, and incidentally respecting scurvy. He quotes Dr. Hayes, his countryman, well known amongst the distinguished arctic explorers, for the good effects of raw meat as an antiscorbutic. He says: "Dr. Hayes has frequently found that the stomachs of scorbutic patients which rejected the cooked meats retained the raw." He observes that the repulsiveness of raw meat is entirely destroyed by freezing, and he reminds his readers that raw meat is best borne by the stomachs of children labouring under cholera infantum.

The subject of hospitals, as might be expected, has had Dr. Ham mond's very careful attention. This is a part of his work which is peculiarly valuable. His descriptions of various hospitals both in America and in Europe are accompanied and well illustrated by plans. In his critical remarks he points out the defects of some, and the excellences of others. He prides himself, and justly, on his country having had, during the war, some of the best, as well as largest, that have yet been anywhere constructed. He well remarks, that "a perfect hospital has never yet been built, and perhaps never will." As the nearest approach to such a one among existing hospitals, he selects that of the Episcopal Hospital of Philadelphia. He says, "In architectural finish and in completeness of detail, in all that regards the comfort and hygienic condition of the patients, it is not excelled by any other in the world." The largest hospital, and one of the best that has been built during the war—and this he claims as the largest in existence—is that of Chestnut Hill, within the city limits of Philadelphia, which contains 2820 beds for patients, besides 500 for the officers, stewards, nurses, cooks, &c. It was organized by the medical officer who has charge of it, and is conducted under his superintendence, he says, in the most efficient manner. The name of this meritorious public servant is Surgeon Jos. Hopkinson, of the United States Volunteers. The personnel of this vast establishment consists of 30 medical officers, 8 hospital stewards, 3 chaplains, and 495 cooks, nurses, and other attendants, with a guard of 86 men. In common with the general hospitals there of the same date, it has dining-rooms apart from the wards, a chapel, a library, and reading-rooms. It has, morever, its magnetic telegraph and railway, and a fire-alarm apparatus, connecting all the wards and offices with the office of the surgeon in charge. Dr. Hammond points out the evils of leaving the designing of hospitals and barracks to engineer officers, men hitherto ignorant of sanitary science, unassisted by the advice and suggestions of medical officers. This, we may remark, is unquestionably the cause of the very defective construction of so many of our own hospitals, both at home and in our colonies. If any observations can convince our government authorities of the impolicy of such a proceeding, those of our author should accomplish it; his work can hardly fail of being consulted. For a most valuable amount of information on this subject, comprising details respecting permanent and temporary, general and field hospitals, also concerning barracks and encampments, we can with confidence refer to the work itself. It is no small satisfaction in witnessing the progress that has been made in the improvement of these establishments of late years, to see how war has its alleviations, and how the cause of humanity and civilization is advanced even in the midst of its horrors. We too well remember the defects of our own military hospitals only a few years ago, many of them not yet corrected; and we cannot forget what we have witnessed in our wanderings, in the hospitals of the Levant, especially in Constantinople, where in some instances, owing to bad ventilation and neglect of cleanliness, they were more adapted to engender disease than to promote recovery. We have seen their surgeons going their rounds accompanied by attendants bearing braziers, in which aromatic herbs were burnt to disguise the stench during the dressing of sores.

Food and clothing, other two great elements of hygiene, and of importance equal, if not superior, to that of shelter, are ably and amply discussed by Dr. Hammond. This portion of his work, which occupies 134 pages, is not less deserving of careful perusal by the medical officer, and may be read with advantage by others. The principles which he lays down both regarding diet and dress, seem to us sound and in accordance with science and experience. When treating of the soldiers' rations, he gives examples of those of the troops of different nations, and also of their hospital dietaries, from which it appears that those of the United States' armies are, as to quantity and variety, most liberal. Amongst the more important articles of food supplied to the troops in the field are preserved meats and milk, desiccated fruits and vegetables. Another noteworthy circumstance is, that whenever a permanent hospital or barrack is established, there a garden is made and cultivated for the supply of fresh vegetables. We could wish that the members of "The International Temperance Prohibition Convention" would read what our author has written respecting "accessory food," on which he has a distinct chapter. Under the title are comprised those articles of low status as aliments, yet extremely useful either as making the food more savoury, as promoters of digestion, or as agents for developing nervous or muscular force—such as, to mention the principal, pepper, cayenne, mustard, and vinegar, alcohol in its various forms, tea, coffee, and even tobacco. Of course, it is the use of these that he advocates; their abuse he deprecates. He holds—and we think on correct data—that wine, not alcohol or ardent spirits, is something more than a stimulus of the circulating and respiratory functions or than the retarder of tissue metamorphosis:

> "More than any other alcoholic liquor," he observes, "it acts as a soother and restorer, and this, when used in moderation, without the production of any injurious effect. On the contrary, it would appear from the observations which have been made by many distinguished physiologists, that those who drink good wine, with due care to avoid excess, will, other things being equal, live longer, and to better purpose, than those who entirely abstain."

For details on food and clothing, especially the rations and dress of the soldier, we must refer, as in the instances of hospitals and barracks, to the work itself. Under Dr. Hammond's advice, in his administrative faculty as surgeon-general, we believe the dietary of the United States' vast armies has been in some measure regulated as well as their hospital establishments; and it speaks well for their efficiency that, during the protracted war, unfortunately still raging, the mortality from disease (more than 100,000 cases have come under treatment) has been, it would appear, proportionally less than in any campaign of modern times of which we have correct statistics.

We must not conclude our imperfect notice of this work without expressing the pleasure we have had from its perusal. It might be briefer, with a view to immediate use in the field, but had it been as brief as it might have been made as a hand-book for the military surgeon, it would have been, as a treatise on hygiene, far less interesting, and less readable; debatable subjects would have been left undebated, science would have merged into art, inquiry probably into dogmatism; little scope would have been afforded for criticism, or for more than commonplace approval or disapproval of the author.

REVIEW VIII.

A Practical Treatise on the Diseases and Infirmities of Advanced Life. By DANIEL MACLACHLAN, M.D., Fellow of the Royal College of Physicians of London, late Physician and Principal Medical Officer to Chelsea Hospital, &c. &c.—*London*, 1863. 8vo, pp. 713.

IT might very naturally be urged by those of the community whose earnest lives have mainly been employed in exertions and forecast for the welfare and security of others, that a due share of medical attention has not been exercised towards alleviating the bodily inconveniences of the aged, and warding off the frequent assaults of that enemy whose eager approaches, not less than in earliest infancy, menace the period of social existence which we accustom ourselves somewhat too lightly to speak of as the "season of regrets." It is then, indeed, that death and disease may be said to come on stealthily, "like a thief in the night," and the history of pneumonia, pericarditis, heart complaint, apoplexy, eminently evidence this truth. The volumes before us contain many instances characterizing the insidiousness of morbid change when it is accompanied by that obtuse or depressed condition of the nerves which we refer to a lower degree of irritability; the human frame no longer as before, under changes of structure and function, being capable of those loud and lively manifestations which are displayed in youth and maturity. There are also phenomena and conditions of decay which, if not quite so exclusively as has been believed, are at least very generally witnessed in the aged. Metropolitan repute has, it is true, at all epochs pointed to particular physicians as better skilled and more experienced than their class in warming up the embers of life and repairing the failing links which hold the soul to its earth; and eagerly is such aid sought after whenever, as is its wont, society learns to value these fading existences in proportion as it can less reward by its distinctions or allure by its deceits. But, however we consider it, medical literature among us until now has entirely failed in a response to the preceding argument. That which the intelligent Professor of St. Andrew's[1] had the merit of perceiving and initiating in this country, Dr. Maclachlan has since carried out, and in the performance has displayed an industry, ability, and perseverance which justifies our praise and also the privilege he has so long been fortunate to enjoy in the study of this class of diseases at the Royal Hospital at Chelsea. Indeed, this field could hardly have found a more careful expositor than he appears to be in the volume before us. What at first discourages from its bulk will, on examination, be found to comprise a masterly compendium of each division of the subject. His extraneous stores of learning have been well arranged for his intent and our use; and however much we may have desired more ample detail of his personal experience, he has by confirmation, selection, or addition, so welded his information into one mass, that for a book, as books are used now, it is perhaps more generally serviceable, complete, and better so. Of the sources to which he is principally indebted for suggestion and comparison, he has mentioned Canstatt[1] with especial eulogy; but, in short, never was literary production freer from the taint of egotism than the one before us.

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[1] Day on Diseases of Advanced Life.

In his prolegomena our author has considered at large, and somewhat abstractedly, the conditions of advanced mortality. He regards shortness of breath on exertion as the most characteristic of many features of life's decline, connected as it is with normal changes in the chest and lungs and failing nervous power. The advance of age operates, he believes, more rapidly and demonstratively at early periods on men than on women, contrary to the opinion received by the crowd. We then find noticed the diminution which takes place in the quantity of the chief constituents of the blood (with an increase of cholesterine, on the authority of Becquerel and Rodier), and a similar diminution in the secretions leading to constipation and dryness of skin, &c. The heart alone among the organs retains an undiminished size, and the pulse does not fail in strength; fatty degeneration is the common intruding element preceding other change. In the teeth of the Registrar-General's returns, however, our author refuses to admit that advanced age is an abnormal or exceptional condition accounting for death. Out of thirteen hundred casualties at Chelsea Hospital, their average age being seventy years, there was invariably found some diseased condition to account for dissolution. Bronchitis, pneumonia, apoplexy, and also phthisis, figure in the first rank of frequency, while typhoïd fever is scarce ever seen. Acute rheumatism is extremely rare; so, too, is pure neuralgia; and tonsillitis scarcely ever reaches suppuration. "I do not remember ever to have seen such an instance above sixty." The decay in the vessels naturally leads to head affections. It is only occasionally that inflammation is sthenic. Erysipelas and herpes zoster, the latter a frequent and serious complaint in old persons, are characterized by a dull brown and even livid hue. The ratio of death from typhus in old persons attains a frightful proportion, and as respects them coldness of temperature stands ever in firm alliance with death:

> "If," says our author, "there is any period more than another requiring the closest scrutiny to unravel disease, it is the period of old age, where there is no symptom sufficiently developed to guide and direct the practitioner. It

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[1] Die Krankheiten des höheren Alters und ihre Heilung, 1839.

will scarce be credited by those who have not had the opportunity of observing to what extent active disease may exist and proceed with little or no suffering or alarm, and without the usual symptoms."

The so-called climacteric decay, which has received attention from able describers, Dr. Maclachlan believes to be very rare as an idiopathic affection; it is rather the engrafting of some malady on a senile constitution, granular degeneration of the kidney playing a large part as its constituent.

We hasten to consider the body of the work, and, firstly, of the nervous system. The susceptibility to purely nervous diseases declines with declining years—even tetanus and epilepsy follow this rule. There is no chapter of this work more interesting than that which treats of meningitis, very commonly confounded with other disorders of the aged. It is a disease of rich and poor alike. We have ourselves experienced difficulty in diagnosing it from delirium tremens, and receive an explanation in the fact that this latter disorder in old people is often accompanied by inflammation in the meninges.

The different forms of apoplexy are carefully treated of. Especially worthy of note are cases in which the symptoms are masked. The author's experience furnishes singularly striking instances. Vomiting accompanied with syncope in old persons should always arouse suspicions of head mischief (for a case in point, see p. 143). Sudden serous effusions seem to be extremely rare. In the chapter on meningeal apoplexy he remarks on the violent pain which occurs in some cases of intra-arachnoïd effusion:

> "In many cases the individual is seized with violent pain in some part of the head, and, uttering a loud scream, falls to the ground, deprived of consciousness, paralyzed on one side, convulsed on the other. Occasionally in aged females, and sometimes in males also, the immediate attack is announced by hysterical phenomena, weeping, and a sense of suffocation from spasm of the gullet recurring also from time to time on any temporary suspension of the apoplectic symptoms; and fatal coma may suddenly ensue when the patient is supposed to be merely hysterical. This, however, is no uncommon event in cerebral hæmorrhage. The pain in the head generally continues after consciousness has been restored, but often it entirely disappears or assumes a periodic form."

There is an excellent chapter on ramollissement of the brain and its diagnosis, which we regret that our limits do not allow us to discuss; and for a singular case of paralysis agitans we will refer to p. 216. This division of the work teems with interest.

In treating of respiratory diseases, Dr. Maclachlan acknowledges his great obligations to Hourman and Dechambre[1] for anatomical characters. We have before us an exhaustive treatise of bronchitis in the aged of forty-three pages, and fifty on pneumonia. Acute bronchitis supervening on asthma from emphysema and cardiac complication is instanced as one of the most frightfully painful and distressing complaints to which man is liable. In pneumonia, Dr. Maclachlan has found it necessary to give a particular description of hepatization and softening according as it is modified by age, expanded from Laennec and Andral:

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[1] Archives Gén. de Méd., Août, 1835.

> "My own experience," he says, "entirely accords with the statement of Grisolle, that in almost all subjects between the ages of fifty and seventy, it is only developed consecutively to acute and chronic inflammation of the bronchi. The disease occasionally sets in with well-marked symptoms, almost habitually announcing it at the middle period of life; and even at eighty and upwards it sometimes assumes a character of intensity that could hardly be expected at so advanced an age. It then only differs from pneumonia in the adult by the rapidity of its progress, the greater prostration and the brevity of the formative and fully developed periods. The rigors, the pain in chest, febrile commotion and tension of the circulation, are also more speedily followed by nervous symptoms, and, in unfavourable cases, the patient soon sinks into a comatose typhoïd condition, from which he hardly ever recovers. . . . . In the Chelsea Hospital," our author goes on to say, "I have seen men present themselves at the infirmary, after endeavouring to work in their gardens, complaining only of some oppression in the chest, or of general weakness, without any symptom referrible to the chest, when a physical examination discovered, perhaps, a portion of one or both lungs hepatized. Nay, cases have occurred to me in which men must have been thus going about with a portion of one or both lungs in a state of suppuration."

A striking example is adduced: It is from delirium, especially of a low muttering kind, with duskiness of countenance, that we anticipate an unfavourable termination, and double pneumonia in the aged would appear to be invariably fatal. There are admirable directions for diagnosis, borrowed from Grisolle, in the masked forms of pneumonia. A rigor, followed by high fever, inexplicable from other cause, should, in the aged, always point to pneumonia as its most probable cause. As regards treatment, venæsection, in some cases, is "imperatively demanded," nor should we look to years alone. Dr. Maclachlan does not ignore the reality of "adynamic wave," which is influencing our present practice, but in certain cases he both recommends bleeding and employs counter-irritation. The aged bear emetics better than might be supposed. Antimony only in small doses cautiously exhibited, with ipecacuanha rather largely employed, mercury, with opium, at an earlier stage than with adults, and a good amount of support, constitute his treatment.

Pleurisy and pleuritic effusion display the same insidious character as pneumonia.

Phthisis, it is asserted, is a very frequent disease in the aged. Ulcerous phthisis without tubercle is almost peculiar to them; in the tubercular cases a catarrhal form is frequent, the laryngeal rare, so also the acute form is rare, the latent common. Chronic phthisis often wears the garb of a pectoral catarrh, the cough seldom is distressing, and the expectoration rarely aids diagnosis. Hæmoptysis, not a frequent symptom, is prone to occur at the close instead of at the commencement of the disorder. The patient's breathing is often that of asthma. It is to be remarked, too, that hectic fever and diarrhœa are seldom strongly marked features. Emaciation forms a prominent feature in the complaint; the fatty liver does not occur in the old. In many cases of phthisis among them, the diagnosis is exceedingly difficult, in others "wholly impossible;" it is, indeed, more curious than important as regards results, but certain leading indications are given (see p. 347).

Our limited space warns us to reduce our reflections on diseases of the heart to the shortest possible terms. We find quoted from Bizot, and corroborated, that the orifice of the pulmonary artery becomes proportionately smaller than the orifice of the aorta, from a progressive enlargement of the latter. Our author also follows Bizot in considering the white spots, so frequently seen on the heart's surface, to be sometimes non-inflammatory—i.e., occurring merely from interstitial change. Dr. Maclachlan points out a vascular injection, which is often perceivable external to the root of the heart, in the base of the great vessels, occurring, it would seem, with greater frequency in chronic pulmonary affections, and which is probably of the nature of varix.

The average pulse in old age is in reality far more frequent than many of us believe; it varies much toward either extreme; insufficiency of the aortic valve would seem to lead to its increased, and fatty degeneration rather to its diminished frequency, as well as irregularity of rhythm; the impressions of cold influence its individual character to a remarkable extent.

Pericarditis assumes an obscurity even greater than its wont in aged frames, and its attrition sounds at that time of life have little distinctness; in this class of affections our author considers the use of mercury rather injurious than beneficial. On the whole, Dr. Maclachlan maintains that disease of the heart is less menacing than in adults with regard to its result, and differs from Littrè, we think justly, in considering regurgitant disease more dangerous than the obstructive forms. He quotes Bouillaud, as to digitalis being "the true tonic of the heart," and mentions opiate preparations with a strong warning. In this last respect, we think, he shows a caution beyond what fair practice will warrant, considering how distressing a complication the loss of sleep is in many of these cases, but we must remember that Trousseau has expressed himself far more strongly. Dr. Maclachlan does not seem to us fully to apprehend the benefit that may be drawn from the extensive use of iron, of which we have been accustomed to avail ourselves in this class of disorder.

Turning to more varied forms of disease. In spasmodic dysphagia, death is referred to sympathetic spasm of the rima glottidis, fully as much as to pressure of ingesta. In treating of alvine flux, mucous diarrhœa is said to be infrequent in the aged, if we except that discharge of ropy mucus which so often accompanies hæmorrhoïds in them. Pyrexial diarrhœa is always dangerous. In ileus our author does not exclude the use of the lancet, and he mentions cases of cancer of the lower bowel and outlet (p. 541), entirely free from pain. A bilious attack he warns us, is full of peril to old people, if it should extend to inflammation of the gall-ducts and gall-bladder, and under this apprehension such an indisposition should always be regarded with anxiety.

After remarking that albumen is very frequently present in the urine of the old, our author, contrary to the Registrar-General's returns, states that in his sphere of observation he has found granular degeneration of the kidney to be pretty frequent. Anuria, urodyalysis senum, and ischuria renalis, are treated of in a full, if not original, manner, and attention is directed to Lecanu's observations.[1] The amount of fixed salts being less than one-half in the aged of the amount excreted in the urine of adults, of uric acid also about one-half; while of the secretion of urea in very old men of eighty-four to eighty-six years of age, it is actually less than one-third of what occurs in children of eight years of age, and of the amount of urine it is in bulk far less than half that of middle life. Anuria appears to arise from imperfection of the depurating function of the kidney and retention of the urinary elements in the blood; headache, dyspepsia, and muscular pains being the most prominent symptoms, and various cutaneous eruptions of a pruriginous character, among the most troublesome of its manifestations.

Ischuria renalis is probably often overlooked in feeble bedridden subjects. "It is one of the rarest diseases to which man is liable;" it is almost always an acute disorder, seldom lasting more than ten or twelve days. It sets in with gastric or intestinal disorder; when the kidneys cease wholly to secrete, stupor comes on to close the scene. Diabetes mellitus often assumes an intermitting form in old subjects, and the disorder is less obtrusive than in adults. A too great severity in regimen is deprecated.

In cases of paralysis of the bladder, Dr. Maclachlan takes advantage of the sympathy which exists between the umbilicus and that viscus,—a sympathy which is remarkable in some subjects—to institute a particular mode of treatment:

> "I have for many years been in the habit of recommending the stimulant lotions, or embrocations, as employed, to be applied to that region instead of to the sacrum or hypogastrium, with the best effects. I have known a single blister, not larger than a crown-piece, placed over the navel, to be followed in a few hours by restoration of the lost power of the bladder, not once, but again and again, though complete retention, requiring the catheter, had existed for ten days or a fortnight. The acetum cantharidis is a most convenient application, and should be brushed round and round and into the navel. Tickling this part with a feather, or more roughly irritating it with the nail, sometimes produces pricking pains in the hypogastrium, extending along the penis, with urgent desire to pass water, which can then occasionally be accomplished."

It would appear that acute rheumatism recedes in frequency in an unmistakable degree after the age of fifty. The use of iodide of potassium in chronic forms of the complaint requires to be closely watched; though useful where there are fibrous thickenings, "it disagrees with many old people, if long continued." In painful forms, the employment of aconitine is recommended both externally and internally.

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[1] Quoted by Dr. Bird on Urinary Deposits; see also Simon's Animal Chemistry, published by the Sydenham Society, vol. ii. p. 166, 167.

As regards gangrene, we shall select the following passage:

> "In the majority of cases occurring in the more advanced periods of life, it seldom goes much beyond the toes. Very generally in old, broken-down subjects, previously exhausted by chronic maladies, death ensues before the disease has even wholly destroyed the toe attacked. For one case, in which it reaches the ankle, perhaps eight or ten perish. In almost all the examples I have seen terminating fatally, the patient has been carried off whilst the disease was still limited to the toes and the fore part of the foot; but some very old people have survived for weeks and months after it has reached the leg."

Dr. Maclachlan writes in further confirmation of the fact that, in the scurvy of old subjects, their toothless gums remain in a healthy condition; it is only round a remaining tooth that fungous vegetations are developed.

In a general view of his therapeutics, while admitting a change to have occurred in the type of disease, he does not find that he has occasion to modify principles of treatment which include such variations of type. He justifies venæsection in certain cases of bronchitis, pneumonia, apoplexy; in hernia and ileus he does not forbid it; nor even in erysipelas, though he has never met with a case requiring bleeding in the latter disorder. Tartar emetic is less controllable than bloodletting; he gives a warning notice on the subject of mercury. His general practice seems moderate, yet not inert. To descend to detail, the common senna purgative is depreciated in favour of castor-oil, and of the pulvis jalapæ compositus; and quinine gives place to the pleasant bitters columba, cascarilla, chiretta, which have so long been dispossessed by their powerful successor and rival. To the internal use of chloroform he yields great confidence, as far as experience has yet justified it, as well as to its inhalation in asthmatic attacks. Arsenic, he thinks, is a remedy ill borne by the aged; and he so far condemns the use of strychnia as to leave himself scarce a *locus pœnitentiæ*, should he henceforth be tempted to employ it. With these reductions, ample means still remain. In hygienic measures, the use of spun silk over flannel under-clothing is a recommendation on which he strongly insists. Considered as an "article de luxe," the spun-silk tunic shirt has had but a limited circle of use in this country and the continent: as it is procurable at no exorbitant price, we would gladly see it enjoy that hygienic application which is pointed out.

In reviewing a book containing such a variety of facts and views, we have not, owing to want of space at our disposal, attempted to do more than to give our readers an outline of its contents. We, however, commend it most heartily to the profession.

Review IX.

1. *Gazeta Medica de Lisboa.* Redactor Principal Dr. Pedro Francisco da Costa Alvarengo. Publica se nos dias 13 e 28 de cada mez. Imprensa Nacional.—*Lisboa*, 1862–3.

Medical Gazette of Lisbon. Principal Editor Dr. Peter Francis da Costa Alvarengo. Published the 13th and 28th of every Month. Printed at the National Press.—*Lisbon*, 1862–3.

2. *Jornal de Pharmacia e Sciencias Accessorias.—Lisboa.*

Journal of Pharmacy and the Accessory Sciences.—Lisbon.

3. *O Escholiaste Medico.—Lisboa.*

The Medical Scholiast.—Lisbon.

4. *Compendio de Materia Medica e de Therapeutica.* Por Caetano Maria da Silva Beirão, Lente de Materia Medica e de Therapeutica na escóla Medico-Cirurgica de Lisboa, &c. Tomo 1^{mo}.—*Lisboa*, 1862.

Compendium of Materia Medica and Therapeutics. By C. M. da Silva Beirão, Professor of Materia Medica and Therapeutics in the Medico-Chirurgical School of Lisbon, &c. Vol. I.—*Lisbon*, 1862.

5. *Estudos sobre a Hemeralopia a Proposito dos Casos observados no Quarnicão de Lisboa, offerecidos á Academia Real das Sciencias de Lisboa.* Por João Clemente Mendes, Cirurgão de Brigada, &c. —*Lisboa.* 1862. 8vo, paginas 80.

Studies on Hemeralopia in reference to Cases observed in the Garrison of Lisbon, and offered to the Royal Academy of Sciences of Lisbon. By John Clement Mendes, Brigade-Surgeon, &c.—*Lisbon*, 1862. 8vo, pp. 80.

6. *Anatomia Pathologica e Symptomatologia da Febre Amarella em Lisboa no Anno de* 1857. Memoria apresentada á Academia Real das Sciencias de Lisboa em Julho de 1860. Pelo suo Socio Effectivo Dr. Pedro Francisco da Costa Alvarengo, Medico da Camara de suá Magestade, do Hospital de S. José, &c. Com 6 Mapas, 15 Taboas.—*Lisboa*, 1861. 8vo, paginas 338.

Pathological Anatomy and Symptomatology of Yellow Fever in Lisbon during 1857. Memoir presented to the Royal Academy of Sciences of Lisbon in June, 1860, by its Fellow, Dr. Peter Francis da Costa Alvarengo, Physician in Ordinary to the King, and also of St. Joseph's Hospital, &c. With 6 Maps and 15 Statistical Tables.—*Lisbon*, 1861. 8vo, pp. 338.

7. *Estudos sobre o Garrotilho ou Crup.* Memoria apresentada á Academia Real das Sciencias de Lisboa. Por Antonio Maria Barbosa, Socio Effectivo do mesma Academia, Cirurgião Honorario da Camara de suá Magestade Fidelissima, Lente da Escóla Medico-Cirurgica de Lisboa, &c.—*Lisboa*, 1861. 4to, paginas 189.

Studies on Garrotilho or Croup. Memoir presented to the Royal Academy of Sciences of Lisbon. By ANTHONY MARIA BARBOSA, Fellow of the same Academy, Honorary Surgeon to his Most Faithful Majesty, Professor in the Lisbon Medico-Chirurgical School, &c.—*Lisbon,* 1861. 4to, pp. 189.

8. *Memoria sobre a Tracheiotomia no Garrotilho.* Apresentada á Academia Real dãs Sciencias de Lisboa. Por ANTONIO MARIA BARBOSA, &c.—*Lisboa,* 1863. 4to, paginas 231.

Memoir on Tracheotomy in Croup. Presented to the Royal Academy of Sciences of Lisbon. By A. M. BARBOSA, &c.—*Lisbon,* 1863. 4to, pp. 231.

9. *Notes of Holiday Tours in the Peninsula.* By a PHYSICIAN. Anonymous.—*London.*

(*Concluded from our last.*)

BELIEVING the outline previously given of Portuguese medical institutions at which students can acquire professional knowledge, and subsequently get licences enabling them to practise medicine or surgery, may have proved interesting to professional readers, we now proceed to notice some of the principal charities in Lisbon, at which practical experience in the treatment of disease may be obtained; and first of "O Hospital de San Josè"—St. Joseph. This establishment constitutes the chief institution of the kind throughout Portugal, and has attached to it the medico-chirurgical school already mentioned. It forms a very irregular structure, being four stories high, and is also of vast extent. Originally the building was a Jesuit convent; but after the great earthquake in 1755, which destroyed much of Lisbon, Government made it into a receptacle for sick persons, and subsequently the name became altered to its present designation. The two lower floors in this establishment are appropriated to male, and the two upper to female patients; but, excepting a clinical ward, containing pregnant women, it possesses no special divisions for treating particular complaints.

The medical staff attached to this large civil hospital comprises eleven attending physicians, with a like number of surgeons, each of whom receives 66*l.* as an annual honorarium. Besides these twenty-two permanent officials, there are seventeen physicians and fourteen surgeons extraordinary, any of whom may, however, be requested to perform the duties of an ordinary medical officer, should one of the latter gentlemen have obtained leave of absence, and so forth; but in that case the substitute gets no remuneration. In addition to the above number of medical officers, there are likewise six surgeons, with a medical director, one of whom attends by turn at the hospital during twenty-four hours consecutively, in order to admit persons labouring under disease, or to give immediate assistance in cases of accidents. These gentlemen further take charge of out-patients, who are very numerous, as no letter of recommendation is required, and, by way of distinction, they form what is called the hospital "Banco." According to the above authentic statements, it hence appears that this metropolitan charity has not less than sixty physicians and surgeons, who officially afford their services to parties applying for professional advice, or when admitted as patients. In fact, the medical staff of St. Joseph's Hospital at Lisbon comprises a much larger number of professional attendants than that belonging to any similar charity in most European capitals.

By way of illustrating the general movement among inmates treated at St. Joseph's Hospital, and indicating its magnitude, the total number of patients actually admitted during one year, and the mortality reported, may be quoted as evidence. Throughout twelve months, ending the 31st December, 1860, the total in-patients received into St. Joseph's Hospital were 10,940 in number, of whom 6267, comprising 4526 males, and 1741 females, were treated by the physicians; while in the surgical wards the aggregate amount was 4673, consisting of 2873 male, and only 1800 female inmates. During the year above specified, 9533 patients were discharged, most being convalescent; but as the exact proportion of cures is not stated in the authoritative report which we have at hand, a comparative statement cannot be given of recoveries. Regarding the relative mortality among medical and surgical patients, it is, however, interesting to mention that the ratio of deaths from maladies comprised under the former category was 16·59 per cent. in both sexes; but viewed separately, the scale proved greatest among females, seeing that 434 of that sex died—i. e., nearly one-fourth of the whole 1741 cases admitted; whereas the death-rate in medical male patients was 13·39 per cent., or about half the former amount, Contradistinguished from such data, it is, moreover, worthy of notice that a much smaller mortality was recorded in the surgical than medical wards, since only 239 patients died among the total 4673 cases so classified, which hence makes the average of deaths in that department about five per hundred; and although very little difference prevailed between either sex, it ranged somewhat higher in males, being 5·60 per cent. against 4·33 among females, suffering from surgical diseases. The very large number of fatal results occurring among female inmates treated in the medical division, therefore, seems remarkable. In fact, the chance of an unfavourable termination to their malady appears to have been six times greater than if they had become patients under the surgeons. The explanation of this anomaly is difficult, but being rather a singular feature in medical statistics, it seemed well worthy of record, when alluding to the chief civil hospital in the Portuguese metropolis, which is also one of the largest establishments of that kind throughout Europe, as shown by its annual admissions being generally about 11,000 altogether, irrespective of the still more numerous outpatients who are likewise treated at this institution.

During ordinary seasons, and when no epidemic malady prevails at Lisbon, which, however, seems no uncommon occurrence, the total number of beds in St. Joseph's Hospital usually average about 900; but a larger amount of in-patients are often accommodated—nay, much beyond that number have been at one time under treatment, when even 100 sick persons were admitted into one ward. Being an old building, this charity cannot bear comparison with many newer structures in other European capitals; nevertheless, much has been recently effected for its improvement, the beds being chiefly of iron, most of them having neat and clean bed-linen. Like similar buildings for patients in southern Europe, the lighting and ventilation of various dormitories are defective; but here, as throughout Spain, natives usually entertain great objections to admitting sunlight into any dwelling they inhabit, and hence pure air often becomes too much excluded—a fact which proves highly injurious to inmates if crowded in hospitals. This prejudicial feeling appears, however, so strong in the popular mind, that any alteration of the system now pursued will always prove a feat of difficult accomplishment.

Adjacent to St. Joseph's Hospital, indeed, forming an annex of that institution, is the "Desterro" infirmary, which was formerly also a convent, but is now appropriated to sick criminals sent from the Lisbon prisons, and hence the name Desterro, or banishment. During 1860 the admissions into this establishment were 1549, of whom 1496 went back to prison more or less convalescent, while 66 deaths, or only 4·26 per cent. were recorded, which shows much smaller mortality if compared with similar results obtained at St. Joseph's Hospital.

The "San Lazaro" Hospital likewise constitutes another appendage of the previously-named charities, being situated also in their vicinity. This institution is chiefly occupied by leper patients, and when recently visited by the present writer, it contained 69 inmates afflicted by that loathsome disease. Of these, 49 were males, and only 20 females, which proves the greater prevalence of leprosy among the former than the latter sex. The malady now mentioned is not infrequent in some districts of Portugal, especially on the sea-coast, and among a poverty-stricken, badly-fed population. In fact, as in the case of various maritime localities in the southern provinces of Spain, leprosy still lingers where in ancient times it was even more common, although at present cases rarely occur of pure Arabian elephantiasis.

Another charitable establishment equally deserves mention—namely, the Lunatic Asylum. Being the chief public institution of that description throughout Portugal, insane patients are therefore often sent thither from different provinces which possess no appropriate place for treating demented persons. When recently visited, this asylum contained 501 lunatics, 244 being male, and 257 female patients. They comprised paupers, as also inmates belonging to the middle class of society, some of whom paid at the rate of *5s. 8d.* per diem. The medical staff comprise an attending surgeon and two physicians, one of the latter officers being resident, and also a superintending director.

Along with these three public institutions for sick persons, a fourth must likewise be enumerated, seeing that all four are managed by the same administration—namely, "O Hospiscio de Rilhafolles," or Lock Hospital for females. If, however, this term be literally translated, it seems derived from "rilhar," to gnaw, and "folles," bellows; but why such an odd appellation was ever selected for the receptacle of its peculiar inmates seems difficult to surmise.

The patients at this establishment are almost exclusively public prostitutes, who have been sent here to be cured of maladies incident to their occupation. These individuals being licensed and ticketed by the police, are obliged, under the risk of imprisonment, to present themselves weekly at the dispensary of their own district, in order to be examined by the surgeon appointed for that duty, of whom there are twelve in Lisbon. If diseased in any form, the parties affected are immediately sent to this hospital of Rilhafolles, where they must remain until completely cured of their special malady.

One feature which characterizes the Rilhafolles' internal regulations is so peculiar, that it seems worthy of being mentioned when describing Portuguese benevolent institutions, more especially as no analogous rule, as far as we know, prevails at any similar receptacle in other countries. The peculiarity alluded to is the fact that, every inmate under treatment, whether physically able to move or otherwise, must, by order, remain in bed daily between twelve and two P.M., without ever speaking a word to her neighbour, or even whispering, and still less attempting to get up, during the exact period thus dedicated to strict silence! This is no doubt intended to be a penitential act. On occasion of our visit, made during the time of silence, every ward had its windows shut, while each patient lay in bed as if during night-time, and none dared to open their mouths, even to an attendant.

Besides the public institutions for sick persons above specified, the large military hospital, situated on an elevation in the suburb named Buenos Ayres, is equally worthy of inspection. The wards of this establishment are spacious, freely ventilated, and much superior in that respect to those of St. Joseph's; at the same time being exceedingly clean, while great order appeared to reign everywhere. With a view to illustrate the desire felt by Lisbonese public authorities to improve, as much as possible, this military institution, in reference to sanitary arrangements, it may be stated that the new kind of window lately introduced at Bethlehem Hospital in London, by Dr. Wood, while he was its resident medical officer, has been adopted here, according to a model obtained purposely from England.

The Marine Hospital, likewise, occupying a splendid situation in the "Campo Santa Clara," also well deserves notice by foreigners who may visit Lisbon, as it further manifests the anxiety which actuates Government to correct existing defects in its public institutions for sick inmates, and, wherever possible, to accomplish improvements. This feeling is creditably exhibited in a new ward which has been very recently constructed in that establishment. It forms one of the best apartments appropriated to patients labouring under disease at present existing throughout Europe. Neither in the Lariboissière Hospital at Paris, nor St. John's at Brussels, which are acknowledged to be two of the best institutions of that kind erected anywhere, will any analogous ward be found, which so much deserves being taken as a pattern, when constructing hospital dormitories. The ample space for inmates, which average about twenty, the admirable ventilation, effected chiefly by windows reaching down to the floor, and having apertures high up towards the ceiling, besides a remarkable cleanliness apparent throughout, with other special features characterizing this dormitory, merit great commendation. Various appendages appeared of a superior description to those usually characterizing wards of public hospitals; while hot and cold water, mirrors, lavatories, and so forth, were provided as well for the residents' health as comfort. In fact, a private bedroom could scarcely be better furnished, or kept in greater order and neatness, than this new apartment of the Lisbon Marine Hospital.

"A Santa Casa de Miseracordia," The Holy House of Mercy, likewise deserves notice, since it bears considerable relation to a question which much interests the medical profession, both with reference to the health as to the rearing of infants and young children. This establishment resembles in many respects an English poor-house, having a foundling hospital attached, where about 2000 infants are annually received and taken care of, as in the case of similar institutions on the Continent. Another division of this charity is appropriated to orphan girls, who are educated and reside here until they attain a certain age, when some are placed in situations, while others obtain marriage-portions. Being principally supported by profits derived from public lotteries, the drawings of these schemes always take place under the direction of officials belonging to this institution.

We proceed now to make one or two observations respecting a recently-established Board, which watches over the sanitary condition of the Portuguese metropolis and country generally. The institution here noticed is "O Conselho de saude publica do Reino"—the Council of Public Health of the Realm; and although it does not yet possess sufficient power, in consequence of defective laws, and being unable to grapple with many of the evils over which such bodies ought to have full jurisdiction, much good has already accrued from its establishment. Sanitary science being yet of rather modern origin in this southern portion of Europe, many questions bearing upon public hygiene and medical police have, heretofore, not obtained that attention in Portugal which they deserve, or been discussed among professional authorities. Still, the Portuguese Board of Health has lately rendered important services, both throughout the metropolis and various provinces —services which were especially noticed during the epidemic cholera that devastated several districts of Portugal during 1856—whereby public attention became awakened to the advantages of hygienic measures, towards preventing the spread of disease, and of improving the physical frames of the population.

The temporary regulations then established proved so efficient, that several were soon afterwards made law by a legislative enactment; and in 1857, when yellow fever devastated Lisbon, the Council of Health, by judicious sanitary administration, as also the surveillance it exercised over subordinate officials, effected much good. Visitations from house to house were then carried out by delegated medical inspectors, through whose activity many local nuisances were removed, and the proverbial filth of Portugal's outwardly magnificent capital became so materially diminished, that it no longer remains the unpleasantly odoriferous or insalubrious residence of ancient times. Sewers have been constructed, public conveniences opened where formerly none existed, and scavengers are now often employed to remove offal; in short, from being one of the dirtiest cities in Europe, Lisbon has become really clean, and one in which foot-passengers may walk without fear of being bespattered by mud, or even coming in contact with worse defilements. Besides being freed from these grievances, fierce dogs, that formerly in large numbers rendered the streets dangerous at night, and far from agreeable during day-time, rarely infest thoroughfares as formerly they did.

Irrespective of the previous allusion to the outbreak of yellow fever in 1857, it seems further interesting to mention that this epidemic proved most severe during September and October of that year; while the largest number of entries into hospitals, for receiving patients so affected, occurred on the 20th of October, when 298 new cases were admitted. From an official published report, 5161 deaths by yellow fever were recorded in special hospitals, of whom 4718 were males, but only 1116 females; hence giving four of the former sex to one of the latter. In addition to these fatal cases, 7842 other patients died at their own or other domiciles, which made the gross mortality amount to 13,757, all classes included; being therefore about one-twentieth of the entire population. Besides the fact as to male patients oftener succumbing to yellow fever than females, it should be stated that not only few children had the disease, but that most recovered; while individuals after they had passed their sixtieth year were rarely affected; and the period of life most dangerous seems to have ranged from puberty to full manhood. Again, bachelors and single women much oftener became victims than married people, the excess of the former being upwards of three single persons to one comprised in the latter category: the proportion of deaths to recoveries, among the total that came under observation, being one fatal termination to two cures, or 37 deaths in every 100 cases affected.

Contradistinguished to the Arsenal, it should be stated that public establishments, such as the Lunatic Asylum, St. Joseph's Hospital, that of St. Lazarus, the Orphan Institution, and House of Industry, suffered very little by the epidemic, notwithstanding their numerous residents. Among inmates of prisons, likewise, examples of the malady in question were rarely met with, seeing that all the institutions now enumerated remained remarkably free from attacks. On the other hand, many labourers occupied in the Arsenal, and persons residing near that building, died of yellow fever; but medical authorities attributed this feature in the disease not only to the filthy condition of the establishment named, but also to its proximity to the Tagus; seeing that nauseous odours frequently emanated from thence, and consequently polluted the atmosphere in that neighbourhood. Whereas residents of the institutions previously mentioned, since they occupied more elevated situations, and were removed from a stinking shore—especially offensive during hot weather and at ebb-tides—experienced comparative immunity.

Considering that it might prove interesting to English readers, were some general remarks made regarding the system pursued by Portuguese practitioners when treating disease, or the doctrines they usually adopt in reference to medical questions, we may observe that the physicians and surgeons of Portugal are not an exclusive sect, or comprise members who differ in opinion and practice from their brethren of other European countries. On the contrary, many are actuated by a laudable desire to know what passes among scientific men elsewhere, in order to benefit by the experience they thus acquire, as well in theory as practice. Speaking generally, Portuguese medical men, at least in the metropolis and Oporto, do not hesitate to select from the works of foreigners whatever seems reasonable or worthy of being followed, when based on facts and observation. Nevertheless, in consequence of few works having been heretofore published on professional subjects by Portuguese professional writers, students and practitioners generally have recourse to the productions of foreign authors while pursuing their studies, or when investigating questions bearing upon the science and practice of medicine. If any particular nation or class of authors enjoy more influence in these respects than another, it certainly seems that French authorities are most in vogue among the physicians and surgeons of Portugal.

Although this tendency towards French doctrines extensively prevails among members of the profession, still the Lisbon school of medicine and surgery appears as if likely to exercise much future influence throughout this kingdom, in consequence of the Portuguese army and navy medical officers being chiefly educated at that institution. Moreover, its professors comprise some of the most eminent men in Lisbon, while many alumni educated under their tuition, as also at the various metropolitan hospitals, will eventually constitute a large body of medical practitioners, both in the capital and provinces. Possessing such a field for observing diseases in every variety, as that afforded by St. Joseph's Hospital, with its adjacent institutions, each containing sick persons, where students have ample opportunities for dissection, and learning pathology through actual observation, the Lisbon medical school (notwithstanding that it seems only yet of modern existence) has already attained a high position in public and professional estimation, even if compared with the ancient University of Coimbra.

This metropolitan school, having the true elements of professional study, derived from so large an hospital as St. Joseph's, thus furnished, even from its foundation, not only plenty of subjects for dissection, but numerous cases illustrating every type of disease. There being, further, no want of bodies for teaching pathology, this medical institution soon became more practical than speculative in its character; and, according to reliable authority, it may be justly said that anatomy, the true basis of medical science, has always been much cultivated by the pupils of the Lisbon school, notwithstanding that many of them, at the commencement of its career, through previous defective general education, were imperfectly conversant with the prevalent doctrines of that period. Hence, whatever knowledge they heretofore acquired became too much limited in its practical application, although an outline of the popular theories was not forgotten by the several teachers. This original feature of Lusitanian medicine continues to distinguish the method of instruction still pursued by those who have succeeded, partly from tradition, and in some degree owing to special circumstances. Consequently, there exists at present not only a considerable blank in native medical literature, but further, very little enthusiasm prevails for scientific discussions, unless they are based on facts and observation.

Thus, it will not be incorrect to assert that the salient feature of medical instruction in Lisbon is essentially practical, while the profession in its systematic tendencies is decidedly in favour of what may be designated modern "physico-pathologism," which rests upon science, and is illustrated by rational theories.

Since the outbreak of yellow fever in Lisbon, during 1857, and alluded to in previous paragraphs, medical observers have remarked that the constitutional character of disease, which has more lately prevailed, seems considerably modified. For example, remittent fever, which was rarely noticed in the capital before that epidemic devastated its population, is now of common occurrence; and further, most febrile complaints which have recently attacked patients soon became complicated with great depression of physical force, dyscrasia of the blood, and exhibited symptoms the very opposite of inflammatory; whereas formerly the general type of disease was often quite otherwise. In consequence of this (supposed) marked change in the apparent diathesis of maladies, tonic treatment has been adopted much more frequently than at former periods, while bloodletting, so common in olden times, is now very seldom employed. But such modern reaction against the "Sangrado" system not only prevails in the Portuguese metropolis, but throughout Portugal, so that the depleting mode of treating febrile and even some inflammatory affections has fallen into discredit. Besides this type of debility, frequently characterizing diseases recently met with, it may be added that diphtheria has proved a common complaint, and even raged epidemically on more than one occasion, being followed frequently by fatal terminations; while ascites, as also anasarca, seemed not unusual sequelæ of other maladies, appearing to indicate that renal disorders are somewhat common affections among the Portuguese people.

Although various quackeries prevail to a certain extent in Portugal, as in other European countries, even the most advanced in science and civilization, charlatans but rarely obtain any lasting reputation among the people, and have seldom derived much benefit through public credulity. The vivacity of character and inconstancy of disposition which generally distinguish Lusitanian populations, however susceptible of receiving mental impressions, render them very versatile in their opinions regarding the efficacy of new remedies, if vaunted by foreign authors, for the cure of disease. Consequently various novelties, although recommended by apparently favourable experience, and at first producing considerable impression, fall soon afterwards into neglect, if not oblivion. Homœopathy and similar modern fallacies have doubtlessly obtained an occasional footing among credulous votaries; but still, according to statements made by competent local authorities, Portugal appears more free from these absurdities than its neighbouring peninsular kingdom, where quackery reigns even in high places, and is under royal patronage.

Throughout Portugal, having a population of 3,600,000 inhabitants, according to its recent census, the entire medical profession consisted, by a return purposely procured from the Lisbon Council of Health, of only 2923 members, thereby giving one practitioner to nearly every 1300 individuals. The above numbers comprise six separate divisions, which may be thus classified, whether practising in the capital or elsewhere—viz.—

	Physicians.	Surgeons.	Pharmaceutists.	Bleeders.	Dentists.	Midwives.
In Lisbon . . .	49	214	185	13	19	92
In the Provinces	196	574	667	821	5	90
Total . .	245	788	852	834	24	182

From these official statistics, the Portuguese metropolis will not be considered as overstocked with medical men, seeing that only 263 regularly licensed physicians and surgeons are legally entitled to practise among a population of 276,000 persons, dwelling in this city, having a resident court and legislature, besides other collateral advantages. But one peculiar feature exhibited by the table now given seems worth a passing notice, from portraying popular customs—namely, the very small number of bleeders—"sangradores"—in the capital, where only thirteen operators of such designation reside; whereas 821 are located throughout the various provinces. That this discrepancy indicates that a greater proclivity to bloodletting prevails among the rural population than in Lisbon, cannot, however, be assumed from the above statement, although the opinion may be fairly inferred that in Portugal, as in Spain, the custom of abstracting blood, even for ordinary ailments, still obtains much popular favour, and is often employed without sufficient reason, or by medical sanction.

Considerable attention having been occasionally directed towards the advisability of selecting Lisbon as an eligible winter residence for invalids, a few cursory and general remarks respecting its climate cannot here prove either out of place or uninteresting. Prior, however, to discussing that question, some preliminary points may be mentioned which seem important. For instance, Lisbon being chiefly situated upon frequently high-lying ground, about nine miles from the Atlantic seashore, its atmosphere is consequently somewhat maritime; and further, the several hills whereon this city has been built are divided into two distinct geological formations, one extending eastward, the other having a western direction. Between these two divisions, a line running through San Bento-street to the Quinta do Leabrar, and the Lago do Rato—Rat-place—may be traced as forming the real boundary of the above-designated portions, whereof the one to the eastward—the site of ancient Lisbon—consists of tertiary miocene deposit, whereas that towards the west is a calcareous formation.

The series of elevations now alluded to in their geological character, while they have mostly a southern aspect, and all slope towards the Tagus, possess another important advantage besides that of situation—namely, that during heavy showers, which are not uncommon in this district—however copiously rain may fall, it quickly runs off, and at the same time washes away whatever offensive matters were previously accumulated. New sewers having recently been constructed in various streets, and the scavengers being now often active in cleansing thoroughfares, which were formerly often very filthy, the metropolis has hence become not only more clean than at former periods, but is much improved in salubrity. For, owing to the state of matters generally prevailing not many years ago, Lisbon was considered the dirtiest city in Europe; and its smells often so very disgusting, particularly during hot weather, that promenading even in fashionable public places was rarely either pleasant or a desirable pastime.

In addition to the sloping situation and other physical features characterizing the Portuguese capital, those northerly winds which generally prevail throughout nine months of the year materially assist not only towards dispelling noxious vapours arising from its muddy river banks, exposed at low tides, and always worst during south winds, but likewise the various effluvia necessarily originating amidst an overcrowded population, and issuing from imperfectly trapped sewers.

Owing to these causes, and others which need not be specified, the Lisbon climate is considered genial by many competent observers; nay, some writers even deem its atmosphere the finest in Europe. But that opinion must be held as an exaggeration, although certainly in various respects the air often feels delightful, and hence is liked by most foreigners lately arrived from colder regions. Notwithstanding the Portuguese capital occupies a more southerly position than Naples by about two degrees of latitude, it enjoys a milder summer, except on rare occasions; as, for instance, in the month of August, 1861, when the temperature rose much beyond its usual height. Nevertheless, the average heat at Lisbon seldom becomes so intense as that common in the Neapolitan metropolis; while the frequent sea-breezes which often prevail in afternoons, by tempering the fiercely hot rays of sunshine, darting through a clear and cloudless sky, render the former locality preferable as a residence.

According to Franzini, who has paid special attention to such subjects, and is considered high authority in reference to meteorological inquiries, the average temperature usually recorded at Lisbon is 61° Fahr.; while the mean of different seasons ranges at 52° for winter, 60° spring, 70° summer, and 59° during autumn; January and February being the coldest months, July and August the hottest, under ordinary circumstances. Irrespective, however, of these official statements, supported by so competent an observer as Franzini, it is reported by other authorities that, during some days in the latter week of May, or beginning of June, great heats are sometimes experienced, as likewise during the early part of September; but such examples of augmented temperature must be taken as exceptional. Northerly winds appear most prevalent and characteristic of the Lisbon climate, although southerly occasionally prevail; the north or north-easterly winds being much the coldest, especially when the latter blows, which causes disagreeable feelings in most residents. Being invariably a piercing blast, the north-east wind frequently excites irritation of the ears or nostrils in those exposed to its influence, and these effects are further accompanied by an augmented lachrymation that often becomes very troublesome. Should no wind prevail, which occasionally happens in this locality, the sun's rays become sometimes so burning hot, even in winter months, provided the sky remains unclouded, that the difference of temperature experienced in some streets, between their shady and sunny sides, varies often twenty-five degrees, if not more extensively. As in the case of Spaniards, most Portuguese persons entertain great dread regarding the injurious effects of strong sunshine, against whose deleterious influence all carefully guard themselves, especially should a north wind be blowing. Should these two contingencies exist, a Lisbonese will not very willingly go out of doors, unless on business or through necessity, and rarely for pleasure or amusement.

These apprehensions, entertained by many Lisboneans, are well founded. Scarcely anything can be worse than thus to get broiled, as it were, on one side of a street, and immediately afterwards to feel almost frozen. In Madrid, analogous influences materially affect residents when so exposed, and often induce fatal diseases, especially those designated under the popular term, "pulmonia." In Lisbon similar results more frequently supervene than amongst persons dwelling in countries further northward. Some medical practitioners have consequently thought that this susceptibility of the skin, so often manifested, and believed really to affect Portuguese constitutions, might become materially improved through hygienic measures. Such beneficial result would be further essentially promoted by frequent ablutions, usually much neglected in southern warm climates, as also by the free admission of pure air and more sunlight into ordinary dwellings. The above important influences Lusitanians seem ever most anxious to avoid, especially the lower classes, many of whom also occupy houses very badly ventilated.

Being situated close to extensive muddy shores of the adjacent river Tagus, Lisbon in its lower parts is hence more insalubrious than higher-lying positions; this shore also being nearly three miles in length, a large portion of the city becomes more exposed to the baneful influence just specified, whose effect was recognised in a most marked manner during the prevalence of yellow fever in 1857, already named, which proved most rife throughout these littoral districts. After that malignant epidemic ceased, the Lisbon civic municipality energetically exerted themselves to remedy, not only the defective sewerage which had materially tended to augment the insalubrity of various localities, especially in low-lying neighbourhoods, and also in conjunction with the Council of Public Health, accomplished other useful ameliorations, whereby the entire metropolis has greatly improved in reference to its sanitary condition. Hence, like the great fire of London, or more recent conflagrations at Moscow and Hamburgh, the late severe epidemic which caused such extensive sufferings among the poor of Lisbon, and proved fatal to many inhabitants, has already produced some highly beneficial results upon their health and physical well-being.

As no high hills surround Lisbon, and only moderate elevations are met with for some distance in the vicinity, its atmosphere proves more bracing, and feels keener than that of several adjacent villages, which usually occupy hollows, or, more properly speaking, neighbouring valleys. For instance, Bemfica, about two miles north-eastward, being situated on low ground compared with the upper portions of the capital, has a milder atmosphere, and hence invalids frequently find a residence at this place beneficial in certain forms of pulmonary complaints. Whereas persons enjoying good bodily health, of temperate habits, and possessing an adequate amount of mental activity, deem the Lisbon climate favourable to such constitutions; and strangers, during the early periods of their sojourn, deem it agreeable.

Nevertheless, however pleasant the air of Lisbon may at first seem, after residing there even for a brief period, many people are apt to experience the enervating effects of this southern region upon their bodily frames, which hence cause considerable disinclination either to engage in active physical labour, or to pursue ordinary intellectual occupations with the same zeal that they possessed on other occasions, and under different circumstances. This feature, characterizing the Lisbon climate, has been long noticed by observers, and may in some degree explain the apathy so generally prevalent among all classes of the population, and verify a popular proverb which says, "Portugal was made for its natives, and Gallegos—Spaniards from Gallina—were subsequently given them as servants."

In consequence of climacteric and other local influences affecting residents, lung diseases are very frequent in the Portuguese metropolis, but especially tuberculous phthisis, which annually causes great mortality. In proof of such statements it appears that, among 76,864 patients admitted into St. Joseph's Hospital at Lisbon during five recent years, 1448 were consumptive cases, of whom 1150 died in that establishment. Further, the Lisbon climate being considered too keen and irritating for persons labouring under pectoral affections, but particularly during seasons when cold variable weather prevails, physicians recommend several localities in the environs to which they should remove in winter, or at least towards the commencement of spring, as preferable dwellings for phthisical patients. The villages usually pointed out by Lisbon medical authorities are Bemfica, already named, Campo Grande, Lumiar, and one or two other places also near the capital. These favourite retreats for phthisical invalids being situated in low-lying ground, are protected against the commonly prevailing northerly winds, which invariably prove inimical to patients so affected. Besides, as the villages above named usually possess pretty public gardens, to which promenaders have free access, they can thus enjoy open air exercise when deemed advisable; and as the atmosphere of these suburban districts is milder and more equable than that of the capital, it often exerts considerable soothing influence upon the thoracic organs and respiration of patients so circumstanced.

Towards further illustrating the desirability of Lisbon, and especially an adjacent village, as a retreat during winter for consumptive cases, we would add to previous observations the opinion of a medical friend, an English physician, who, having resided some years in Lisbon, can speak from considerable professional experience. The practitioner here alluded to states, in a communication to the present writer, that among invalids coming from England affected with phthisis, the Lisbon climate frequently produces a beneficial effect upon their malady; and when proper precautions have been taken, great solace and relief of prominent symptoms often seem to be experienced.

Were Lisbon better provided with lodgings adapted for invalids, and these easily procured, this city would become as a residence much more desirable than at present. Indeed, the admitted paucity of comfortable accommodation, according to the ideas of Englishmen, constitutes an important desideratum to patients proposing to spend a winter season in the Portuguese metropolis.

Indubitably there are some good hotels in Lisbon, where visitors may confidently anticipate being comfortably accommodated; as for instance at the Braganza, d'Italia, Central, Durrand's, and Street's, which will bear comparison with those of most Continental cities. But seeing that these places are frequently full during the months when strangers most do congregate, invalids may then find much difficulty in getting properly housed, and hence be exposed to much inconvenience. Therefore, whenever English persons propose sojourning at Lisbon during winter, they ought to retain lodgings before leaving home, in order to ensure adequate accommodation on their arrival. Respecting the expenses incident to a residence at this capital, competent authorities report they are about equal to those usually experienced in London, if house-rent, servants, the quality of food, and various customary accessories are considered; whereas all articles of wearing apparel are dearer than in England.

Besides pulmonary complaints, it may be stated that eruptive diseases, particularly small-pox, are very common among the Lisbon population. Intermittent and continued fevers also frequently prevail —as for example during 1861, when numerous cases of ague were received into the city hospitals, in addition to those treated elsewhere; many of those patients having become so affected through turning up new ground required for railway cuttings then in course of construction. In further proof of such deleterious influences, this fact may be mentioned, viz., that, among upwards of five thousand persons employed on the Southern Railway of Portugal alone during the same autumn, nearly one-third, or fifteen hundred, were suffering from fever, chiefly intermittent. Indeed, the late excellent and much beloved king, Dom Pedro, fell a victim to fever during his temporary residence at the palace Villa Viciosa, in this neighbourhood; while two other princes, his Majesty's brothers, also died soon afterwards from a similar malady; and lastly, a fourth, Dom Augusto, nearly succumbed likewise; which disastrous events caused not only an intense and painful sensation throughout Lisbon, but nearly produced a popular revolution.

Bowel complaints, often severe, are also not uncommon in the Portuguese metropolis; while apoplexy, paralysis, and nervous maladies frequently come under the observation of medical practitioners, thus demonstrating that diseases are generally of much the same type in this southern district of Europe as those met with elsewhere. Consequently, whatever Lusitanian amateurs may assert regarding the assumed, and often admitted amenity of the Portuguese climate during most seasons, when contrasted with more northern regions, it has still drawbacks. Nay, even natives are sometimes induced to expatriate themselves, during winter months, to a more southern country, having warmer temperature. Thus, residents of Lisbon, predisposed to pulmonary disease, are wont to visit Madeira at that season.

Moreover, although phthisical subjects coming from northern countries may often find the atmosphere of Portugal pleasant to their bodily sensations, and apparently restorative of health, invalid Lisboneans, influenced doubtless by analogous motives, or perhaps only desirous of making some change, emigrate even further southward, and are most anxious to quit a place which they deem undeserving of the encomiums which it often receives as a winter residence from foreigners.

Finally, among several localities frequently lauded as delectable retreats during sultry weather, Cintra well deserves special mention. This royal domain lies about sixteen miles from Lisbon, towards the Atlantic; and as the town stands at the foot of a stupendous granitic "Serra," 1800 to 3000 feet above the sea-level, its site is highly picturesque; and having a cool, salubrious climate, it becomes the favourite resort both of invalids and others, more particularly when the capital gets so hot as to be almost intolerable. At our recent visit to this true "oasis" in the neighbouring sun-burnt desert, the luxuriant vegetation, beautiful flower-gardens, and tree-shaded promenades, which there meet the eye of the visitor, amply prove that the reputation which Cintra has acquired, as a sanatorium in summer, is by no means erroneous or inappropriate.

PART SECOND.

Bibliographical Record.

ART. I.—*Lectures on Surgical Pathology, delivered at the Royal College of Surgeons of England.* By JAMES PAGET, F.R.S., Surgeon Extraordinary to Her Majesty the Queen, Surgeon in Ordinary to H.R.H. the Prince of Wales, Surgeon to St. Bartholomew's and Christ's Hospitals. Revised and Edited by WILLIAM TURNER, M.B. London, F.R.C.S.E., F.R.S.E., Senior Demonstrator of Anatomy in the University of Edinburgh.—*London*, 1863. pp. 832.

MR. PAGET'S lectures were originally delivered before the Royal College of Surgeons during the six years (from 1847 to 1852) in which he held the office of Professor of Anatomy and Surgery to the College. The subjects chosen were various; and each course, as it came to an end, was fully discussed in the pages of this Review. Subsequently, in 1853, these lectures were collected and published in two volumes, under the name of 'Lectures on Surgical Pathology.' They were also briefly noticed by us at the time; but they had been so fully considered at the time of their delivery, that it required only a few words to commend them again to the attention of our readers. They were received with great favour by the profession, and soon took a high place in surgical literature; and still, after the lapse of ten years, they stand almost unrivalled for the large and comprehensive spirit in which they treat some of the most difficult, and yet fundamental, subjects in medical science.

But fortunately, our knowledge is advancing with such rapid strides, that a book which is ten years old is almost sure to be a little behind the science of the day; and this may be said even of such philosophical writings as Mr. Paget's. Perhaps there is no branch of professional knowledge which is making greater progress than physiology; and physiology is peculiarly the science of the medical man—of the surgeon no less than of the physician; and it is in this respect—in the points where they touch physiology—that Mr. Paget's lectures have fallen behind the requirements of the day. Such being the case, he wisely determined to bring out a new edition, which should embody all the "facts, probabilities, and guesses at truth," which have been added to pathology in the last ten years. But the demands upon his time and energies were so numerous, that it was impossible for him to undertake the task without assistance. In his preface he says:

> "I was, therefore, glad to be able to commit the work of revision to my friend, and former pupil, Mr. Turner, whom I know to be not only very conversant with the progress of medical science, but able to test others' observations by his own. It is not for me to say how well he has done the work, for I have so worked with him as to be equally with him responsible."

In comparing the old and the new edition, we find that the general arrangement of the work is much the same as before. Indeed, the only change that we have observed in this respect is, that the "recurrent tumours," including the "fibroïd" and "fibro-nucleated," have been classed after instead of before the myeloid, osseous, glandular, and erectile tumours, so that they now stand immediately before the cancers, and form the link between the (so-called) benign and malignant growths. In consequence of this alteration, the early part of the chapter on recurrent tumours has been re-written.

The minor changes that have been made are more numerous. Every page, almost every paragraph, bespeaks the careful revision which the whole work has undergone. Here and there a sentence has been added, and here and there one has been omitted. Anything which seemed superfluous has been left out, while some subjects have been made clearer by a little further explanation. In some instances, the history of patients has been brought down to a recent date; and the author has given us the benefit of his enlarged experience by introducing new cases, and by adding to the numbers used in the statistical tables, on which some of his conclusions are founded. In a few instances, the terms employed have been altered in conformity with the scientific language of the day. For example, instead of "cellular tissue," we read now "connective tissue;" and instead of "elastic tissue," we read "the elastic form of connective tissue."

But it is in the notes that we find the most valuable additions have been made. Many fresh authorities have been quoted; and the most recent professional literature has been laid under contribution. A number of new references have been added, and among these we notice many to the Edinburgh medical journals, and to the museum of the Edinburgh University, which will perhaps have the advantage of making the lectures both more interesting and more useful to our brethren north of the Tweed. Besides this, several of the questions which now occupy the attention of physiologists and pathologists—such as the development of connective tissue, amyloid degeneration, wasting palsy, &c., are discussed at length; and the views of Virchow, Rokitansky, and other distinguished foreigners, considered and criticized.

If we are not mistaken, the author's hand may be traced in the revision of the text; while the editor's labours have been chiefly confined to the notes. Mr. Paget needs no praise from us; his merits are recognised by the whole profession. To Mr. Turner we can offer no higher compliment than to say that the editor of the volume before us appears to be well worthy of the author.

In the present edition, the work has been printed in smaller type, and compressed into one volume; and though it may not be so agreeable or convenient to read as in its original form, it has the advantage of being less expensive—an advantage not to be overlooked in these days of cheap literature.

Art. II.—*On the Origin of Vaccine Inoculation.* By Edward Jenner, M.D., F.R.S., &c.—*London,* 1801; reprinted, *London,* 1863. pp. 8.

Mr. J. Brendon Curgenven has, by the opportune republication of this tract, brought us, as it were, face to face with the illustrious Jenner, that we may receive from him a mild, but only too well-merited rebuke, for the want of care that we, his successors, have given to the performance of vaccination. Had the cautions of the immortal discoverer been borne in mind by past and present generations of vaccinators, we should not have needed Mr. Curgenven's reminder, nor should we have witnessed what we have witnessed in this metropolis within the last twelve months—a panic of small-pox, and cries for admission into the Small.pox Hospital beyond its capability to meet. It would not have happened that nearly as many cases were admitted into that institution within six months as have ever been admitted in any previous twelve months since its establishment. In the year 1859, 1185 cases were admitted, and in the first half of this year the admissions were 838. It would not have been known that parishes in all directions should have been bewildered to know what to do with their small-pox cases! Neither should we from time to time have heard of fearful outbreaks of small-pox ravaging English towns and rural districts.

With the sons of Jacob, we must confess "we are verily guilty concerning our brother . . . . therefore hath this distress come upon us." It cannot be concealed that we have been too often careless in the manner of doing this small, but certainly not trifling, operation. Contenting ourselves with having inserted the lymph from a vaccine vesicle into an arm, the bare fact has been let pass for a complete protection against small-pox, while due care has not been had that the lymph has been taken from a vesicle of proper age, size, &c., and that it has gone through its own proper course; and so it has happened that in after life the supposed protection has failed, not only to preserve the skin, but also to save lives, fearfully swelling the amount of human misery, and adding to the number of our pauper people.

It cannot be unprofitable to reflect upon the words of the wise, and to learn from their experience; let us, therefore, compare the history of the errors and wrong inferences that attended the origin of vaccine inoculation, as related by Jenner himself, with what we may daily see for ourselves:

"In the course of the investigation of this subject, which, like all others of a complex and intricate nature, presented many difficulties, I found that some of those *who seemed to have undergone the cow-pox*, nevertheless, on inoculation with the small-pox, felt its influence just the same as if no disease had been communicated to them by the cow. This occurrence led me to inquire among the medical practitioners in the country around me, who all agreed in this sentiment, that the cow-pox was not to be relied upon as a certain preventive of the small-pox. This for a while damped, but did not extinguish, my ardour; for as I proceeded, I had the satisfaction to learn that the cow was subject to some varieties of spontaneous eruptions upon her teats; that they were all capable of communicating sores to the hands of the milkers; and that whatever sore was derived from the animal was called in the dairy the cow-pox. Thus I surmounted a great obstacle, and, in consequence, was led to form a distinction between these diseases, one of which only I have denominated the *true*, the others the *spurious* cow-pox, as they (the latter) possess no specific power over the constitution."

Do we not daily witness the failures thus graphically depicted by Jenner? We can picture to ourselves the patient labour with which the pupil of John Hunter and the never-flagging student of nature, quietly, and amid professional work, pursued in Berkeley meadows those researches, seemingly insignificant, but teeming with incalculable blessings to humanity, by which he "surmounted a great obstacle." Here was no inspiration of genius, but a simple fact followed up, and as a life-work industriously reasoned out—an induction traced to its legitimate results, until confirmed beyond doubt, if not beyond cavil. Interesting, indeed, beyond the ordinary interest that invests the pregnant hints of brilliant discoveries—has become the apparently simple incident occurring to the apothecary's apprentice of Sodbury, thus simply told by Dr. Barron, in his "Life of Jenner:"[1]

"A young countrywoman came to seek advice; the subject of small-pox was mentioned in her presence; she immediately observed, 'I cannot take that disease, for I have had cow-pox!' This incident riveted the attention of Jenner. It was the first time that the popular notion, which was not at all uncommon in the district, had been brought home to him with force and influence. Most happily, the impression which was then made was never effaced. Young as he was, and insufficiently acquainted with any of the laws of physiology or pathology, he dwelt with deep interest on the communication which had been casually made known to him by a peasant, and partly foresaw the vast consequences which were involved in so remarkable a phenomenon."

Through observations, experiments, examinations—through evil report and through good report—he judiciously, perseveringly, and successfully fulfilled his early resolution to let no opportunity escape of acquiring knowledge on so important a subject; and we his descendants may reap a full harvest from his labours, if we will but follow in his footsteps, and, like him, feel the full sense of our responsibility as standing between the living and the dead.

A homely illustration of the vast importance of care in the selection of vesicles whence to vaccinate, and of the character of the disease, as well as of the individual constitution of the vaccinifer, may be gathered from another "obstacle" which Jenner's industry surmounted:

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[1] Vol. i. p. 121.

> "It became evident that a person might milk a cow one day, and, having caught the disease, be for ever secure; while another person, milking the same cow the next day, might feel the influence of the virus in such a way as to produce a sore or sores, and in consequence of this might experience an indisposition to a considerable extent; yet, the specific quality being lost, the constitution would receive no peculiar impression."

The disregard of the progressive changes which the vaccine virus undergoes has, doubtless, been the chief among other causes of the many failures in the protective power of vaccination. We cannot, therefore, but protest against the practice of using lymph for vaccination, that has been taken without due attention to the character and age of the vesicle. We have known it to have been taken as early as the fifth day, and as late as the eleventh day, from the same arm! Can other than failure, with its attendant bitter disappointment and unavailing regrets in after life, be looked for from such violations of the instructions laid down by Jenner? Were those instructions followed more generally, and were the example of him who enjoined them more closely imitated as to the investigation of all the phenomena attendant on vaccination, no human voice would be heard to gainsay Jenner's proposition—"It now becomes too manifest to admit of contradiction, that the ANNIHILATION OF THE SMALL-POX, the most dreadful scourge of the human species, MUST BE THE FINAL RESULT OF THE PRACTICE." We have emphatically marked these few last words of Jenner's tract, to indicate our adhesion thereto, and by implication, therefore, to cast a deliberate censure upon all who, wittingly or unwittingly, hinder the bringing about a "consummation so devoutly to be wished," and who have consequently involved us in the disgraceful stigma that, of all civilized nations, that which owns Jenner *is the worst vaccinated of all nations!!*

ART. III.—*On the Question, Is Oxide of Arsenic, long used in a very small quantity, injurious to Man?* By JOHN DAVY, M.D. (Reprinted from the 'Edinburgh Philosophical Journal,' July, 1863.)

DR. DAVY has visited the Whitbeck stream, near Whitehaven, has inspected the vegetation, and has carefully investigated the conditions of animal life on its banks, without finding any ill effects traceable to the arsenic contained in the water of this stream, which, issuing from the Black Combe mountain, flows over arsenical pyrites existing among the minerals abounding in the hill-side. The question Dr. Davy proposes is answered in the negative—a result which need excite little wonder when the author informs us that, from many analyses, he finds the quantity of arsenic contained in a pint of the water to be ·008 of a grain!

ART. IV.—*On the Investigation of Instinctive Movements.* By WILLIAM MURRAY, M.D., &c. (Read at the Physiological Section, British Association, 1863.) pp. 13.

THIS is an ingenious attempt to account for instincts by resolving all instinctive actions into volitional, emotional, and nervous actions, with a source in special nervous centres. The author holds as a principle that life is not an entity, but the result of physical and chemical forces only.

Further, he holds, as regards instinctive movements, that those of lower animals do not differ from those of man in *kind*, but in degree. At the same time, he allows that "in man the volitional, as representing reason, abstract deduction and experience, is immensely superior to the others."

He asks the question—"Is there in animals an intelligence?" And answers it—"We strongly incline to the belief that there is, and that it varies in its power with the kind of animal, and manifests its existence by the extent to which it controls the emotional, or purely instinctive part of his actions." We take it for granted that he considers vital force the correlative of the other forces.

ART. V.—*On Human Entozoa: comprising the description of the different Species of Worms found in the Intestines and other parts of the Human Body, and the Pathology and Treatment of the various Affections produced by their presence.* By WILLIAM ABBOTTS SMITH, M.D., M.R.C.P. Lond.—*London*, 1863. pp. 251.

WE learn from the preface to this work, that its author, in the arrangements of his materials, has followed the plan adopted by Dr. Davaine, in his more extensive treatise on the same subjects, and that much of the information he offers is derived from the same source, of which the numerous quotations afford ample proof. But as Dr. Abbotts Smith's acknowledgments are commensurate and well expressed, this indebtedness no wise detracts from his credit, and we are sure that it enhances much the value of the book.

The subject—entozoa—we need hardly say, has its special interests; how remarkable that there should be animals, and these so numerous, the choice, and indeed the only habitats of which are the organs of other animals, higher in the scale of organization; how remarkable further, the many kinds of these parasites, including in their number some of the lowest forms of life, such as the microscopic protozoa, the vibriones and bacteria, with others as conspicuous for their magnitude and well-defined structure, such as the teniæ and many of the nematoïdea; and further still, how remarkable are they for the mystery in which their origin is enveloped, and for the metamorphoses and changes of abode to which they are subject.

If interesting zoologically considered, how much more so are they in relation to pathology in the maladies they occasion, and these in no few instances fatal in their consequences. We are assured on good authority, that as many as a million of sheep and lambs perish yearly in this country from the rot, a disease produced by the liver-fluke, a species of distomum; and that three per cent. of the pigs killed in Ireland are "measly"—that is, are afflicted with a parasite, a cysticercus, rendering their flesh unwholesome, and capable, there is reason to infer, of giving rise to the tape-worm in man. And though less formidable and less frequent in man, they are only so in degree, and that occasionally inconsiderable, there being countries the natives of which are nearly as much plagued by a certain kind of them (hydatids and tœnia solium) as the sheep in England and the swine in Ireland.[1]

Interesting and highly important as the subject is, viewed as a study it is not without its difficulties, especially when considered medically: the symptoms which worms produce being often obscure and varying much according to the kind of parasite, the organ infested, and the age and condition of the patient. These difficulties, so well adapted for differential diagnosis, are peculiarly fit to train the mind of the student, and test his ability. On this account, we think, helminthology is deserving of more attention in a course of medical instruction than is commonly bestowed on it.

The work before us is a good compendium; its author has brought together much valuable and curious information derived from various sources, though mainly from the French treatise already mentioned. It is divided into three parts. The first part contains a synopsis of the entozoa which are found in man, comprising five classes: 1, protozoa, or infusoria; 2, cestoïdea, or tape-worms; 3, trematoda, or sucker-worms; 4, nematoïdea, or thread-worms; 5, acanthotheca, or spinous-worms; with their sub-classes, species, or varieties, all of which are well-defined in as many chapters. The second part treats of the pathology and treatment of human entozoa, in thirty chapters—a superfluous number, as we think, leading to unnecessary repetition, without compensation in clearness. The third part is on special therapeutics. A glossary of the principal terms employed in the synopsis is added, and also an index. The woodcuts by which the descriptions of forms are illustrated, are well executed.

The opinion we have formed of the work, as a whole, is so favourable that we have no hesitation in recommending it as deserving of a place in every medical library.

ART. VI.—*The Quarterly Journal of Science.* No. 1, January, 1864.—*London.* pp. 210.

JUDGING from the published list of contributors, and from the contents of the first number of this Journal, we cannot but augur favourably of it. In an introduction by its editors, occupying seventeen pages, its design is amply explained—and that a very comprehensive one, embracing science in the largest acceptation of the term, and also the arts, so far as they are connected with science.

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[1] We hope shortly to be able to place before our readers all that has been made out with regard to the prevalence of the Trichinæ in the human body as an element of disease.

As our notice must be brief, we shall chiefly confine the few remarks we have to offer to the introduction, in which, besides a programme of the undertaking, a sketch is given of the present state of scientific knowledge. That this is partial and slight, touching chiefly on popular subjects, is no more perhaps than might be expected. We could have wished that it had been either longer or shorter—longer, to do justice to the great subjects; or shorter, by the omission of portions of common notoriety, and of passages *ad captandum*. Of the latter class, as at least they appear to us, are the appeals on the score of religion. Such we hold to be as unnecessary as they are novel. Was not Bacon right in maintaining that science and religion should be kept apart? And has he not truly pointed out the danger of commingling them?

There is another topic about which we can hardly agree with the editors—viz., that science "is beginning to exercise an influence in every circle of society." Its influence, we believe, is increasing, but not beginning to be felt. Has it not been active and fruitful, and in a very remarkable manner from the beginning of the present century?—a period in which positive science has made such wonderful progress, and has been so productive of noble inventions. Then, perhaps, the motto given to this Journal might have been appropriate, "*Post tenebras lux.*"

Of the contents of this first number we cannot speak too highly. They are strictly scientific. The original articles are happily chosen, and are worthy of the distinguished men, their authors.

Should the after numbers but maintain the character of the first, the Journal can hardly fail of being a great success. Thinking of its future, we call to mind another Journal, one of the earliest and best, Nicholson's, which was begun in 1797, and ended in 1813. The principles on which that scientific Journal was conducted, as expressed by its worthy editor, were so excellent that we are tempted to transcribe the passage. Mr. Nicholson, after asserting his claim to fidelity and accuracy, proceeds:—

> "I have descended to none of the arts of book-makers. No commendatory letters have flowed from my pen: no imaginary congratulations are echoed: no pretended success forms the subject of my acknowledgments. I have confided in the sincere performance of my engagements with the public, and have solicited the approbation of good men by such means only as my heart could thoroughly approve. I trust it will give pleasure to many of my readers to hear that I have not been disappointed. The friendship and correspondence of men whose talents and virtues I revere, men whose approval constitutes the only estimable part of fame, have amply repaid my exertions; and in a commercial point of view, though I have found the sale of my book unequal to what might have been expected in times of less general distress, yet it has been progressively increasing, and sufficient to encourage my perseverance."

This was written after the publication of the first quarto volume, in 1798. In taking leave of the public in 1813, after adverting to the vast advances the sciences had made in the short interval, he adds:

> "I have felt it to be a proud situation to act as a journalist of our own improvements, along with those which have been made on the Continent. My labours have been remunerated partly by income and amply by the marks of public and private respect which have attended them."

We more than hope, we confidently expect that the editors of the new Journal of Science will be guided by the same principles, and be in like manner rewarded.

Art. VII.—*Manual of the Metalloïds.* By James Apjohn, M.D., F.R.S., &c., Professor of Chemistry in the University of Dublin.—*London*, 1864. pp. 592.

We could wish that this book had another title than that of 'Manual of the Metalloïds.' It is to the latter word that we object, inasmuch as, though the term has often been used, there is little agreement amongst chemists as to the substances to be grouped under it. It was once, we remember, applied to potassium and sodium; now it is more usual to employ it to designate those simple substances which, on union with oxygen, form acids, of which sulphur is the best example, and hydrogen the opposite. The main objection to the term is, that it is an arbitrary one, and not founded according to the rules of a philosophical classification on just analogies. Dr. Apjohn, who seems fully aware of its defectiveness, says, he adopts it for want of a better. We think it would have been well to have discarded it altogether, as being an impediment rather than a help to the novice, and more likely to lead the mind astray, and produce confusion of ideas, than a sense of order.

Of the work itself, which is chiefly designed for the use of students, the opinion we have formed is favourable; if it does not add to the reputation of its author, certain we are that it will not lower that reputation—one so well earned as Dr. Apjohn's, as 'an able veteran teacher, and an original inquirer.

The introduction, occupying one-sixth of this compact and handsome volume, relates to the laws of chemistry. It displays a thorough mastery of the subject; and critical as it is, as well as explanatory, it may be read with profit by those who have made themselves familiar with the elements of the science.

The same remark and commendation apply to the body of the work, judging from those portions of it which we have read. In the account of the different substances, beginning with oxygen and hydrogen, ending with silicon, boron, and carbon, there is a happy combination of the historical and descriptive, with a detail of illustrative experiments which are equally instructive and amusing. Great accuracy is the general characteristic of the work; yet in one or two places we have met with an oversight. Thus, when treating of hydrogen, Dr. Apjohn justly states, that it was first distinguished as a peculiar substance by Cavendish in 1766, and "that to him the credit of its discovery is usually given, though in modern times it has been claimed for Watt." Had he said that the discovery of the composition of water had been advocated for the latter, he had been more correct. Watt himself made that claim, but never supported it; and it would have been well had his friends, after his death, followed his example, and have been contented in giving him credit for a happy conjecture.

The present volume is one of Galbraith and Haughton's manuals of the experimental and natural sciences. We hope it will have the success it deserves, and that its author may be induced to give to the public others, comprising the whole of chemical science.

ART. VIII.—*The Nullity of Metaphysics as a Science amongst the Sciences, set forth in Six brief Dialogues.*—*London*, 1863. pp. 104.

THESE dialogues are written with an amount of ability and acumen, and will be read with interest by the few who take an interest in metaphysical inquiries.

The observations of their unnamed author on language—on language in relation to reason—the offspring of reason, and, as such, characteristic of man—that which truly distinguishes him from the brute animal, are ingenious, and, we think, can hardly be controverted.

His main intent seems to be to raise his voice against German metaphysics, and the transcendental philosophy of the Germans. The following extract well expresses this:

> "In particular," the Crito of the dialogue says, "I am anxious to rid modern learning of the German elements, which, under pretence of furthering its foundation, is yearly shifting its points of view, without the least likelihood of settling, and ever throwing mystery around a path which would otherwise be plain. I recur to what I have already touched upon in a previous conversation. I ask why, when we have accepted the facts that all we know, or can know, is, in the *first* place, one's individual self with relation to all which is not oneself, and then as to that which is not oneself—namely, the world in which we live; *secondly*, the individual things into which this world becomes distributed as fast as we grow aware of the relations to each other of these component things, still and always with relation to oneself the cognizor—I ask why, when we have embraced these *two* all-embracing exhaustive truths—exhaustive so far as truth is now attainable by us—we are bound to admit, as a *third* element, the *idea*, as they name it, of the absolute, the unconditioned, the eternal, originating in what they call the pure impersonal reason, which they affirm to hold a place above our merely human or personal understanding?"

Whilst he refuses metaphysics a place in the sciences—i.e., according to him, a knowledge of phenomena and their laws, he admits that there may be a higher grade of metaphysics, as a science of sciences, distinct from phenomena. Further, he objects to that philosophy, which he calls, and justly, "a miserable philosophy, which would extinguish all our noblest aspirations, all the lights of poetry, all the sweet gleam of ultimate hope, all the comfort we derive from faith;" adding: "that we are intended to live in an hereafter is as clearly indicated by our straining to transcend the present scene of things, as the tendency of a plant to rise above the earth is proved by its early germination."

ART. IX.—*On Life and Death; Four Lectures delivered at the Royal Institution of Great Britain.* By WILLIAM S. SAVORY, F.R.S., &c. —*London*, 1863. pp. 203.

THIS is a very interesting little volume, and is deserving of a wide circulation. Addressed as the lectures were to a mixed audience of educated people of both sexes, such as invariably constitutes the audience in the theatre of the Royal Institution, they are admirably adapted to convey instruction on the most important subjects of Life and Death, not only to educated persons generally, but even to our brethren of the medical profession, who have not made physiology their special study, or who have not kept pace with its recent progress in its more abstruse departments. A great excellence of these lectures is that they are never superficial; limited, as they necessarily are, they comprise the results of advanced science, and it is on this account that we can recommend them to many of our readers of our own profession.

As Bichat's Researches on the same subject show the advance that physiology had made up to his time from that of Haller, so these lectures are equally well fitted to impress us with its subsequent progress during the present century, the treatise of the former, 'Sur la Vie et la Mort,' having been written at the beginning of it.[1]

The two or three passages which we have marked for quotation will, we think, both as regards matter and style, justify our commendation:

> "Life," as the author happily defines, "is not a state of resistance; even now erroneous views commonly prevail on this point. To say the least, changes are as active during life as after death. The proofs of this are clear and complete. We have only to remember that any man, under ordinary circumstances, in the course of a year, consumes, roughly speaking, something like eight hundred pounds of solid food, about an equal quantity of oxygen, and perhaps fifteen hundred pounds of fluid; that notwithstanding this vast supply, amounting in the aggregate to more than three thousand pounds, his condition during the whole period remains the same, or nearly so; inasmuch, as all this matter, after being assimilated into his structure, and forming part of him, is excreted or cast-off in quantity exactly equal to that taken in, but widely different in the forms which it assumes, and in the manner in which the several elements are arranged."

When treating of the alternation of action and rest, of waking and sleeping, as essential to normal life, he refutes the dictum of a high authority, that certain organs are instances of exception, such as the heart and muscles of respiration, which act whilst we sleep as well as when we are awake; they, as he shows, being in fact no such exception, keeping in mind their rhythmical action. He asks:

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[1] 1804.

> "Is not every interval of contraction a period of rest?" adding, "there is no evidence of any real exception to this law, only apparent ones. True, we say, for example, that the heart is always acting; but we see that its action, strictly speaking, is not continuous, but intermittent. Every part is subjected to alternate intervals of contraction and relaxation, to say nothing of the possibility that only some of the fibres of each part act at one time, while others are passive. The same may be said of the muscles concerned in the act of respiration, and of the plain or unstriped muscles generally."

The next passage felicitously describes and illustrates the natural course of the animal body:

> "To be born, to live, and to die, is the epitome of the history not only of every living being as a whole, but within this general life, of every particle of which it is constructed. 'As the race of leaves, such is that of men.' The fairy rings of our meadows, which puzzled and delighted us as children, illustrate simply, but forcibly, the relation of life and death. These magic circles of simple vegetable cells are continually spreading in ever-widening curves; and this is the result of decay and renewal—of life and death; the cells on the central side dying and disappearing, while simultaneously new cells grow and develop on the opposite one. Thus the loss on one side is met by reproduction on the other; old individuals die out, new ones succeed. The race continues and extends."

The following relates to what he believes to be a widely-spread error, the idea—viz., "that the moment of death is one of agony." He well remarks:

> "Now, those who have looked into this subject closely, agree in declaring that usually it is not so. In certain cases, the moments of dissolution may be one of extreme anguish, but these are exceptional. And indeed it follows from what has been already said, from the mode in which death supervenes, that the actual process of dissolution cannot be attended by pain or suffering."

He affords ample proof of this, confirming it by the authority of Sir B. Brodie, and of other authors, including Miss Nightingale. He might have quoted Haller in support of this consolatory truth. That great writer, concluding his chapter, bearing the title, 'Vita Humana et Mors,' says:

> "Animam Deo reddimus, cui soli ejus a morte status notus est. Adfulgentis tamen fugienti animæ spei non raro in moribundis signa vidi, qui serenissimo vultu, non sine blando subrisu, de vita excesserunt. Quæ ipsa mors sapientis hominis merito ultimum est atque potentissimum desiderium."

The last remark we have to offer respecting this excellent little work is, that it is written with so much judgment and good taste, that it may be read with satisfaction as well as improvement by the most fastidious. After its perusal we, for our part, feel only regret that its pages are so few, and that they do not comprise the discussion of certain questions which are interesting, either theoretically considered or in their practical bearing, such as, to use the words of Haller, "Num detur rejuvenescere;" another, "Vitæ genus"—i.e., the mode of life favouring most length of days. Much should we be pleased could Mr. Savory have confirmed Haller's conclusion regarding the last, that "Plerique, qui ad summam longævitatem pervenerunt, vitam secuti sunt contemplativam."

ART. X.—*Topics of the Day: Medical, Social, and Scientific.* By JAMES ANSLEY HINGESTON, Regist. Pract., &c.—*London*, 1863. pp. 400.

As this book has been written, we have no doubt, with a good intent, we should be sorry to disparage it, yet we cannot take upon ourselves to commend it. As its title indicates, it affords information on a variety of interesting and unconnected subjects. Some of the information—indeed, a large proportion of it—would have been more valuable had it been more precise; and we should have felt more confidence in many of its author's conclusions had his reasoning been governed by a more exact logic.

A considerable portion of the work, ninety-two pages, is on cholera and on meteorology in connexion with health—this a reprint of papers written on the spur of the occasion chiefly during the prevalency of the disease. Both subjects are obscure. Of the ætiology of cholera, it must be confessed, we are as much in the dark as when we were first astonished at its outbreak in India, now nearly half a century ago; and as to meteorology, how little do we know with any precision of its agency in the production of diseases. A science it can hardly be called: the data for constituting it such, and the mind for the labour, are yet, it would appear, wanting. This being the case, however meritorious it may be to collect observations on the passing changes in our atmosphere, we cannot but hesitate in attaching importance to, and placing confidence in, fleeting phenomena, after the manner of our author. One passage may suffice to show—and we give it without comment—how readily Mr. Hingeston comes to a conclusion:

> "That cholera is not confined to any particular localities, high or low, terra firma or the ocean, is proved by its geographical history. It may have originated in the jungles of Jessore, in Bengal; but we have heard of it in the snowy passes of the Caucasus, along the sea-beach in various parts of the globe, in sandy deserts traversed by caravans, on board-ship frequently, on alluvial plains, such as those on which Moscow stands, or in cities as elevated as that of Mexico, the loftiest in the world; in the *fiords* of Norway and Scandinavia, upon the shallow Baltic, the deep German Sea, and the broad Atlantic. But whenever authentic accounts have reached us respecting the state of the atmosphere during its prevalence, they are uniformly the same. It would be exceedingly interesting could we be placed in possession of accurate meteorological accounts from Marseilles, visited so fearfully at present by this pestilence."

Vaccination is the next topic discussed. This we think a redeeming part of the book, for it is carefully and well written, comprising a good summary of the literature of the subject, with excellent practical rules for performing the operation, founded on the experience of the author. The remarks with which he concludes this section are deserving of special attention:

> "It is surprising how many loose opinions are afloat upon vaccination, and pass current for truth among the public. But if we deduce the few facts that are really known, we shall find the absolute data are only three—viz., 1. The source of genuine lymph, i.e., the cow; 2. The natural history of the vesicle; and 3. Its power of protection when rightly performed. Other points of no less importance meet us at every step; but they are enveloped in doubt, elude our grasp, and escape from sight. They remain as subjects of investigation for the philosophical student; and it only requires a school regularly organized, appointed, and authenticated, to bring them within the focus of enlightened research. The cow might be variolated from time to time, so as to procure a genuine supply of fresh lymph whenever it is called for; vaccine wards might be opened; vaccinators, as well as a lecturer on vaccine, might be officially installed; and pupils accurately educated might be sent forth, capable of discerning between true and false pock, as well as skilful in the art of keeping up a constant succession of approved vesicles. *At present* (1863), *we have nothing of the sort.* The legislature may shift the scenery for us; but the chief and real actors of the drama must be those who preside over the destinies of the medical profession as its preceptors, guides, and friends."

The second part of the work treats first of hypochondriacism—a reprint of a review by Mr. Hingeston of "Confessions of a Hypochondriac." In reading it there are passages which suggest that the critic at the time of writing it must himself have been labouring under the ailment. The following is an instance in point, and there are others of which the style and sentiments are as open to objection:

> "Actual vice apart, there is nothing in this world worthy of either joy or grief; for, strictly speaking, success and failure are equivalent terms; and the last state of experience is to receive all that happens without emotion, and to regard events with a cool, deliberate, and dispassionate eye. Too serious a reflection on the transitory nature of earthly goods is more than enough to drive any one crazy, unless he be blessed with a constitution congenitally apathetic, stoical, or extremely religious. But religion itself is, when abused, a powerful source of hypochondriacism. For either it is believed and disobeyed, which gives rise to reproach of conscience; or else it is believed in a wrong sense, whereby the terrors of Divine justice are made to supersede the promises of mercy and forgiveness; although when received in its true sense, according to the rule of faith, religion is a charm that sweetens everything. *O potestas quid non præstas homini?*"

Of the remaining topics our notice must be brief, and we regret to say they can be but little favourable. Amongst them are "Cleopatra's Death," and "Horace's Death." Why these should be considered "Topics of the Day," we are at a loss to understand; and we are equally at a loss to comprehend the author's conclusion relative to the cause of the latter, that it was diabetes. These two deaths end Part II. of the volume. They are preceded by "Ethnological Psychology," "The Human Brain," "Deformities of Infantile Crania," "The Deformed and their Mental Characteristics." On all these subjects, with one exception, there is little reliable information to be found, but much that is hypothetical and fanciful. The exception is the article on the Deformities of the Infant Cranium, which is simply and well written. It is a reprint, it would appear, of a well-known French work by Dr. Foville.[1] A question may arise whether this writer, and Mr. Hingeston after him, have not taken an extreme view of the bad effects of the compression of the head by head-dresses. Be that as it may, there is no room for doubt that the practice has its risks, and that there is the best chance of healthy development and growth the less the form of the calvaria is interfered with, the degree of injury probably being in proportion to the compressibility and compression of the parts.

Part III. comprises topics as miscellaneous as the preceding, and as incongruous. Their titles are the following—"The Wear and Tear of Medical Life," "Three Thousand a Year," "Homœopathic Triumph," "The War of 1854," "The Peace of 1856," "The Indian Rebellion in its Moral and Psychological Aspects," "Orientalism," "Ancient and Modern Civilization," "Change of Scene." As we cannot praise the manner in which they are treated, we pass them by, remarking merely that the several articles are striking examples of the *cacoethes scribendi*. There are other terms of disapproval that occur to us, but we refrain. We are reminded, in reading a large portion of the book, of a favourite word of Coleridge—"Ultra crepidation." We cannot but regret that Mr. Hingeston has not restricted himself to practical professional subjects, on which (judging from his observations on vaccination), he might have written with much credit to himself.

ART. XI.—*Over de Elasticiteit der Spieren. Academisch Proefschrift ter verkrijging van den graad van Doctor in de Geneeskunde, aan de Hoogeschool te Utrecht.* Door ANTHONY PIETER VAN MANSVELT. —*Utrecht*, 1863.

On the Elasticity of Muscles: an Academic Thesis for the degree of Doctor of Medicine in the University of Utrecht. By ANTHONY PETER VAN MANSVELT. Royal 8vo, pp. 60.

THE manner in which the author of this dissertation has handled his subject is so far abstruse that such of our readers as feel an interest in the question will no doubt take an opportunity of consulting the original work. We would, accordingly, refer the reader to the illustrations contained in the work itself, references to which we have retained in the quotations which we make use of in our notice :—

> "A muscle is an organ which can act only by altering its length: on the force with which its extremities, in the several degrees of shortening, are drawn towards one another, depends its power of work. As the muscle in its action always experiences resistance, and its shortening, in connexion with the force of this resistance, determines the work done, its *elasticity* is the principal element in the estimation of its action. Consequently, to ascertain the amount of

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[1] Déformation du Crâne résultant de la Méthode plus Générale de couvrir la tête des enfans.

elasticity present in different conditions of the muscle (in rest, in various degrees of contraction, in different states of fatigue) would be of great importance." (p. 1.)

Schwann[1] was the first to show that in muscles elasticity comes into play. Weber[2] investigated this subject more fully; Volkmann disputed some of his views, and between the two a contest arose, which has not been brought to a satisfactory conclusion. Wundt[3] also published investigations on the subject.

Still difference and uncertainty upon some points remained; fatigue, too, had a disturbing influence upon the experiments of the observers just named; at length a mode of investigation by which previous difficulties were avoided was proposed, and provisionally applied by Professor Donders.[4] By this method the elasticity of muscles can be directly observed; it can be overcome at will by work, or by the exertion of force without work; the muscles can be brought into an arbitrary degree of contraction, and the curve of the extension, by different weights and under different circumstances, can be approximately determined, while the experiments, being made with the flexor muscles of the arm of the experimenter himself, are directly applicable to human physiology.

The author details Weber's mode of investigation, alludes to Heidenhain's[5] experiments, made with a view to ascertain whether the condition of the muscle with reference to its elasticity is modified by a galvanic current traversing it or not, on which point that observer arrived at a negative result; refers at some length to the researches of Volkmann,[6] Wundt, and Harless,[7] and to those of Adolf Fick[8] on the closing muscle of mussels (shell-fish); and having given an anatomical description of the elbow-joint and of the flexor muscles of the arm, proceeds to the detail of his own experiments, based upon the suggestions of Professor Donders. As to the object of his researches, and the mode of carrying them out, we shall allow him to speak for himself:

"The immediate object of our experiments was," he says, "to ascertain how far a muscle in the living body, brought into a definite degree of contraction, is extended by a given weight. This was investigated for the flexors of the arm. The experiments were made in the following manner:

"The experimenter arranged himself behind a vertically-placed board, so that the latter came between the side of the body and the arm. On the front of the board was a graduated scale; in the centre, whence the arcs for this scale were drawn, was a small excavation (*a*), lined with a soft substance. In this excavation the internal condyle was placed. A moveable perch (*b*) was fixed behind the board in a stand, so constructed that the humerus stood vertically when its upper part touched the perch. The whole board could at will be pushed higher or lower, and so fixed. At the lower extremity of the radius a bracelet (*c*), furnished beneath with a hook (*d*), was now fastened.

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[1] Müller's Physiologie, Band ii. pp. 59–61.

[2] Art. Muskelbewegung, in Wagner's Handwörterbuch der Physiologie, Band iii.

[3] Die Lehre v. d. Muskelbewegung. Braunschweig, 1858.

[4] Verslagen en Mededeelingen der Kon. Akad. van Wetens. 1859, D. ix. p. 113.

[5] Monatsber. der Berl. Acad., 1856, p. 128. Communicated by Dubois-Reymond.

[6] Ber. der Kön. Sächs-Ges. der Wissensch., 12 April, 1856, and Müller's Archiv, 1857, 1858, 1860, and 1862.

[7] Untersuchungen über die Muskelstarre, Sitzungsber. der Baier. Akad. der Wissenschaften, 1860, p. 428.

[8] Beiträge zur vergl. Physiol. der irritabelen Substanzen, 1863.

"In performing the experiment, the forearm was brought to a particular degree (which was read off) by means of a wooden triangle, kept with the one rectangular side over the upper part of the radius, with the other along the edge of the board. When the arm was brought into this state, the weight (*e*) was suspended with a strong silk thread to the hook, and was for a short time (in the first series of our experiments for ten seconds) held at the same height. The thread was now cut with the scissors, and the forearm sprang up to a certain degree, which was again read off in the same manner. During the experiment the arm is quite in a state of supination, and the hand is not fixed by its flexors and extensors. The person experimented on must practise until the springing-up of the arm occurs quite involuntarily. In daily life, it often occurs that we have to keep the muscles for some time at the same degree of contraction; this was therefore attended with no difficulty during the bearing of the weight in our experiments. If the weight was now suddenly removed, the arm made a movement which did not proceed from the will. On perceiving an unexpected movement, for example, of the arm, we are very much inclined, either actually to suppress it or to complicate it with some other involuntary movement. Neither the one nor the other must here take place; the muscle must remain in the same state of contraction which it was in so long as the weight hung upon it, the attention of the experimenter must be directed to the arm, only to prevent any independent movement superadding itself to the rising of the limb. That this is not so difficult as it probably appears to some, we can satisfy ourselves by placing a weight on the hand, and causing it to be unexpectedly removed by another person; we shall soon learn by the muscular feel to distinguish whether the movement made by the arm is purely the result of *elastic* contraction of the muscle, as was intended in our experiments, or whether the muscle, on the removal of the load, contracts more or less than while bearing it. We may assume that after some practice the movement was quite involuntary, as, from the constancy of the numbers obtained, and from the similarity and peculiar form of the movement, would appear to have been the case.

"The weights which we used were 3, 5, 7, 9, 11, and 16 ounces; smaller weights seemed to have little or no constant effect upon the muscle; on cutting off larger weights, the tendency of the humerus to spring up with the forearm was so great that the internal condyle, even when strongly applied, was lifted out of the hollow, so that the experiment failed.

"The course of the experiments was as follows:—If we began by keeping the arm quiet, with different loads at the same point, it sprang up, when the weight was cut off, to different heights; the muscles had therefore been contracted to different degrees. But the object was, in each series to ascertain how much the muscles, at the same degree of contraction, were extended by different weights; therefore we wished to make the arm, after having been differently laden, to spring up to a definite degree. For this purpose, it was sought at what degree the loaded arm must be kept, in order to make it spring up to the degree fixed upon; and now some consecutive experiments were made with these numbers. While bearing the load, the arm fell or rose almost always one or two degrees; if this difference did not amount to more, one or two degrees, with the sign + or − were added in the same direction in which the arm had removed from the starting-point. Often the arm rose from the fixed starting-point to one or two degrees higher or lower than that intended; then the difference was added to or subtracted from the terminal figure, just as was done with the commencing number. From the commencing numbers thus obtained, of usually five experiments with the same weight, the average was considered to be the true one.

"With a view as far as possible to exclude the influence of the will, the order of the experiments was changed in all manner of ways. Some experiments were taken consecutively with the same weight, or the weights were changed in each experiment in a different order; the arm was placed alternately at a high and low figure, &c. All this gave the same results, which appeared to us to afford some guarantee for the involuntariness of the movements. We shall now proceed, having described the experiments, to their analysis, in order to ascertain what inferences we may draw from them.

"(*b.*) *Analysis of the Method.*

"In the analysis of our experiments, two questions are to be answered: *How does the weight act upon the muscles?* and *What was ascertained with respect to their extension?*

"The following may serve to answer the first question:—The weight, suspended on the extremity of the radius, naturally exercised, at different angles of the arm, a different tractive force upon the muscles. The latter, besides the appended weight, had also to bear that of the forearm, which likewise, in its different positions, had not the same influence. On the different muscles, too, or parts of the same muscle, the weight had a different effect. From the angle of the fore and upper arm, with each point of origin and insertion, it could be calculated what the length of each flexor muscle in its integrity, and that of its muscular fasciculi alone were; the lengthening and shortening had reference, of course, only to the latter, as the tendons might be considered as inextensible for our weights. (Wundt found that the muscle of an ox of 49 5mm. in length was extended 0·480mm. by 10 grammes, that is, $\frac{1}{103}$; the tendon of an ox of 193·5mm. in length was extended by the same weight 0·20, that is, $\frac{1}{967}$.) For the arm with which the experiments were made, we determined the lengths of the muscles and of their fasciculi, also the distances of the axis of the joint from their origins and insertions, from the average of different measurements made upon eight arms of dead bodies. That our calculation can possess only approximative value, and that it cannot lay claim to absolute accuracy, is evident.

"What light now did our experiments throw upon the extension of the muscles?

"While the loaded arm was held on a level with its origin, the muscle possessed its natural length (λ) + the extension (d) by the weight. To keep the arm at this height, the muscle must, during the experiment, remain at the same degree of contraction. If the weight was suddenly removed, the muscle contracted again to its natural length at that degree of contraction; this was made known by the arm springing up. We could calculate λ and $\lambda + d$, and thus the extension at different degrees of contraction and by different weights was found. If fatigue came into play, the matter was somewhat different; this took place in the second series of our experiments. We had by practice to attain to that point, that the arm, while bearing the weight, was held motionless, and that, on cutting off the weight, the muscle contracted, without the will having influence upon the movement of the arm.

"The reason why we have not taken the resistance of the antagonistic muscles into our calculation is, that we could in the first place satisfy ourselves by feeling, that the extensors of the arm remained relaxed during the experiment. That the relaxed muscles exercise little force, although by their natural attachment they are kept somewhat extended, we can easily perceive, when we consider how great the extensibility of the living muscle is. This we can distinctly see, when one of the muscles of the eye, for example the

M. rectus internus, is paralyzed. During the first days, before the rectus externus has acquired a contraction, the pupil is still brought quite into the middle of the space between the eyelids, solely through the *elastic* contraction of the rectus internus, a proof that its antagonist in a state of relaxation exercises no force worth speaking of.

"(*c.*) *Estimation of our Experiments.*

"From the numbers obtained was to be calculated, by what part of their length the muscular fasciculi were extended by a given weight. The fasciculi of the different muscles, and also the different fasciculi of the same muscle, have their points of attachment at different distances from the axis of the joint, the longest bundles being inserted farthest from the axis. This observation, communicated to us by Professor Donders, was made by him on a previous occasion. He thought that such a connexion might very well exist, that the extensions in movement for all fasciculi of co-operative muscles might be equal. But he remarks, that even when this relation did not obtain, all fasciculi would co-operate by their elasticity, although in a different degree. Now, in our measurements we found the relation between the length of the fasciculi and the distance at which their point of attachment lies from the axis of the joint, to be such as we expected, with slight variations, which probably depended on faults in the measurement, which it was difficult to avoid, as the boundary between muscular and tendinous substance is not everywhere strictly defined. Perhaps it would not be unimportant to ascertain whether this relation is met with also in other parts of the body. As in consequence of the law, that *the length of a bundle* and the *distance of its point of attachment from the centre of motion*, stand to one another in a *fixed relation*, it is indifferent of which bundle we calculate the length at different angles of the arm, provided this bundle fulfils the law laid down, we might for all fasciculi of the muscles, whether they belong to the biceps or to the brachialis internus, take an average; the curves of contraction found for this bundle must hold good also for all others. We assumed for our arm such an average, determined what part of the whole length of the muscle must be allowed for the tendon (which we, as has been said, consider to be inextensible), calculated the length of muscular fasciculus + tendon for the angle, which the arm made without and with an appended weight, and thus ascertained how great the distance between the two extremities of the fasciculus in each case was. In order to know by what part of its length the fasciculus was extended, we subtracted from both lengths the unalterable length of the tendon, and thus found the measure of extensibility for the muscular fasciculus.

"Having ascertained how much the muscle was extended, it had next to be calculated by what weight this was effected. The weight acts in different positions of the arm, with different force upon the muscle. The annexed figure may illustrate how the value of the weight for each position was calculated.

"Let B D be a rod, moveable around its centre of motion, D; A D, another rod connected by a hinge with B D, and fastened vertically; A C, an elastic band. Let P be the weight, which, attached at B, counterbalances over two pulleys the weight of the rod, B D.

"Let Q be a weight, which in B is attached to the rod B D; the question now is—*What force must the band* A C *exercise to keep the rod* B D *with the weight* Q *in that position at rest, when the weight* P *is taken away?*

"If P be taken away, two weights act on the point D; P and Q in the direction of gravity B Q. These weights we can replace by a weight R, attached at C; in order to exercise the same force, R must be $= (P+Q) \frac{B\,D}{C\,D}$, as P + Q acts on the arm of the lever, B D; R, on the arm of the lever

C D. Let the line C *b* represent the amount and direction of the force R. This may again be resolved into the forces C *a* and C *c*. C *a* acts in the prolonged direction of A *c*. Now, as $\Delta\, C\,a\,b$ and $\Delta\, C\,A\,D$ are similar, C *a* : C A = C *b* : A D, and, consequently, $C\,a = C\,b \times \frac{C\,A}{A\,D}$.

"According to our construction: C *b* is = the value of R attached at C, $= (P + Q)\,\frac{B\,D}{C\,D}$, therefore $C\,a = (P + Q)\,\frac{B\,D}{C\,D} \times \frac{C\,A}{A\,D}$.

"This formula gives us the force which C A (the muscle) must exercise in each position of the arm, in order to support the weight of the arm + the appended weight.

"In order to apply the formula, we required to know the weight P, which attached at B (the extremity of the radius), made equilibrium with the arm, when A D (the humerus) was placed vertically. This weight we determined directly in a dead arm; a cord was fastened to the radius in the same place where in my experiments the bracelet was worn. This cord ran over two pulleys which had little friction. The humerus was fixed vertically, and now the value of P was determined. After that it was ascertained how much water was displaced by the arm to the elbow-joint, as well as by my arm; the radius of the dead arm was measured, and now we had, supposing the specific gravity of both arms to be the same, all the data necessary to find P for my arm. P and Q were therefore known quantities.

"In order to find the average measurement of the muscles, we made determinations of the distances and lengths sought for in eight dead arms; but rather than employing the averages thus obtained, we used the determinations made on one arm, which deviated little from these averages."

The author appends the following as an example of his mode of calculation:

"The arm stood, when loaded with 11 ounces, at 90°; after cutting off the weight, it sprang up to 74°.

"The muscle is the side of a triangle, of which we know two sides and the intervening angle.

"The one side (*a*) is the distance between the axis of the joint and the origin; the other (*b*), the distance between the axis of the joint and the attachment of the muscle; the intervening angle (C) is in the one case 74°: the length of the muscle (*c*) is sought.

"Now $C^2 = a^2 + b^2 - 2ab \cos C$.

"If we here substitute the known values, we find:

For *c*, if C is=74°	218·71
And if C is=90°	229·00

"The tendon, subtracted from both lengths, gives for the extension of the muscular substance: 10·29 mm.

"At the angle of 90° the muscle bore the weight of the arm (P)+the appended weight (Q).

"For my arm P is =8·2 ounces, Q is here=11 oz.

"We therefore have for the value of the weight, when the arm stands at 90° $(P+Q)\,\frac{B\,D}{C\,D} \times \frac{C\,A}{A\,D} = 19.2\,\frac{265}{37} \times \frac{229}{226} = 133{\cdot}07$.

"At 74° the muscle bore only the weight of the arm, therefore: $P\,\frac{B\,D}{C\,D} \times \frac{C\,A}{A\,D} = 8{\cdot}2\,\frac{265}{37} \times \frac{218.71}{226} = 56{,}835$, the muscle was, therefore extended 10·29 minim by 76·235 ounces, or, according to the formula of Weber, $E = \frac{L'-L}{L'+L} \times \frac{2}{p'-p}$ its measure of extensibility was $\frac{61-50{\cdot}71}{61+50{\cdot}71} \times \frac{2}{133{\cdot}07-56{\cdot}835} = 0{\cdot}012631$ since $\frac{p'-p}{2} = 9{\cdot}4952$ kilogrammes, that is, the muscle was, in this case, with a length of 106;85 mm., when loaded with 9·4952 kilogrammes, extended $\frac{1}{12631}$ of its length by 1 kilogramme."

The author next proceeds to a statement of his experiments on the unfatigued muscle:

Experiments with Taking Away the Whole Weight.

The results are given in a table.

"It is to be observed that the position of the arm was measured by the upper surface of the radius in perfect supination. As, properly speaking, the position of the axis of the arm was sought, we have throughout subtracted half the thickness of the arm at the radius (for my arm on the edge 4°) from the first and terminal numbers. The loading lasted in this series of experiments each time ten seconds.

"The numbers in the table comprised under the words 'commenced at,' are averages usually of five different experiments in the same series, taken with the same weight.

"It appears that, on different days, some difference was found in the commencing numbers, which differences, however, have a very slight influence on the measure of extensibility of the muscles.

"The same experiments were performed by Professor Donders as by me. At first he obtained much greater and less regular results than I had found. He thought he thus discovered that the dividing of the thread, which always before was somewhat felt, excited some voluntary action. The method was therefore so modified that the weight was not cut off, but was only suddenly and quickly raised, so that the arm could spring up freely. This modification had the effect of making his numbers nearly equal to mine."

b. Estimation of these Experiments.

A table is given, in which the result of the estimation of the author's experiments is to be found. The *first* column contains the consecutive numbers of the experiments; the *second*, the angle which the forearm made with the upper arm after the weight was cut off; the *third*, the angle which the loaded forearm made; the *fourth*, the weight which was suspended on the radius; the *fifth*, the weight which the non-extended muscle had actually to bear; the *sixth*, the length of muscle + tendon; the *seventh*, the length of the muscular fasciculus alone; the *eighth*, the average length; the *ninth*, the average weight for which the measure of extensibility is calculated; the *tenth*, the value of E = coefficient of elasticity.

"In the calculation of the value of E we have placed; l = the length of the muscular fasciculus, when the arm was unloaded, p = the weight of the arm acting in the direction of the muscle expressed in kilogrammes.

"In fig. 3, we see the course of the extension by different weights at different degrees of contraction of my flexors; in fig. 4, the same for Professor Donders. The abscissæ represent the weights, the ordinates the lengths.

Here, too, the weight is expressed in kilogrammes, the length in mm. The first table refers to *my* experiments."

c. Experiments with Removal of a Part of the Weight.

Besides the experiments above given, some were made in the following manner:

"The arm was loaded with two weights, each of 5oz., and set at 90° (that is, its axis at 94°); after ten seconds the ten ounces were removed, and the arm sprang up. With the same load it was again placed at the same number; after ten seconds, one weight of 5oz. was removed, the arm still loaded with the second weight, again sprang up. After some seconds' rest it was set with this last weight at the number to which it had last risen; after ten seconds the weight was removed, and the arm again rose."

In this way two series of experiments were made, the results of which are given in a table, on which the author makes the following remarks:

"We see that the muscle was 5·14 mm. shorter when the load was at once taken away than when it had to raise a part of the weight before that too was removed.

"It is remarkable that it had to perform nearly as much work in order to bring the arm from 94° to 82°, as to bring the arm + 5oz. from 94° to 90°, the arm alone from 90° to 84° 5′. The work in the first case was: 1·2509, in the last 1·2454 kilogrammes. After the performance of the same work, it had in the two cases a different length; whence it would appear that it cannot be regarded altogether as elastic, but that, during the extension, its nature changes, as Fick also remarked."

V. GENERAL RESULTS OBTAINED FROM THIS SERIES OF EXPERIMENTS.

The author deduces as the principal result of his experiments: *that the muscle is, at least within certain limits, extended in proportion to the increase of the weights:*

"We see in fig. 3, that the lines A, B, C, D, E, F, and I, confirm this law, as their deviations from the perpendicular are easily explained from the many sources of error which our method presents; the lines G and H, indeed, exhibit greater deviations, but in an opposite direction, so that we have here probably to do with errors in the observation. In fig. 4, we perceive that the lines A, D, E, F, G, and H, deviate not much from the perpendicular, while B, C, and I, exhibit more deviation, although not so as to make them quite at variance with the law, which we thought we observed.

"Our experiments confirm Wundt's opinion respecting the relation between extension and weight. In the first place, we found the extensibility of the muscle tolerably equal in different degrees of contraction. In the values of E we may observe that there is no regular rise or fall, but that the differences in each separate series nearly compensate each other; the same we can deduce from figures 3 and 4. For as we can determine by what part of its length the muscle is extended in all series by the same weight, if we, namely, for each line separately divide the greatest length which the muscle had in all series with the same weight (in fig. 3 with 12, in fig. 4 with 17·5), by the shortest length with the same weight in all series (in fig. 3 with 6, in fig. 4 with 9), we obtain the fraction sought.

"We see that it does not hence appear that the muscle, with different measures of contraction, has a different co-efficient of elasticity: *à priori* we should expect that it should diminish with the increase of contraction, as it diminishes with the transition of the muscle from rest into activity. On this point the earlier experiments, with cut-out muscles of frogs, could not lead to any result, as the measure of contraction could not in them be voluntarily regulated.

"The only difference that we constantly found between the much and the little contracted muscle was, that in strong contraction the bearing of heavy weight became much more rapidly troublesome, and even painful, than in slight contraction.

"We found, moreover, for the biceps and brachialis internus, the measure of extensibility as an average from the whole table, for Professor Donders, 0·00836, and for me, 0·00941 for 1 kilogramme.

"In order to calculate for what weight this measure held good with reference to the primitive bundles, we had to ascertain the cross section of the flexor muscles and that of the primary bundles. We found for the cross section of the long head of the biceps, on an average, 530 sq. mm.

"Of the short head . . . . 452 „
"Of the brachialis internus . . 614 „

"The cross section of a primary fasciculus is about $\frac{1}{800}$ mm., and therefore the muscles named contain together about 798,500 primary fasciculi.

"All these fasciculi are extended in the same proportion; therefore the measure of extensibility is for each fasciculus as great as for the whole muscle. While the whole mass of the brachialis internus bears 1 kilogramme, each primary fasciculus bears $\frac{1}{800}$ gramme; and therefore the measure of extensibility in the fasciculi is related to that weight. The result, consequently, is, that a primary bundle, extended by 1 milligramme becomes nearly 1 per cent. longer. In this calculation, the supinator longus is not taken into account, as with the angle, which the arm made in most of our experiments, it certainly did not cooperate much as a flexor."

VI. Experiments on the Influence of Fatigue.

Fatigue Dr. Mansvelt endeavoured to develop in three modes :—

1st. By causing the same weight to be borne for a long time.

2nd. By causing a heavy weight to be borne.

3rd. By causing a heavy weight to be lifted.

The first four series of experiments made on this subject were by Professor Donders; the three following by the author. Two series, which do not differ much from these, have not been communicated.

"From our experiments it appears, in the first place, that after bearing for a long time, the arm rises more than after bearing for a shorter time. Rising higher indicates that the muscle was more strongly contracted; hence it appears that the muscle, in order to keep the arm at the same height, must have a less natural length, that therefore its extensibility increases under the burden. This influence is more plainly perceptible for slight than for greater differences. The consequent fatigue, arising from long-continued extension, again rapidly disappears, but especially when work was performed by the lifting of heavy weights.

"In this last case, it is to be observed, that the amount of mechanical work performed was not very great; in the regulated displacement of smaller loads the muscles of our arms performed much more work, yet in this instance the rapidly-occurring and increasing feeling of fatigue soon prevented us from proceeding. Probably after more work the result of fatigue would have made itself longer felt. We often observed that the *feeling* of fatigue arose and continued in a high degree when the numbers no longer indicated increased extensibility.

"If we put together what we ascertained from this series of experiments, we arrive at the following results:

"In a short time after long-continued extension the muscle becomes more extensible; the will must stimulate it more strongly, if it is to maintain the same shortness.

"The influence of fatigue rapidly disappears.

"The *sensation* of fatigue is no measure of the condition of the muscle."

To make our readers acquainted with Dr. Mansvelt's interesting researches, we have found it necessary to quote largely from his work, and to follow the text of the volume pretty closely. The nature of the subject, involving so much of direct experiment and of calculation, appeared to call for such a course. The facts observed by him will speak for themselves, and will be found to supply an important contribution to the physiology of the muscular system: but, as before said, we would refer those who take special interest in the question to the original.

ART. XII.—*The Weight and Dimensions of the Heart in Health and Disease.* By THOMAS B. PEACOCK, M.D., F.R.C.P. (Reprinted from the 'Monthly Journal of Medical Science.')—*London*, 1854. pp. 64.

THIS pamphlet contains a large series of observations, with calculations, and the inferences thence deducible. They have been made by the author himself. The tables include 198 observations of the *weights* of organs regarded as healthy, and 150 observations of the weights of hearts presenting either primary or secondary disease. The *measurements* are of 42 healthy and of 45 diseased organs. The whole series have been carefully selected from a larger number of observations. We cannot here deal with such a solid array of facts, otherwise than by urgently commending them to the attentive study of pathologists.

ART. XIII.—*On the Arcus Senilis, or Fatty Degeneration of the Cornea.* By EDWIN CANTON, F.R.C.S.—*London*, 1863. pp. 227.

AN exact title-page of this work would have run thus: 'On Fatty Degeneration in general, and of the Cornea in particular, as a sign of advancing Age.' This will appear from a glance over its pages; thus the first chapter gives a very good description of this morbid change as obvious to ordinary inspection, and an account of the structural alteration as revealed by the use of the microscope. In the second chapter we have the family histories of several examples showing hereditary tendency. In the third chapter several cases are quoted in which the arcus senilis followed on ophthalmia. Longevity forms the topic of the fourth chapter, in which mention is made of instances of prolonged life without the occurrence of this sign of old age. The following chapters embrace the consideration of fatty and calcareous degeneration as the consequences of age, disease, and intemperance. The sum of Mr. Canton's researches is quite in accordance with the exact and concise statement of Paget—viz.: "The arcus senilis seems to be, on the whole, the best indication which has yet been found of proneness to an extensive or general fatty degeneration of the tissues." This proposition is fully borne out by the observations, and by the very full literary history of the subject presented in the pages of Mr. Canton's essay.

ART. XIV.—*On the Causes and Treatment of Closure and Immobility of the Jaws.* By CHRISTOPHER HEATH, F.R.C.S. 1863. Pamphlet. pp. 23.

THREE cases of immobility of the jaws, caused by rigidity of cicatrix, or by a bridge of bone, serve as the occasion for describing an operation for its relief, which consists in the formation of an artificial joint in the lower jaw, as introduced by Dr. Frederick Esmarch, Professor of Surgery in the University of Kiel, and which gave most satisfactory results in one of Mr. Heath's cases. The author quotes also the various causes of immobility of the jaws, as given by Dr. Gross of Philadelphia. The literature generally of closure of the jaws has not been overlooked by Mr. Heath. The cases related possess great surgical interest, and reflect a considerable degree of credit upon the skill and patience of the operator.

The cases related being strictly surgical, and, as such, of the severest kind, do scarcely answer to the scope of the title-page, which would seem to embrace all varieties of immobility of the jaws. This affection is not infrequently met with in an acute form, the result of inflammatory action in the ligamentous structures, induced by, or extending from, acute inflammation of the periosteum of the alveoli and maxilla, and obstinate though it be in this form, yielding usually to active local treatment. So far, therefore, this brochure is incomplete, whereas a complete treatise founded upon Mr. Heath's clinical observations would have possessed great value. For the history of those forms of spurious lock-jaw, dependent upon the more easily removeable causes, we would refer our readers to several discussions which are reported in the 'British Journal of Dental Science.' At the same time, we would direct attention to an ingenious instrument devised by Mr. Cattlin, of Highbury, for the purpose of separating the jaws. This instrument is so contrived as to effect either rapid or gradual extension of the joint, with complete control over the degree of force exerted.

ART. XV.—*Das Puerperalfieber am St. Petersburger Hebammen-Institute, von* 1845—1859, *mit Bezugnahme auf gleichzeitige Verhältnisse in den übrigen Gebärhäusern und dem Weichbilde der Stadt St. Petersburg.* Von Dr. TH. HUGENBERGER, Sen., Professor und Accoucheur am Hebammen-Institute.—*St. Petersburgh*, 1862.

Puerperal Fever in the St. Petersburg Midwives' Institution, from 1845—1859, *with a Comparative Account of the Experience of the other Lying-in Hospitals, and the Town Practice of St. Petersburg.* By Dr. TH. HUGENBERGER, Sen., Professor at the Midwives' Institution.—*St. Petersburgh*, 1862.

THE object of this work is to trace the causes of the development of puerperal fever. The method pursued is admirably calculated to place the great question of etiology in the clearest possible light. Minute, searching, exhaustive, and candid, it ought to carry conviction to every impartial mind. We have here brought together for comparison and contrast the experience derived from the various conditions under which obstetric practice may be conducted. We see exhibited in the most striking manner the different results attending hospital and home obstetric practice. The history of puerperal fever—that ruthless scourge which alone destroys more lying-in women than all other causes united—is carefully traced through its various phases and invasions in the Midwives' Institution of St. Petersburg, during a period of fifteen years. The mortality of the remaining lying-in hospitals of the town during the same period is also exhibited. And, lastly, the general obstetric history of the extra-nosocomial population is contrasted with that of the hospitals. We are thus enabled to arrive at certain definite conclusions of the highest medical and social importance.

During the fifteen years, 1845—1859, there were delivered in the St. Petersburg Midwives' Institution 8036 women; of these, 1614 fell ill, 306, or nearly four per cent., died. The diseases affecting these women are divided into: 1. General diseased conditions, which for the most part were continued from pregnancy into labour and the puerperal state. Of these, there were 40 cases of bronchial catarrh, 31 of lung-inflammation, 17 of lung-consumption, 50 of cholera, 16 of typhus, 12 of ague. Of 220 cases of this class, 46 ended fatally.

In the second class are enumerated 38 cases of laceration of the perinæum, 5 of laceration of the uterus, 5 of paralysis of the lower extremity, 32 of inflammations of the breast, 26 of anæmia from placenta prævia and flooding, 25 of eclampsia, and 23 of mania. Of 160 cases of this class, 22 ended fatally.

In the third class are ranged cases strictly belonging to the puerperal process. The large proportional number of 1234 cases of sickness to 8036 labours, calls for the remark, that every case marked by strong shivering with consecutive heat, pulse rising to 100—120, and disturbance of the puerperal function, was classed under the head of puerperal fever. These cases are subdivided as follows;

A. *Localized Puerperal Processes.*

1. Mild metroperitonitis	278	without a death.
2. „ endometritis	306	„ „
3. Severe metroperitonitis	346	with 116 deaths.
4. „ endometritis	76	„ 30 „
5. „ metrophlebitis and pyæmia .	123	„ 78 „

B. *Non-localized Puerperal Processes.*

1. Mild inflammatory fever	88	without a death.
2. Severe acute septicæmia	14	with 14 deaths.
Totals	1234 cases,	238 „

In addition to the registered deaths, not less than 15 deaths occurred amongst the women who persisted in leaving the institution whilst sick.

Tables showing the numbers of cases of sickness occurring during each month are given. The general results are as follows:

In winter .	2106 lying-in women were treated,	405 fell sick,	88 died.
In spring .	1934 „ „	292 „	66 „
In summer .	1927 „ „	227 „	39 „
In autumn .	2069 „ „	310 „	45 „

In tracing the history of the several epidemics of puerperal fever which arose in the hospital, Dr. Hugenberger observes that there were days when nearly every lying-in woman in the institution appeared more or less ill, and most of the cases beginning gradually and mildly had the most pernicious results. The ruling form was metro-peritonitis. One epidemic that raged in the spring and autumn of 1848 is thus described. It began in March, culminated in May, abated in June and July, rose again in August and September, to culminate in October. In the spring, out of 98 lying-in women 20 were attacked, and 8 died. In the autumn, of 66 women 19 were attacked, and 10 died. As early as December, 1847, isolated cases of puerperal fever occurred, mostly with obvious causes, and lasted in a milder form and with no death during January and February. In March, there died quickly, after a patient with suppurating struma who took metrophlebitis, a primipara, who had metro-peritonitis after difficult turning for cross-presentation. From that date cases of sickness increased in number, and culminated in May, when the first complications of the puerperal fever, with the then commencing epidemic of cholera, were observed. Henceforth, the adynamic stage of the puerperal fever was hardly to be distinguished from the cholera, and it often remained doubtful to which pest the patient fell a victim. Several forceps operations occurred in this period through the paralysis of the uterus occasioned by the cholera, and four Cæsarian operations were performed upon moribund cholera patients. By restricting the admissions, the number of cases of sickness was diminished during June and July. The number, however, rose again in August and September. In October it was at its acme, when there were two cases of gangrene of the genitals, brought about by difficult labour, occasioned by pelvic contraction and cross-presentation. At the beginning of November the Institution was closed for three weeks, but fever nevertheless happened in the following year.

The disastrous influence of the hospital fever—for so puerperal fever may surely be called by as good a title as "hospital gangrene"—was transmitted to the children. The infantile mortality rose and fell with the prevalence and remission of puerperal fever, and that not simply in consequence of privation from the loss of the mothers, but from well-defined diseases, the expression of blood-poisoning.

Of the 8210 children born, 498, or 6 per cent., were putrid; but in the spring of 1846, the proportion was 10 per cent., 8 per cent. in the autumn of 1848, 12 per cent. in the winter and spring of 1856, and 9 per cent. in the winter of 1856. These were the periods of greatest intensity of the puerperal fever. During the years 1849, 1852, and 1854, the proportion stood at the normal rate. As to the mortality of the live-born children, the years 1846 and 1856 were especially unfavourable. Whereas, the mean of the entire fifteen years was 2·6 per cent., the mortality rose in the first year to 12, and in the last to 14 per cent. The difficulty of making a full comparison was increased by the custom of transferring the children of single women directly to the Foundling Hospital. Most of the diseases of the newborn children arose amongst those whose mothers were sick, and only in the rarest exceptions amongst those whose mothers were in good health. During the acme of the puerperal pestilence of 1846, there were numerous cases of sudden death of newborn children from eclampsia and trismus; and most healthy, blooming, and well-nourished children sank rapidly. In this year also, acute atrophy, with jaundice, erratic and phlegmonous erysipelas, and induration of the cellular-tissue, were not infrequent.

In 1856, 32 cases of malignant ophthalmia occurred in January and February. Numerous pyæmic exanthemata, bullous and gangrenous erysipelas, umbilical phlebitis, and acute exsudative peritonitis, accompanied the severest affections amongst the mothers.

Dr. Hugenberger makes an elaborate etiological analysis. He observes, that neither clinically nor anatomically has he discovered anything new, that is special, in his cases of puerperal fever. The influences of age, number of pregnancies, social state, general health-condition of pregnancy, previous stay in hospital, excessive distension of the uterus, duration of labour, floodings, street-births, and premature labour, the carrying dead children, suppressed milk-secretion, errors of diet, chills and emotions, and of epidemics, are minutely investigated.

He next enters upon a comparative examination of the history of the Imperial Foundling. Here there were 45 beds. During the same fifteen years 16,011 labours took place; 3842 cases of sickness, and 85 deaths were recorded. Corrections made, the death-rate was 4·3 per cent.

Another comparison is drawn with the lying-in department of the Kalinkin-Town-Hospital. Here there are 15 beds. In the fifteen years, 1288 labours and 20 deaths, giving a death-rate of 1·5 per cent., were recorded. The cases of sickness were isolated, and generally followed pathological labours.

A third and most valuable comparison is then made with the childbed returns of the town of St. Petersburg—that is, with labours conducted outside hospitals. Excluding the country and non-Christian communities, there were registered in the fifteen years 235,293 births, and 2520 deaths in childbed. Of these, 25,711 labours and 1117 deaths belonged to the hospitals, giving *one-ninth* of the labours and *nearly half* the deaths. Subtracting these, there remain for home practice 209,582 births, and 1403 deaths. For the hospitals there is a mean mortality of 43 per 1000, for the town of 7 per 1000. He quotes Marc d'Espine, as showing that the childbed mortality of Geneva was 8, of Prussia 7, of Belgium 6, of England between 6 and 9 per thousand. He cites Faye as giving the mortality of Norway in 1851 as 8; and refers to Robert Barnes' Statistics of the London Royal Maternity Charity, which give only 3 deaths in a thousand. This last fact seems to have been the immediate cause of Dr. Hugenberger's careful inquiry into the relative mortality of lying-in women in hospitals and in home practice. The following conclusions were elicited—

1. Entire want of coincidence was shown during 28 months of relative greater mortality in the town with lesser mortality in the hospitals; and, on the other hand, 26 months of lesser town mortality were accompanied by great hospital mortality; and lastly, during 19 months of mean town mortality, there was excessive hospital mortality during 5 months, and lesser during 14 months.

2. A partial coincidence showed itself with the Midwives' Institute during 26 months of lesser, and during 9 months of greater mortality.

3. Complete coincidence was shown during 11 months of contemporaneous greater mortality, during 32 months of lesser, and during 8 months of mean mortality, between the hospitals and the town.

4. The so-called puerperal-fever epidemics coincided only exceptionally with the prevalence of the disease in the lying-in hospitals.

5. In the town, puerperal fever never raged as an epidemic during the fifteen years.

6. The fluctuations of *plus* and *minus* in the town mortality always stood in opposition to the contemporaneous unfavourable or favourable relations in the hospital.

7. The seasons of the year alone exercised a constant influence upon the health of lying-in women, inside and outside the hospitals.

Having by a most laborious and exhaustive process established the fact that hospital puerperal fever cannot be explained by epidemic influences, the author proceeds to examine the action of cadaveric infection, of self-infection, and of hospital air. Denying that Semmelweis' theory of cadaveric infection is one of general, or even of large application, he admits that several undoubted cases of this nature occurred in the Midwives' Institution. Instances of self-infection were more numerous; but the predominant cause was hospital air. The influence of this condition was manifested, as elsewhere, notwithstanding the denial of Kiwisch, by the transmission of severe and even fatal metroperitonitis and erysipelas to non-puerperal women. The transmission to the children we have already adverted to. Several marked cases of female pupils of midwives thus suffering are related.

It cannot be maintained that the condition of the St. Petersburg Hospitals as to site, ventilation, cleanliness, or other sanitary arrangements, explains the prevalence of puerperal fever in them; and there is no pretence for suggesting that the pestilence was introduced from without. The internal regulation seems to have been as well cared for as possible. We can only fall back, as Dr. Hugenberger does, upon the one constant and incontrovertible fact, that the aggregation of lying-in women in hospitals will surely develop the pestilence amongst them. This comparison between the mortality of hospital and domiciliary midwifery, so ably and so completely drawn for a long period of years, ought surely to fix the attention of all Continental administrations. If, in the face of such facts as Dr. Hugenberger urges, lying-in hospitals be still defended, it must be shown that they are called for by some overwhelming social necessities, that leave poor, suffering, helpless, confiding woman no refuge, no resource, but the venomous charity of the puerperal-fever factory. But surely no such overwhelming social necessity need be recognised. It cannot be true that lying-in hospitals, which generate puerperal fever and administer the deadly poison to those who fly thither for health, are the highest blessings of Christian pity and Christian charity. We blush with shame and indignation in having to denounce another argument sometimes urged—namely, that lying-in hospitals must be maintained because they are schools of medical science. Schools of Medicine! In the name of that Christianity which gave birth to hospitals, we ask, what is the end and aim of medical science? To save or to destroy? Where do we find justification for treating one portion of our fellow-creatures as vile bodies, to be made the victims of artificially induced disease and death, for the benefit of another portion? Science so acquired is accursed in its very origin, and cannot bear good fruit. Dr. Hugenberger himself, we are glad to learn, does not shrink from the logical consequence of his researches. He proposes the establishment in St. Petersburg of a system of domiciliary obstetric assistance, on the plan of the London Royal Maternity Charity.

We heartily wish him success in his undertaking, and that an institution, whose beneficence shall be unalloyed, will so prosper in St. Petersburg as to compel imitation in the other capitals of Europe.

ART. XVI.—*A Manual of Chemical Analysis, Qualitative and Quantitative. Part I. Qualitative.* By H. M. NOAD, F.R.S., Lecturer on Chemistry at St. George's Hospital.—*London*, 1863. pp. 211.

In 1852 there appeared the second edition of the work which heads this notice. In the interval a few additions and numerous omissions have been made in the treatise. New furnaces, lamps, and other apparatus have been described; two additions have been made to the re-agents (fluosilicic acid and molybdate of ammonium), but in other respects we do not find at first sight much change. But on examining the text carefully, it is evident that the information conveyed on all or most matters having a direct bearing on medicine and allied arts has been greatly increased. For instance, the important subject, the detection of arsenic, has received ample and excellent treatment at the author's hands.

A few slips occur. Dr. Noad (p. 147) talks of making phosphorous acid by acting on *sesqui*chloride of phosphorus with water. We never heard of this sesqui-compound before. Again, the following paragraph is somewhat perplexing (p. 72):—

> "39. Oxide of ZIRCONIUM ($Z_2 O_3$).
>
> "*General characters.*—When pure and calcined, glucina is a white infusible powder; when ignited, it becomes brilliantly incandescent; it is sufficiently hard to scratch glass. Its specific gravity is 4·3. After having been ignited it is soluble only in concentrated sulphuric acid. Its soluble salts have a purely astringent taste, without any sweetness."

Here not only is an unusual atomic weight for zirconium adopted, but its oxide is confounded with glucina, the salts of which are, moreover, stated to be without sweetness. Such errors as these might easily have been avoided, but they do not detract in any considerable degree from the merits of the book as a manual of analysis.

We have already stated that the account of arsenic and its detection is admirably exact and comprehensive; we must award similar praise to the sections on "Poisoning by Lead" (pp. 86—93); "Poisoning by Mercury" (pp. 97—100); and "Poisoning by Copper" (pp. 103—107).

The concluding twenty-four pages of Dr. Noad's "Manual of Analysis" contain an account of the way in which the reactions previously described in that work are employed in the detection of acids and bases. The absence of tables renders the sequence of the processes less intelligible to the beginner; this absence is particularly noticeable in the case of the "Preliminary Examination" (pp. 188—192). In this most important part of an analysis the student's progress is greatly helped if he arrange his results in three parallel columns, respectively headed "Experiment," "Observation," "Inference;" the table by which he works being similarly constructed.

ART. XVII.—*Statistical Report of the Health of the Navy for the Year* 1860. (Ordered by the House of Commons to be printed.) 8vo, pp. 230.

THIS is the fifth of the annual Statistical Reports of the Health of the Navy, for which the profession and the public are so much indebted to Dr. Bryson. The present is in some respects the most valuable of the series, in consequence of the larger extracts now given from the reports of the medical officers themselves, in the case of unusual and excessive sickness in certain ships, explanatory of the probable causes thereof, and suggestive of the measures required for preventing its recurrence. We hope to find this plan carried out still more fully in future, and somewhat after the method now adopted in the annual Returns of the Army, so that the information derived from the two services may be better compared together for the benefit of both, as well as for the promotion of public hygiène everywhere. There is an opinion very general among the medical officers of the Navy that the health of our ships of war has, on the whole, anything but improved of late years; and convinced, as they are, that not a little of the sickness and mortality is due to causes which might be greatly abated by sanitary appliances, they rightly feel that after what has recently been done, and what is still doing, for the hygienic welfare of the soldier, it is high time that public attention be now directed to rendering their service as thoroughly effective as possible in the vital, as well in the material, elements of its strength.

In 1860 the numerical force of the Navy was 64,025. Of this number, more than 5 per cent. (53·6 per 1000) were daily on the sick list, so that the really effective force was at all times less than the numerical force by 3456 men. The deaths were 938, or in the proportion of 14·7 per 1000; and the number of men invalided, or discharged from the service on account of disease or injury, was 2844. The total permanent loss, therefore, amounted to 3782 in the course of the year, or at the rate of 59 out of every thousand men in the service. The average of the four preceding years was higher in point of mortality (19·1 per 1000), but lower in point of invaliding (33· per 1000); so that we shall not be far from the mark in estimating, taking one year with another, the total annual loss in the Navy at between fifty and sixty men in every thousand of the force. This is certainly a very heavy decrement.

Of course, the rates of sickness and death, &c., vary much on different stations. Let us look therefore a little more closely into the matter, and select for the purpose two of the healthy stations and two of the usually unhealthy. Taking the last five years, we find the following averages:

	Death-rate.		Invaliding-rate.
Home	10·3 per 1000.	...	25·1 per 1000.
Mediterranean	11·2 „ „	...	35·8 „ „
N. America and West India. .	25·1 „ „	...	27·3 „ „
East India and China	44·0 „ „	...	61·2 „ „

These figures indicate a state of things anything but satisfactory to the sanitary statistician. The yearly loss is seen to be 35 per thousand in our Home fleet, a rate much above what most people suppose. The death-rate alone, making every allowance for the excess of deaths from violence among seamen, is quite as high as among males of the same ages in our civil population. How many of the invalided men die in the course of the year, there are no means of determining; if we take the proportion as low as 10 or 15 per cent. of them, the death-rate of the Navy on our own coasts will stand very high compared with other classes of the community. In the fine climate of the Mediterranean, a still more unfavourable result appears; and when we come to the West Indian and East Indian stations, it presents a dark and distressing picture of the sickness and mortality of our fleet in these climates. Can nothing be done to reduce these excessive death-rates in the above table? This is naturally the main question that occurs to the mind; but there is another, which we had better look at first. Are things better or worse than they were twenty-five or thirty years ago? The determining of this point will clear the way for answering the other. Taking the seven years, 1830–36, the average death-rate in the West India squadron was 19·6, and the invaliding rate was 39 per 1000 of the force. During the seven years, 1837–43, the average death-rate in the East India and China squadron was 44·7, and the invaliding rate was 28 per 1000. By comparing these rates with the averages of the five years, 1856–60, the reader will discover anything but grounds for believing that the health of our ships of war has been improving of late years.[1] The very instructive paper of Dr. Nelson, R.N., in this Review for July, 1863, on the "Medical Results of the recent Chinese Wars"—viz., of the first from 1840 to 1842, and of the second, from 1857 to 1860—which well deserves the study of every medical officer in the Navy, affords a striking comment on this remark. Notwithstanding many signal advantages in respect of diet, season of the year when hostile operations were carried on, &c., which the fleet possessed in the second war over the fleet in the first Chinese war, the proportion of sickness and death was considerably higher during the former than it was, under less favourable circumstances, fifteen years before! Has any searching official inquiry ever been made to discover the cause, or causes, of this unlooked-for and most unsatisfactory state of things? We fear not; for indeed one of the chief defects in the organization of the medical department of the Navy seems to be, that there is no principal medical officer in a fleet, as there is in a division of the army, whose duty it is to supervise and report upon the health and sanitary condition of the whole force upon a station. The result is that we have never a comprehensive and connected account of any wide-spread or remarkable sickness in any one of our fleets; and no systematic inquiry is ever instituted to discover why one or several ships should suffer inordinately, while others similarly situated in respect of outward circumstances should be comparatively healthy. The deputy-inspector on a station ought clearly to make himself acquainted with the hygienic and sanitary condition of all the ships in the command as far as possible, and maintain a general supervision over them; so that he may be able to communicate to the Admiralty the results of his yearly experience. At present, there are only the individual and unconnected returns of the medical officers of the different ships transmitted to Somerset-house; and from these, the statements in which, on matters of fact, are sometimes not accordant, no thoroughly satisfactory account of any general sickness can ever be drawn up. This defect is strikingly conspicuous in the report now under consideration, that for

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[1] In the Mediterranean fleet, the average rates of mortality and invaliding during the seven years, 1830–36, were 11·1 and 25 8 per 1000. As compared with 1856–60, the death-rate has been stationary, and the invaliding-rate has increased of recent years.

1860, during which two of the fleets suffered an unusual amount of sickness, to which we shall direct the reader's attention.

In several of the ships of the Mediterranean squadron, a low asthenic form of pleuro-pneumony[1] prevailed to an enormous extent, causing a serious detriment to the service by the large temporary and permanent losses among the crews. One ship, the *St. Jean d'Acre*, had no fewer than 156 cases of the disease, besides an extraordinary number of cases of other severe pulmonary affections, in very many of which a phthisical complication existed. Considerably more than a third of the aggregate number of these diseases throughout the entire fleet, mustering upwards of 14,000 men, occurred in this one ship. Two or three other vessels suffered much, but none to the extent of the *St. Jean d'Acre;* while the rest of the squadron were nearly exempt, although engaged in similar duties, and exposed to the same atmospheric and climatorial influences. But it was not merely in respect of pulmonic diseases that the excessive sickliness prevailed in some of the ships of the fleet over others. The following tabular arrangement of the number of cases, during the year, of diverse classes of disease in four of these vessels shows at a glance the striking difference:

	Crew.	Fevers.	Diseases of Lungs.	Diseases of Bowels.	Ulcers.
Marlborough . . .	1145	... 10	... 129	... 64	... 75
Agamemnon . . .	840	... 17	... 241	... 58	... 30
St. Jean d'Acre . .	815	... 136	... 401	... 171	... 115
Cressy	720	... 12	... 298	... 254	... 102

The table speaks for itself, and cannot fail to suggest to any one acquainted with ships that there must have been something in the sanitary condition, structural or otherwise, of the last two ships different from that of the first two, to account for the extraordinary difference in point of sickliness among crews under similar external circumstances. That the principal cause was the more defective ventilation and the greater overcrowding of the men at night in their quarters, scarcely admits of doubt by the extracts from reports of the medical officers, which deserve, and we hope may obtain, the serious attention of the Admiralty. To what an extent these evils are carried in respect of some crews may be judged of from what we are told respecting the *St. Jean d'Acre:*

> "All the ship's company, with the exception of the cook and about forty boys, slept on the lower deck. *The hammock-hooks were placed ordinarily at only fourteen inches apart—less than the average breadth of the men's shoulders; consequently while in harbour, when no watch was required at night, and all hands turned in, they formed a compact mass close beneath the beams, the only air available for respiration being above them. All the ports, as well as the round scuttles, were kept closed at night.*"

With such a state of atmosphere in the between-deck of our ships of war, who can be surprised at the terrible outbreak of epidemic disease we every now and then hear of in the Navy? as was the case with some of the vessels of the West-India squadron also during 1860, which we shall now briefly mention. Of 143 cases of yellow fever during the year, 72 were fatal. No fewer than 39 of the deaths occurred in the *Icarus* among a complement of only 110 men, and this, too, within two months! At this rate, the whole crew would have perished in six months! But, unfortunately, this was not the only regrettable incident in connexion with the sad event. When this floating pest-house arrived at Port-Royal, a boat's crew of healthy lads, from another vessel, was not only sent on board of the *Icarus*, but actually allowed to go down into her between-decks, which were, of course, in just the same condition as an ill-ventilated ward would be where a score of malignant typhus patients were lying. Many of them caught the fever, and became the means of communicating it to others; and, to add to the painful mismanagement on the part of the naval authorities (would that we could entirely exculpate the medical officers!), more than a hundred supernumeraries from one of the infected vessels were crowded into a steamer—the *Barracouta*—with her own full complement of 180 men on board, and sent off to the northward, "in order to remove them from the influence of yellow fever, then existing at Port-Royal." The disease broke out soon after leaving, and several deaths occurred before she could reach Bermuda. Although there is a naval hospital there, the commanding officer *refused to allow the sick to be landed*, and actually ordered the vessel off for Halifax immediately after being supplied with necessaries. Adverse winds prevented her reaching that port, and the result was, that she was obliged to make for England, where she did not arrive till the 25th of October, four weeks after leaving Jamaica. Of 21 cases of yellow fever which had occurred, 6 had proved fatal. Altogether, the history of this episode appears to us to be one of the most discreditable to the administrative management, as regards health, of a ship of war, we have ever heard of. If such serious blunders are liable to be committed, it is surely high time that the general sanitary discipline of the whole service be looked into. The refusal to permit the sick to be landed at Bermuda was a monstrous and most unjustifiable act, which must be reprobated by every enlightened physician. There are many other highly interesting matters that might be gleaned from this report, if our limits permitted. The 'Annual Reports of the Army and Navy' now constitute a most valuable contribution to hygienic literature, and should be regularly consulted by all who wish to follow the progress of the science of public health.

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[1] In several respects, the disease seems to have had a resemblance to the epidemic pleuro-pneumony in cattle; there were grounds for believing that it was to some extent contagious, as several of the patients in the Naval Hospital at Malta became affected with it soon after the admission of the sick from the infected ships. The subject has been brought by Dr. Bryson before the Epidemiological Society, and we may therefore hope that a fuller investigation of its history may yet be published.

ART. XVIII.—*Zur Diagnostik und Behandlung der Chronischen Laryngitis. Ein Laryngoskopischer Beitrag.* Von Dr. H. VOGLER, zu Ems.—*Berlin*, 1863.

On the Laryngoscopic Diagnosis and Treatment of Chronic Laryngitis. By Dr. H. VOGLER.

THE author describes the changes produced in the several structures of the larynx by chronic inflammation, as shown by the use of the laryngoscope, taking each part of the organ in succession. The treatment described consists in the application of various topical agents and the employment of Ems waters by inhalation and internally. Several cases are related in illustration of the therapeutic benefits derived therefrom.

ART. XIX.—*A Yachting Cruise in the Baltic.* By S. R. GRAVES, Commodore of the Royal Mersey Yacht Club.—*London*, 1863. pp. 399.

THIS book has been lying on our table a considerable time; and during our delay the Austro-Prussian occupation, Federal folly, German sentimentalism, Coburg pretensions, and Augustenburg assertions, suddenly elevate the Baltic and its basin into active interest.

We own to having been negligent of our many Northern friends, and had we suspected that 'A Yachting Cruise' had contained so many interesting notices of our old acquaintances of the Friedrichs Hospital at Copenhagen, and the Seraphim at Stockholm, we should not have waited for German injustice before presenting our readers with a notice of the book.

A more pleasant, readable production than this Baltic cruise we have not met with for a long time; and if the gallant commander can make his *Teine* walk as easily and pleasantly over "the Baltic's broad billows" as he can take us along with him through his pages, we hope there may be a vacant berth on board the little schooner when next he takes her "foreign."

Should his friend the doctor be particularly engaged, we shall be glad of the opportunity of taking his place. We have tasted the quality of many waters, European, Asiatic, and African, but we have no pretensions to the many accomplishments, scientific, social, and professional, which must have made this member of our craft so acceptable a shipmate.

Indeed, we cannot help thinking that the doctor has had a hand in the book, unless, indeed, Commodore Graves is one of those few fortunates of our profession, who having " walked it" in early life, has found the love for our noble calling cling to him when he had raised himself above the pursuit of it as an occupation and a bread-winning.

With the exception of the Russian part of his journey, we are familiar with a very large part of his cruise; we have more than once been on the same ground and the same waters.

We have visited Denmark more lately than the commodore, and we can with pleasure doubly endorse his opinion of the sort of reception the Trek-oner and its associated forts will give the German fleet (God save the mark) *when* it appears before Copenhagen. The descriptions given of country costumes, customs, and manners, is life-like and lively; and we have a strong suspicion that the bridal festivities which the author witnessed were at the house of a valued friend of our own, whose Welekomme has more than once greeted us at Bellevue.

The description of the voyage along the Gotha Canal is capital; and though no doubt the greater facilities afforded by the railway from Stockholm to Gottenburgh will always attract the time-tied traveller, yet three days can hardly be more pleasantly or restfully passed than in viewing all the varied beauties of the lakes, waterfalls, and woods traversed by the Gotha Canal. The larger part of this cheery book is for the general reader, and will be found on the swinging book-shelf of every yacht afloat; but there is much that has a special interest for the physician and philanthropist. We join most heartily in the commendation bestowed upon the hospitals at Copenhagen and Stockholm. We have visited hospitals from London to Lisbon, from Bergen to Barcelona, from Scutari to St. George's, Hyde-park Corner, and we have never seen any better managed than those of the Seraphim at Stockholm, and the Frederichs at Copenhagen. We have seen brilliant surgery, diligent investigation, and accurate diagnosis in both places, and we give ourselves the pleasure of joining in a hearty recognition of the courtesies of the accomplished Professor Saintisson, of the former place.

We subjoin a rather long extract on the subject of the Copenhagen hospitals, because we agree with all the commodore there says about them, and also because it may serve as an example for imitation among ourselves:

> "From the Frederichs Hospital we proceeded to the Commune, or general hospital of Copenhagen, situated at the opposite side of the street, where we found Dr. Nithhusen going his rounds. This hospital is one of the oldest in the city, covers a large space of ground, as the buildings are detached and irregular; it makes up 1200 beds, and receives all classes of diseases save those which are contagious; there are 113 nurses in attendance. We went through some of the wards, which appeared very clean, and well ventilated. The children were mixed with adults throughout the wards; and Dr. Nithhusen considered they are thus better attended than if separate, and have a good influence on the adults.
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> "As a class, the patients seemed much superior to what one meets with in English hospitals; and it was quite evident that there is a much stronger desire on the part of respectable patients to go into such institutions than with us. For a small payment special privileges are given, which entitle the patient to greater comfort and seclusion. It appears the several trades' unions, or benefit-clubs, pay a doctor for attending the members, as the English clubs do, but make a practice of always sending the serious cases to the hospitals, where, by paying a small sum, they secure these privileges—an arrangement well worthy of imitation by our unions.
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> "The Commune is now building, a very fine hospital about a mile outside the city; and when it is completed, the old one will be given up to lunatics and incurables. The old Commune Hospital is of more than one hundred years' standing; it contains 800 beds for curable diseases, and others for incurable, who are never refused when applying there for refuge for the rest of their lives, if they can prove that they cannot subsist in any other way, and if their disease is incurable. The curable are either received free or at the rate of seven shillings per week. Those rates are usually paid by musical clubs, of which almost all the artisans of Copenhagen are members.
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> "All paupers requiring medical aid are received free. The new Commune Hospital will be finished next summer for the reception of patients; it has likewise 800 beds, and is a splendid building, with all the modern improve-

> ments, such as ventilation, warm air, heating, steam-washing and boiling; baths of all sorts; a separate building for the insane, another for infectious diseases. It has a charming situation, open on all sides, and is only five minutes' walk from the town. It was commenced three years and a half ago, and will cost a very large sum of money." (p. 72.)

This hospital, when we were there last summer, was finished, and we presume is now tenanted, and fully deserves all the author's praise.

> "From the Commune Hospital we went to an institution recently erected about two miles outside the city, called the Sick House; it is for the reception of decayed gentlemen and women, and is supported by donations. There are 69 inmates, who are provided with every necessary and comfort except their clothes. Donors of 550*l.* have the privilege of nominating one person a home which continues for life; or by an annual payment of 30*l.* a subscriber can use the same privilege as long as the payment is kept up.
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> "The total which can be accommodated is 104; the inmates can board in their own rooms, or in the saloon, at their option, and are regularly waited on.
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> "This institution is justly regarded with great pride by the citizens of Copenhagen, and supplies in their city a want which is deeply felt by the middle classes among ourselves; for it is remarkable that while every care is taken of the poor of the lower classes, little or no provision is made for the reception of the better educated, who feel poverty more acutely, and are more deserving of our sympathies." (p. 44.)

The tendency to abuse *medical charities* is a growing and mischievous one: a very large number of persons, there can be no doubt, are habitually relieved at our public hospitals who are perfectly able to help themselves in obtaining medical aid. If a question is raised about their being fit recipients, they are often saucy, and claim it not as a favour, but a right. This is a double injustice; it defrauds some poor person, and detains from some class or other of medical practitioner a certain amount of remuneration. Nor is this all the harm which promiscuous and superabundant charity is inflicting on the poorer classes themselves: the honourable self-reliance is destroyed, self-help is ignored; all the preliminary degradations of confirmed pauperism are fostered; and all this time the contributors to this baneful system are under the impression that they are doing good in their generation.

The value of an institution where, for a moderate sum, a sick or injured person can obtain as a right, not as an alms, all the advantages of hospital treatment, is far beyond the immediate influence on the patient. How many an artist, author, or governess, can testify to this! and with what a sad reflection did we read but a few days back that one of our eminent sculptors, whose busts now grace the saloons of royalty and rank, closed his eyes in a London hospital!

In conclusion, we will hope that the *Teine* is chartered for fresh trips, and that we may have fresh "logs" of the same agreeable kind as this 'Baltic Cruise.'

PART THIRD.

Original Communications.

Art. I.

Sketch of the Geography, &c., of the Plague during the Present Century.
By Gavin Milroy, M.D., F.R.C.P., &c.

Towards the end, and particularly in the closing year, of last century, the plague reigned with epidemic force throughout Lower Egypt, and along the entire extent of the northern coast of Africa. The Barbary states suffered very severely; in the towns of Oran and Algiers the loss of life was enormous. Nor was the disease less destructive in 1799 in the kingdom of Morocco, where it followed upon a widespread famine caused by the devastations of locust swarms over the country.[1] In the city of Morocco, and also in Mogadore on the Atlantic seaboard, it seems to have reached its acme in the months of July and August, to have begun to decline at the end of the latter month, and to have ceased about the beginning of October. In smaller towns and in villages, the visitation usually lasted from three to five weeks only. The disease was always observed to be of a much more malignant and fatal type at the height than during the decline of the epidemic. Persons, however, who had fled from an infected place, and returned only when it had nearly ceased, were often attacked with a virulent form of the fever. The outbreak in Morocco had been preceded by an extensive visitation of small-pox; and the same occurrence had been observed there in respect of the plague on former occasions. A destructive murrain among cattle followed in its wake.

To what extent the plague prevailed in the European provinces of the Ottoman empire at this period, we are unable to say. In the closing years of the century it appears to have existed in some places in Poland and Volhynia, and also in the Banat within the Austrian dominions.

The French Expeditionary Army to Egypt under General Bonaparte, which left Toulon in May, 1798, reached Alexandria at the beginning of July, and, immediately after the capture of that place, marched on to Cairo, which was occupied before the end of the month.

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[1] After an excessively wet autumn in 1799, and the great scarcity and general distress which followed thereon, typhus was unusually prevalent in Great Britain during the first and second years of the present century.

It was about 30,000 strong on landing. For the first five or six months, dysentery and ophthalmia were the chief diseases among the troops; and it was not until towards the close of the year and the commencement of 1799 that genuine pestilential fever began to manifest itself, and then chiefly in the sea-coast towns of Alexandria, Rosetta, and Aboukir, springing up usually about that season. The force for the invasion of Syria, which mustered about one half of the entire army, left Suez in February, and soon afterwards suffered severely from the plague and other endemic diseases, chiefly bowel disorders, at Jaffa and at Acre. The tendency to dysenteric affections was believed to be greatly promoted by the use of impure water for drinking, and also by the insufficient clothing of the troops in a climate subject to great alternations of temperature, especially from day to night. The diet, too, was often faulty. Before the return of the force to Egypt in the month of July, upwards of 700 of the men had died from the plague, independently of heavy losses from other diseases incident to the climate and the nature of the service (the mortality from which is not stated by Desgenettes); while the number of deaths from the plague, during the same period, among the troops remaining in Egypt had been between five and six hundred, and that from other diseases between six and seven hundred. During the rest of the French occupation, which lasted to August, 1801, the plague appears to have been considerably less prevalent than during the first twelve months, although up to the end it continued to manifest itself at times in the army, more particularly in the spring and early summer months of each year. Whenever it was present, the existence of southerly winds and of a hot, humid atmosphere seemed to promote its spread and malignancy.

Notwithstanding the plague and other intractable forms of fever[1] endemic in Egypt, and to which foreigners are far more liable than the natives of the country, the total mortality from disease in the French army during the first two years and a half of the campaign seems to have been under, rather than above, what it has often been in other parts of the world under somewhat similar circumstances of active service,—provided, indeed, the official statement of the deaths up to the end of 1800 can be at all relied on. During this period, the entire mortality was set down at 8915. Of this number, 3614 were slain in battle, 290 were killed by accidents, and 854 died from wounds received in action, giving a total of 4758 deaths from the casualties of war. The deaths from disease during this time are stated to have been only 4157, of which number 1689 were from the plague, and 2468 from other maladies, chiefly dysentery.[2] But, judging from the results of other campaigns in almost every other part of the world, these figures must give but a small part of the actual losses from disease in the French army during their stay in Egypt; and probably we should not be far from the truth, if we raised the mortality from this cause to three times at least the number stated. No official account was ever published of the actual number of the thirty thousand troops, sent from France in May, 1798, that returned to their homes in September, 1801, after the capitulation of Alexandria. Nevertheless, there seems no good reason to question the statement made by Desgenettes as to the comparative salubrity of the climate of Egypt upon the troops, and as to the sick and death-rates having been on the whole less in the Egyptian army than in any of the other armies of the French Republic, engaged at that time in Europe in active hostilities. Much would, of course, depend upon their commissariat arrangements; and it is well known that Bonaparte ever paid vigilant attention to this most important subject of military hygiene.

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[1] Larrey has described an aggravated form of bilious remittent fever, having many of the characters of genuine yellow fever, which attacked many of the wounded in April and May of 1800, and proved very fatal, sometimes on the second or third day of the attack. It was confined chiefly to the patients in the ground floors of the hospitals, which were crowded at the same time.

[2] Desgenettes remarks that at first the mere suspicion of a fever being the plague (it was always called "pestilential fever") prevented due attention being paid to the sick, but that afterwards, when the disease was better known, the hospital attendants waited on them with much greater readiness. It often sprang up in different localities

The British force, about 17,000 strong, sent out from this country under the command of Sir Ralph Abercrombie, reached Aboukir Bay on the 1st of March, 1801; but in consequence of the bad weather the troops could not be landed till early in the morning of the 8th, when they were at once engaged with the enemy, actions being fought on that day, and on the 13th and 21st of the month. One corps remained stationed at Alexandria to blockade the city, with a detachment encamped at Aboukir, which is about eight or ten miles distant. The other corps of the army moved on against Cairo, and in their advance had to pass through many villages, which were more or less infected with the plague, and where it was impossible to prevent all intercourse with the people. Yet the troops escaped almost entirely both during the march and after arriving at Cairo, although that town had, at an earlier period of the season, suffered pretty severely. That year, as usual, the fever ceased to prevail there in the third or fourth week of June. At Aboukir, where a frightful massacre of several thousand Turks by the French had taken place the year before, and the corpses had been insufficiently interred, the plague began to manifest itself among our soldiers in the second or third week of April, and soon afterwards it appeared among the troops stationed in the lines before Alexandria. The character of the disease at first was not well marked, so that doubts as to the true nature of the earliest cases were entertained by some of the medical officers. As might be expected, the accommodation for the sick was extremely defective, consisting only of a few rude huts, which the French had built and occupied while they were in possession of the place. With the view of preventing, as was then believed, the spreading of the disease, these wretched hospital huts were strictly guarded by sentinels placed around them, so as to bar any

spontaneously, and without any traceable communication with infected places; and by merely crossing from one bank of the Nile to the other, the infection frequently ceased. Larrey, too, makes similar remarks—viz., that the disease is propagated in different ways, and that there is scarcely any danger from going into a sick ward, "provided there be a current of air" through it.

egress from them, as well as all communication *ab extra* with the infected. No wonder then that, as we learn from Dr. Franz, who had charge of the plague establishment, "the fever increased in the huts, and that the medical officers and other attendants upon the sick were almost all seized, and several of them died." He adds, "it is a certain fact that the infection of the plague is destroyed by ventilation."

The troops employed in the lines in front of Alexandria suffered very little as compared with those in the depôt at Aboukir, where, it is worthy of notice, the chief mortality occurred among the inmates of the huts kept in strict quarantine, while the patients suffering from wounds and other casual illnesses, and who were accommodated in tents and temporary buildings about a mile off, comparatively escaped the fever. In all, the number of cases of plague in the British army amounted, according to Sir Robert Wilson's statement, to about 400, and of these 173 proved fatal, between the landing of the troops and the middle or end of August, when the hospitals were broken up.

The force that was sent from India, under the command of Sir David Baird, landed at Kossier on the Red Sea in May, 1801, and remained in the country till June of the following year. The total mortality during this period in the corps, which mustered 7886 men, of whom 3759 were Europeans, and the rest were natives of India, amounted to 700 deaths, or nearly a tenth of the strength. Of this number 165 were caused by the plague, 110 by (other forms of) 'fever,' and 195 by dysentery. Of the deaths from plague, 38 occurred in Europeans, and 127 in sepoys. Nearly all who were attacked with the disease died; there seem to have been scarcely any recoveries. The cause or causes of this excessive fatality were never satisfactorily explained; most probably, over-crowding and confinement in unsuitable hospitals were at the root of the disaster. In the French army, the death-rate from the plague seems to have been usually fifty or sixty per cent. of the attacks; in other words, rather more than half the seizures were fatal. No case of the plague had occurred during the march from Kossier to Cairo; and during the rest of the first year, the disease was confined mostly to Rosetta. Sir James MacGrigor, who was the principal medical officer of the expeditionary force from India, mentions that, on the return of the troops from Egypt in 1802, quarantines were established at Bombay, Ceylon, Madras, and Bengal to provide against the dreaded importation of the plague into India, and yet, at the very time, the disease was prevailing in various parts of Persia, particularly at Ispahan and Bagdad.

It does not appear that either the French or British fleet suffered from the plague, throughout the three years and more that their land forces were in Egypt. The like exemption has often been noticed in respect of shipping elsewhere and in other epochs.[1]

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[1] During the great plague of London, "nearly ten thousand persons betook themselves to ships and barges, moored in lines down the river, and there they lived many weeks, very safe and easy. Notwithstanding, too, the violence of the disease in the city, it was never on board the fleet, although for some time in the beginning there was a press for seamen in the river, and even in the streets, to man the fleet." (Hancock,

In 1803, the pestilence appears to have been wide-spread throughout the Turkish empire. During that year it prevailed for many months at Constantinople, and proved extremely fatal. The visitation was said to have been more severe than any that had occurred there since 1778, when, if we can believe the loose statements usually made on such matters, upwards of 150,000 persons perished in Constantinople during the months of May, June, and July. Prior to that terrible attack, the plague had been absent from the city for three or four years.

For the eight or nine years following 1803, there was a complete lull of the disease throughout the East generally, not only in Europe but also in the Levant, Egypt, and Barbary. In 1812–13, it reappeared with force in numerous and distant regions. Constantinople again experienced one of the most dreadful visitations on record; for more than 100,000 of the inhabitants in and around the city died. Three thousand deaths are said to have occurred in one day alone. Many places, too, in the Danubian Principalities, as Bucharest, Adrianople, &c., suffered heavily at the same time; and some provinces of Greece, especially Albania and the Morea, were also ravaged. There was a fatal outbreak at Odessa, on the Black Sea, in 1812. Smyrna was also visited the same year, but still more fatally in 1814, when a frightful mortality took place there, and when, too, the towns and districts of Broussa and Erzeroum, in Asia Minor, suffered severely.

In 1812–13, Syria and Egypt, and, it is believed, Tunis and other places on the coast of Barbary, were more or less virulently affected. It was in 1813 that the memorable outbreak occurred in Malta; and this was the more remarkable, as it was generally believed that the island, lying in the line of transit between Egypt and the Levant in the one direction, and Tunis and Morocco in the other, had remained quite free from the plague for considerably more than a century.[1] The disease began in May, reached its acme in July, and then gradually subsided, till it ceased almost entirely in October. It carried off 4000 persons out of a population of about 100,000, and inflicted enormous damage on the colony, which continued to experience its effects, in the way of impeded intercourse and obstructed commerce, for long afterwards.

The troops suffered but little throughout the epidemic. Solitary cases occurred in some of the regiments, the disease showing no tendency to spread. Not more than twenty in all, out of a force of between one and two thousand, died from the fever. The infected

p. 64.) The shipping in the harbour of Valetta remained unaffected, too, throughout the plague at Malta in 1813; only two or three cases occurred in seamen who had been wandering about the town.

[1] If unvisited by the plague during that period, it had repeatedly been the seat of very fatal malignant fevers. Hennen, in his most instructive work on the Topography of the Mediterranean, makes the following statement: "The winter of 1798 and the first eight months of 1799 were peculiarly unhealthy and fatal to the inhabitants of Valetta and to the French garrison. Between September, 1798, and August, 1799, no less than 2468 of the former and 555 of the latter died. In the country districts, too, the ravages of the fever were frightful; it seems to have been a low typhus. It continued to spread long after the French had evacuated the island; and it has been stated to me that, from first to last, not fewer than 20,000 lives were lost."

corps were camped out in small parties, and with marked benefit to their health. The adjacent island of Gozo was not infected till the spring of 1814.

The year 1813 was, it is worthy of notice, extremely sickly throughout many countries of Europe. While the plague was raging in Malta, a pestoid fever, we are told by Dr. Maclean, "commenced, spread, declined, and ceased, at periods precisely similar, in the Turkish province of Wallachia." The city of Bucharest suffered severely; and the pestilence is known to have existed about the same time along the shores of Albania, Lepanto, and the Morea. Malignant typhus and dysentery were epidemic in Britain, France, Holland and Germany; yellow fever was very fatal in the south of Spain; and a pernicious fever, having many of the characters of this pestilence, proved very fatal to the British army in the north of the Peninsula, as well as among the inhabitants of Murcia and other provinces of that kingdom, during the summer and autumn months. In 1814, and during the next two or three years, the plague appears to have existed with varying intensity throughout Egypt, and also in various parts of Dalmatia and Albania along the shores of the Adriatic. In 1815 the whole coast line from Albania to Spalatro in Dalmatia was infected. The district of Maccassa in the latter province must have been long the seat of the disease; for it was not in free pratique with other countries till 1817–18. When the plague, or what was called the plague, appeared in the malarious district of Leftimo in the island of Corfu, towards the end of 1815, it was conjectured to have been imported from Albania.[1] The town of Noia, on the coast of Bari, in Naples, was attacked about the same time. How it came or was brought there defied all discovery, and this, too, notwithstanding that a prize was offered by the Neapolitan Government for the purpose of stimulating inquiry. The fever followed a season of great scarcity, and its pestilential nature was not recognised for six or eight weeks after its commencement. It lingered in the district for nearly six months. While this pestoid disease prevailed on the Neapolitan coast, a very fatal fever, but without the characteristic symptoms of the plague, was raging at Cagliari in the island of Sardinia. Neither at Bari nor in Corfu, did the disease spread to any considerable extent till the following May, about which time it appeared also in Cephalonia, in the district of Comitato; while in another district of that island so deadly "an endemic fever" prevailed that, of a detachment of the 14th Regiment just arrived from England, no fewer than 140 of the men were speedily struck down, and 90 died; in some of the cases after a few hours' illness. In what respect the two fevers differed as to nature, symptoms, or causes, has never been clearly stated; in point of fatality, the endemic fever seems to have equalled the worst attack of the genuine plague.

The towns of Arta and Prevesa, on the coast of Albania, were infected with the plague in 1816; but the adjacent island of Santa Maura—intermediate, too, between Corfu and Cephalonia—appears to have escaped.

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[1] We may judge of the condition of plague hospitals in those days by the fact, mentioned by Hennen, that of twenty-eight cases, treated in the military hospital at Corfu, only three recovered. Civil hospitals were no better; very few of the many hundreds taken to the hospitals in Malta during the plague there left them alive.

The latter part of 1816 and the whole of 1817 formed a period of extraordinary suffering and sickliness throughout Europe. The harvest of 1816 had failed almost everywhere, and the poor were reduced to the utmost distress. Never were the working-classes in our own country worse off than during the winter and early spring of 1816–17. In many parts of France and Germany, things were still worse; the people, after their scanty stock of corn and potatoes was consumed, being reduced to live upon nettles, bean-stalks, and other herbage fit only for cattle. Numbers died of sheer want, while the damaged constitutions of the survivors rendered them ready victims to the epidemic fever which had been springing up in many countries during 1816, and which prevailed so extensively and fatally in the course of the following year.

In the spring and summer of 1817, the city of Naples and its neighbourhood were the seat of a pestoid fever, "which corresponded with the description usually given of the plague."

This year was memorable in India, not only for the commencement of the first great epidemic of cholera on the banks of the Ganges, but also for the prevalence of the Pali Plague in some of the northern districts of Hindostan. It seems to have reappeared in the same localities in 1821. Malignant fevers, accompanied with glandular swellings, had repeatedly been observed before in different parts of India, as in Cutch and Kattywar in 1815 and again in 1819.

From 1817 to 1819 inclusive, the chief seat of the plague appears to have been the north coast of Africa, from Egypt to Barbary, Algiers, and Morocco.[1] Constantinople suffered a good deal in 1819; the city had been nearly exempt since the dreadful visitation of 1812–13.

After 1819, little is heard of epidemic plague for the next five or six years, when it reappeared in Egypt, at Erzeroum, and also at Constantinople, as well as in Albania and the Morea. In 1824 Cairo was the seat of a severe outbreak. More than 30,000 persons are said to have perished in that city, while only a few cases occurred that year in Alexandria, although the intercommunication was uninterrupted all the while. The same year the district of Dulcigno, on the coast of Montenegro, was affected with the disease. Throughout the greater part of the war of independence in Greece, it often committed disastrous ravages in both of the belligerent armies, Greek and Turkish, more especially whenever the troops or the inhabitants were massed together and suffering the usual calamities of war. That it may have accompanied Ibrahim Pacha's troops from Egypt is more than probable;

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[1] A vessel arrived in June, 1819, at Zante from Tunis, where the plague existed at the time of her sailing. There had been no sickness during the voyage; but, there being no lazaret on shore, the crew, eight in number, were confined on board the vessel, which was in a very foul state, under strict quarantine. Within the next nine days, seven of the crew and the health guardian, who had been put on board, were attacked with a malignant pestoid fever, which proved fatal in every case.

but the disease had been present in several provinces of the country, especially in Albania, for several years before the commencement of the war.

In the Russian campaign against the Turks on the Danube, in 1828–9, the Czar's army suffered terrible losses from a malignant fever, having most of the characters of genuine plague, although doubts were entertained by some of the physicians as to its real nature, because in the milder cases the fever occasionally exhibited a remittent or intermittent type. Like uncertainties have often perplexed the diagnosis in various other pestilential diseases. Dr. Seidlitz, one of the physicians of the Russian army, made the general remark that, whenever the Russians have carried on war against the Turks on the Danube and the coast of the Black Sea, the troops have invariably suffered from a fatal fever having most of the characters of the plague, and which he considered to be but an aggravated form of the endemic fever of the country. It is known that the plague existed at Bucharest, Cronstadt, on the Wallachian border of Transylvania, and at Constantinople in 1828; and that Odessa, on the Black Sea, was attacked in 1829. Several parts of Persia, also, seem to have been affected about this time. In 1832, Bussorah and Bagdad suffered severely; and that year, too, the disease raged in many of the towns, including Mecca, in the Arabian Gulf.

In 1834–5, there was throughout the East generally a recrudescence of epidemic plague, which for two or three years afterwards continued to prevail, with varying severity, in almost every province of Turkey in Europe. Cairo sustained a severe invasion in the spring of 1835; it was most fatal in the month of April; for several days, upwards of 1000 of the inhabitants were carried off by the pestilence. Constantinople suffered in 1834, and still more severely in 1836, when from 1200 to 1500 deaths occurred in one day, during the height of the sickness. In this year, the fever broke out in Bulgaria, where it continued to linger till 1838–9, destroying between eighty and ninety thousand of the population. Sophia, the capital city, lost more than a third of its inhabitants, and several other towns in European Turkey were similarly depopulated. The ravages of the disease were, doubtless, greatly aggravated by the means that were used to arrest its spread. Military cordons were established to prevent the ingress or egress of the inhabitants; and, as the deaths multiplied, the eager desire of the people to escape increased to such a degree that nothing could withstand the rush of thousands, who preferred dying by the bullets of the soldiery to remaining shut up within the walls of an infected town.

During 1836–7, the towns of Rustchuck, Silistria, Ibraila, Shumla, and Galatz, situated on or near to the course of the Danube, all suffered more or less severely. The disease spread into Servia, Macedonia, and Greece, but without advancing farther westward to the provinces of Herzegovina, Dalmatia, or Albania. Sporadic cases had occurred at Salonica in 1836; but the violent outbreak of the epidemic in that city was in the following year, when the disease visited the neighbourhood of Mount Athos (twenty of the monks in one of the convents died of it), and the town of Cassandra, intermediate between the Monte Santo and Salonica. The mortality in Salonica was estimated at 9500 deaths, of which upwards of 4000 occurred among the Jewish population of about 20,000 souls, living in the most filthy and unhealthy part of the town.

In the same year, 1837, the isle of Poros, at the entrance of the Gulf of Athens, was attacked; the disease having been brought, it was believed, by a vessel from the coast of Macedonia. Between the 17th of April, the date of the first seizure, and the 23rd of June, when the fever ceased, 150 deaths occurred in a population of 3316. Of the persons attacked, only twenty recovered. For many months after the cessation of the disease, the island was subjected to a most strict quarantine in all the ports of the Mediterranean.

The disease prevailed epidemically in Smyrna, in 1836–37. Whether other towns in Asia Minor were infected at the same time, we have no means of determining.

In Egypt there had been but little sporadic plague, and no epidemic, between 1824 and 1834, in which latter year it began to reappear. The great Egyptian epidemic of 1835 will ever be memorable from the researches then made on the spot by the enlightened medical men, French, Italian, and English, resident in the country, and from the highly important results, scientific and practical, to which these researches eventually led. In 1837, there was a renewal of the disease, but in a much less severe degree, in Alexandria and some other places on the Nile. In that year, it prevailed also extensively in Asia Minor, Syria, and Barbary. The towns of Adana and Tarsus, Smyrna, Aleppo, Jaffa, and Jerusalem, Tripoli, and other places on the North African coast, were all more or less severely visited.

From 1837 to 1839, a malignant bubonic fever, known as the Pali Plague, prevailed extensively and fatally in different districts of India.

After 1838–39, the remarkable subsidence, followed ere long by the complete cessation, of the pestilence throughout Turkey in Europe, began to take place. No case of genuine plague has been seen in the city of Constantinople since 1839;[1] and, since 1840–41, no instance of the disease has been recognised in any of the European provinces of the Porte, nor in the neighbouring provinces of the Russian and Austrian empires; although not a season passes without much loss of life from typhus and other bad forms of endemic fever. The plague continued to linger in several parts of Asia Minor, as at Erzeroum, &c., and also in Syria, till 1843; and for a year or so longer in Egypt.

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[1] In June, 1841, a vessel which had lost eighteen persons from the plague during the voyage from Alexandria, arrived at Constantinople. A guardian who was put on board was attacked a few days afterwards, and died in the lazaret. A porter who had landed some of the cargo sickened a few days later, but he recovered. The attendants upon these two cases do not appear to have suffered; but two of the children of the purveyor of the lazaret were attacked, the one a fortnight and the other three weeks after the porter, and both died. How the disease originated in these children is not stated. A monk who had come from Syria, and was detained in the lazaret at the time, showed some symptoms of the plague in his convent at Pera about the end of July. No other inmate of the convent was affected.

The last epidemic invasion in Syria and Palestine, and in Egypt, occurred in 1841. That year our fleet was engaged in hostile operations along the coast of Syria, against the Viceroy of Egypt, and a small military force was landed at Acre; but, in consequence of plague being in the town, it was afterwards encamped for some time on Mount Carmel, and eventually removed to the neighbourhood of Beyrout.

There had been a great deal of fever in the country generally since 1840, and the disease still existed in various parts, causing much sickness and loss of life among the Turkish and Egyptian troops, as well as among the inhabitants of the infected districts, which were not confined to the neighbourhood of the coast, but were, in some instances, on the high lands of the Lebanon at an elevation of 3000 feet above the level of the sea. The British detachment suffered severely from the climate, and many men died, but chiefly from dysentery and remittent fever. The only cases of genuine plague among our forces occurred among the crew of H.M.S. *Zebra*, which was driven ashore in December, 1840, near Caifa in the Bay of Acre. For the first five or six weeks, while camped on the sea-shore, the crew remained healthy; but upon their being quartered in the dirty Turkish town, cases of fever soon began to appear, and one was speedily fatal. Nine or ten other cases occurred within the next few days; and as the *Castor* frigate, had meanwhile arrived to assist in getting off the stranded ship, or in breaking her up, all the sick, and the rest of the *Zebra's* company on shore, were taken on board, and she sailed for Malta. It was only then that the true nature of the disease was suspected, in consequence of two of the patients complaining of swellings in the groin and armpits. On examination, it was found that the majority of the sick were similarly affected. Two fresh cases occurred on board; in all, thirteen cases of "unquestionable plague" occurred, and out of this number nine proved fatal during the voyage of twelve or thirteen days, between the 22nd of February and the 9th of March, when she arrived at Malta. One only of the *Castor's* crew was attacked, and he had been quartered on shore with some of the *Zebra's* men. The disease, however, showed no tendency to spread on board; nor were the medical officers or any of the attendants on the sick at all affected, although none but the usual precautions in ordinary fever cases were resorted to.[1]

The entire cessation of the plague, even in its sporadic form, throughout the whole of the East since 1843-44, however remarkable the event may be considered, has been verified by repeated and most accurate inquiries. The attention of the International Conference, held in Paris in 1851, was specially directed to ascertain the facts of the case. In 1846, the Russian Government had sent commissioners to Egypt for this very purpose; and in 1849, the Austrian Government did the same. On both occasions, the gentlemen employed in conducting the inquiry failed to discover a single case of genuine plague in any part of the country. Still more important was the evidence, and to the same effect, of the resident physicians whom France, with such enlightened policy, appointed in 1847 as sanitary inspectors in six of the principal towns in Turkey and Egypt, with the view of obtaining exact information as to the health state of these countries, and of aiding with their counsel the carrying out hygienic improvements in the districts of which they had the supervision. Dr. Bartoletti, who was employed by the Turkish Government to report upon the state of the Asiatic provinces, and was well acquainted with every part of the Ottoman empire, gave like testimony.

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[1] This was not the first time that cases of plague had occurred in our ships of war. During the siege of Acre in 1798-99, five cases were treated on board the *Theseus*, and without any ill consequences to the attendants, although no unusual precautions were taken to prevent its spreading.

Since these statements were made in 1851, a like condition of the public health in the countries of the East, as regards the entire absence of the plague as an endemic or epidemic disease among their populations, continued for the next seven or eight years; and then its reappearance was only in a very partial and circumscribed extent among the famishing and squalid inhabitants of an Arab village in the *district* of *Bengazi* (*the ancient Cyrenais and Pentapolis*, celebrated for its richness and fertility), on the African coast, between Alexandria and Tripoli. The circumstances were these:—After three or four years of unusual drought, the people were reduced to a state of famine in 1857. Great numbers of the poor Arabs perished from sheer want, both in the country and in Bengazi, to which they had gone in quest of food. An epizootic disease had also prevailed among the cattle. The utmost misery continued up to the time when a malignant fever appeared about the middle of April, 1858, in an Arab encampment, about eight hours' distance from the *town* of *Bengazi* (*the ancient Berenice*). The disease manifested itself in the town at the beginning of May, but did not spread much till June, during which month it caused great mortality. By the middle of July between 600 and 800 deaths occurred among an estimated population of about 10,000 souls, of whom, however, more than half had fled in terror to other places. Out of the garrison, about 200 in strength, 30 fell victims. It was impossible to ascertain the mortality among the Bedouin tribes. The disease seeming to have ceased in August or September. There was a slight reappearance of the fever in the spring of 1859, but it soon passed away; and since that time we have not heard of any return of the distemper, which not only caused much alarm in the affected district, but affrighted the quarantine authorities throughout the whole of the Mediterranean. For a considerable time after the first breaking out of the fever, it was regarded as typhus; and it was not till two or three months later that its real pestoid character was recognised. There can be no reasonable doubt but that it had sprung up spontaneously, under the favouring influences of squalor and prolonged destitution and misery, as appears to have been also the case in the great plague of the province of Erzeroum in 1841, as well as on various former occasions both there and elsewhere.

With this one exception, occurring in a limited district on the northern coast of Africa, the pestilence, which for centuries was endemic throughout the regions of the East, seems to have been now entirely absent from its former *habitats* for the last twenty years or thereabouts. The fact is a curious, but not a singular one in the history of epidemic diseases; nor does this prolonged subsidence warrant the expectation of any permanent extinction of the Oriental plague in the Ottoman dominions, unless indeed it be accompanied with a substantial improvement in the sanitary and hygienic condition of the people, and then its place may be taken by other less malignant form of continued and periodic fever.

I propose to deal with yellow fever as I have now attempted to do with the plague.

Art. II.

Historical Sketch of the Progress of the Anatomy and Physiology of the Nervous Centres, from the time of Hippocrates to the time of Willis.[1]

By J. Lockhart Clarke, F.R.S., &c.

The earliest cultivators of anatomy divided the cerebro-spinal nervous system into three great parts: the *anterior* brain (εγκέφαλος); the *posterior* brain (παρεγκέφαλὶς, ἐπικρανὶς); and the spinal or dorsal cord (μυελὸς ῥαχίτης, νωτιᾶιος). Of the structure and functions of these centres, however, they were almost wholly ignorant. Hippocrates, it is true, considered the brain to be the organ of mind, or the seat of the rational soul; but on account of its colour and consistence, he classed it with the glandular structures which are destined to eliminate the superfluous fluids of the body.[2] It is questionable whether Hippocrates ever actually dissected the human body, notwithstanding the statements of Riolanus[3] and Haller.[4]

In the treatise '*De Corde*,' the mind or intelligence (γνώμη) is said to belong to the left ventricle of the heart, and to govern the rest of the soul; but there is every reason to believe that this is not a genuine production of Hippocrates, since it is not mentioned by either Galen or Erotianus; and in other works, which are known to be genuine, the opinion just stated is strongly opposed, as in '*De Morbo Sacro*,' where he says, "ἡ καρδίη αἰσθανεταί τε μάλιστα καὶ αἱ φρένες, τῆς μέντοι φρονήσιος οὐδετέρῳ μέτεστιν. ἀλλὰ πάντων τουτέων ὁ ἐγκέφαλος αἴτιός ἐστίν," and again, "ἐς δὲ τὴν σύνεσιν, ὁ ἐγκέφαλος ἐστὶν ὁ διαγέλλων,"—*the instrument by which intelligence is rendered manifest, or announced;* for he believed that the respired air supplied the intelligent principle, leaving in the brain its own force or vitality (ἀκμὴ), and whatever understanding or mind it possessed, καταλιπὼν ἐν τῳ ἐγκέφαλῳ

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[1] This Sketch is founded on a careful perusal of all the obtainable works to which it refers; and while it presents and compares the facts in their true historical order, and attempts to exhibit the scientific tendency and methods of investigation which prevailed in different ages, it will be found to contain—especially with regard to the nerves—information which is not to be met with in any histories of the subject; and to correct certain errors into which the authors of those histories have inadvertently fallen.

[2] De Glandulis.

[3] Anthropographia, lib. i.

[4] Opera minora, tom. iii. p. 268.

εωϋτοῦ τὴν ἀκμὴν, καὶ ὅτι ἂν ἔῃ φρόνιμον τε καὶ γνώμην ἔχον. . . . . διότι φημὶ τὸν ἐγκέφαλον εἶναι τὸν ἑρμηνεύοντα τὴν ξύνεσιν, Wherefore I consider the brain to be the expositor or interpreter of intelligence.[1]

The opinions of Aristotle on the same subject, in a sciential point of view, are not worthy of notice.[2] It is interesting, however, to observe that he considered the spinal column as the origin or foundation of the bones of the skeleton, *τὰ δ' ὀστᾶ τοῖς ζῴοις ἀφ' ἑνὸς πάντα συνηρτημένα ἐστὶ καὶ συνεχῆ ἀλλήλοις ὥσπερ αἱ φλέβες. ἀρχὴ δὲ ἡ ῥάχις ἐστὶν ἐν πᾶσι τυῖς ἔχουσιν ὀστᾶ*, &c.,[3]—a doctrine developed in modern times by Oken, Duméril, Carus, Owen, and others. But of the nerves, as distinct from other fibrous structures, he appears to have been entirely ignorant; and it is therefore an error to state, as is sometimes done, that he believed them to originate from the heart. In those days anatomists made but little or no distinction between the structure of tissues that resemble each other in external appearance; so that the term *νεύρον*, which at a later period was applied only to nerves, was often employed by them in a more general sense, to designate other *fibrous* tissues, such as tendons, ligaments, and even arteries and veins. This want of distinction is frequently met with in Hippocrates, who confounds even the optic nerves with veins (De Locis in Homine, and De Carn.), and often assigns the function of the one tissue to the other. That Aristotle employed the term *νεύρον* to signify fibrous structures in general is evident from his descriptions in 'Histor. Animal.' lib. iii. In asserting, therefore, as he does, that the *νεύρα* arise from the heart, he meant simply that this is the origin of fibrous and ligamentous tissue, which he supposed to be an extension of the cordæ tendineæ of the left ventricle; and when he speaks of the aorta as a nervous vein, *νευρώδης φλεψ*, he meant that it is a vein containing, or composed of, *fibrous* tissue. The later Greek anatomists obtained a clearer knowledge of the specialities of structure, and called the *nerves only*, *νεύρα*, the *tendons*, *τένοντες*, and the *ligaments*, *σύνδεσμοι*.

Passing by the immediate successors of these great men—Diocles, Praxagoras, Plistonicus, Philotemus, Chrysippus, and others, whose knowledge of the nervous system was but little superior to theirs—we come to Eudemus, Erasistratus, and Herophilus, who are mentioned by Galen as having made great advances in this department of anatomy, and who were probably the first to draw their information from dissection of the human body.[4] They appear also to have been the first who held any settled or correct opinions on the functions of the nervous system. Erasistratus, a pupil of Chrysippus, taught that the brain is the seat of sensation, the source of motion, and the organ of mind; that in man the number and variety of its convolutions are the cause of his superior understanding; and that it gives origin to two kinds of nerves, sensory and motor. According to Rufus Ephesius, Herophilus, a pupil of Praxagoras, and contemporary with Erasistratus, described three kinds of nerves: the first were for sensation and voluntary motion, and proceeded partly from the brain, and in part from the spinal cord: the second had their origin in the bones of one part, and their termination in those of another: and the third set had a similar connexion with the muscles. From the two last descriptions, it is evident that even Herophilus had not learnt clearly to distinguish *nervous* from ordinary *fibrous* tissue. It was from this anatomist that the retina, the choroïd and arachnoïd membranes, and the calamus scriptorius received their names.

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[1] De Morbo Sacro.

[2] De Partibus Animalium, lib. ii. (p. 30, Bekk.) But still, as an anatomist, he is honourably mentioned by both Galen and much later writers. His accuracy as a zoologist is particularly shown by Dr. Kidd, in his Bridgewater Treatise, p. 298, and Appendix.

[3] Hist. Animal., lib. iii. p. 65, Bekk.

[4] Fragments only of their works have been preserved, chiefly by Galen. For the first description of the human brain on record, which was given by Erasistratus, see Galen 'De Hippocratis et Platonis Decret.' lib. vii. cap. 3; and Leclerc ('Histoire de la Médecine,' 2e Partie, liv. i. chap. 3), who has translated it.

Soon after the time of Herophilus, anatomy fell into neglect. The sect of Empirics—the followers of Serapion and Philinus—affected to despise it as useless, and relied on experience and observation alone, in the treatment of disease; nor was the study of it resumed with any regularity, or cultivated with much advantage, until the first century of the Christian era, when it received a new impulse from Marinus, Quintus, and Numisianus, but chiefly, a little later, from Galen, who, moreover, preserved from oblivion the opinions and scattered fragments of these and other early writers. This great man flourished during the middle and latter parts of the second century, and, of all the ancients, was the first to leave us complete treatises on anatomy and physiology.[1] As he may be said chiefly to have laid the foundations of these sciences, to have greatly influenced their development, and supplied the most important materials contained in the works which appeared during many succeeding generations on these subjects, I shall give, from my own examination of his writings, the briefest possible account of his opinions on the brain and nerves.[2]

That Galen applied himself with great energy and perseverance to the pursuit of practical anatomy, must be acknowledged by all who are familiar with his writings. In these he describes, often with great accuracy, the origin and distribution of the nerves, as well as the most important parts of the brain:—the *corpus callosum*, *fornix*, and *septum lucidum;* the *infundibulum*, *pituitary body*, and *thalami optici;* the *nates* and *testes*, *conarium* and *velum interpositum;* the *ventricles*, *aqueduct of Sylvius*, and the *vermiform process* of the cerebellum.[3]

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[1] 'De Usu Partium,' and 'De Administrationibus Anatomicis.' The first is a treatise on anatomy and physiology; the second, a dissector's manual. Vesalius, who was so much indebted to these works, and himself sometimes the author of gross errors, was unjustly severe on those of Galen, as is acknowledged by his own devoted pupil, the admirable Fallopius ('Observationes Anatomicæ,' p. 8). It is questioned by some whether even Galen dissected the human body: on this subject see Leclerc, Eustachius (Ossium Examen), and especially Lauth (Histoire de l'Anatomie).

[2] Portal justly observes, "Combien de traités élèmentaires ont été publiés depuis, et surtout de nos jours (1770), qui sont inférieurs aux écrits de Galien: il n'y aurait qu'à les mettre en parallèle pour faire une critique ignominieuse pour les auteurs de ces nouveaux livres, et humiliante pour l'esprit humain."—*Histoire d'Anatomie et de la Chirurgie. Preface.*

[3] De Usu Part., lib. viii., and De Anatom. Administrat., lib. ix.

Like other early writers, however, Galen dogmatically assigns a use to every part that he describes.[1] According to him, the cerebral convolutions are not, as correctly stated by Erasistratus, subservient to the mental faculties, but formed to admit the *pia-mater* to the deeper parts. The *anterior* brain is softer than the *posterior* because it is the seat of sensation and mind, for which functions that which is soft—being more easily affected—is best adapted. For the same reason he divides the nerves into two kinds—the *soft* nerves, or those of special sense, arising from the base of the *anterior brain;* and the hard nerves, nearly all contained in the spinal cord, but having their real origin from the *posterior* brain (παρεγκέφαλις), and serving as instruments indifferently of motion and common sensation.[2] The nerves are harder just in proportion as their origin is distant from the *anterior* brain. But a soft or sensory nerve may become hard and dry, or motor, by pursuing a lengthened course, or by passing through hard or dry substances, as in the case of the *portio dura* or facial nerve. The nerves are composed of three kinds of tissue: a central portion—which he compares to the pith of trees—continuous with the substance of the brain; and two coats derived, the one from the pia- and the other from the dura-mater.[3]

The ventricles are four in number: two anterior, one middle, and one posterior. Before they were altered in shape by the surrounding parts, they were round or spherical, because that which is round is most capacious, and most like itself.[4] The anterior ventricles have three important offices. 1. They collect the excrementitious fluids of the brain, and discharge them in two ways—the heavier fluids below, through the nose, palate, and infundibulum; the light vapours above, through the sutures and foramina of the skull.[5] 2. They form the

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[1] In the early ages of philosophy, before the human mind had clearly apprehended the most successful method of investigating the phenomena of nature, and been sobered down or directed by *habits* of observation and induction, the extent to which, as might be expected, speculation was sometimes carried by energetic minds in matters of science, and the readiness with which the most gratuitous conclusions were accepted, afford an amusing, and indeed, in a psychological point of view, an instructive topic for contemplation. As an example bearing on the present subject, may be mentioned Plato's *serious*, and, for that very reason, *ludicrous*, description of the creation of man by the junior gods; as well as his account of the formation, position, and functions of the different organs of the body, and their final causes or uses.—(See Plato's *Timæus*).

[2] It is usually stated that Galen derived the hard or spinal nerves from the cerebellum, which the term παρεγκέφαλις is understood to signify, and which is the meaning attached to it by his Latin translators; but by taking the general sense of the word in various parts of the Greek text, we shall find that it comprehends the pons Varolii and fourth ventricle, which lie at the base of the cerebellum, and with which alone, according to Galen, the dorsal or spinal cord is continuous,—τοῖς κατὰ βάσιν μόνην εγκέφαλου μέρεσι συνεχης ἐστὶν ὁ νωτιαῖος. See De Usu Part., cap. 6 and 8. This may appear a matter of no great importance, but if the history of science is of any value at all, it can be valuable only so far as it records the truth.

[3] De Hippoc. et Platon. Decret., lib. vii. cap. 3.

[4] Πάντη δὲ γὰρ ομοιότατον ἑαυτῷ τὸ κυκλοτερές ἐστὶ. This reminds one of Aristotle's "perfection of circular motion," and other physical dogmas.

[5] For sixteen centuries after Galen this opinion prevailed, and was the occasion of the most absurd speculations on the functions of the infundibulum and pituitary body. The need of the brain's excretory function is thus curiously explained by Galen:—

animal spirits from the *vital spirits* contained in the blood of the choroïd plexus, and generated in the heart from the inspired air. 3. They contain the central organs of smell. For the two last reasons the brain is subject to peculiar motions of dilatation and contraction for the inhalation and expulsion of the atmospheric air. The animal spirits leave the anterior ventricles through the aqueduct of Sylvius, which is closed or opened, more or less, by the vermiform process, to regulate their passage to the posterior or fourth ventricle, where they receive their final elaboration, and become ready for distribution through the nerves of the body.

It was Galen who discovered and named the sympathetic ganglia, which he described in three places—the neck, thorax, and abdomen.[1]

Of the cerebral nerves, he described seven pairs, and classified them as follows:—1, Optic; 2, motores oculorum; 3, gustatory (comprising all the branches of the trifacial, except the spheno-palatine); 4, spheno-palatine branches of the second division of the trifacial; 5, auditory and facial; 6, vagus, spinal-accessory, glosso-pharyngeal, and sympathetic; 7, hypoglossal.[2]

He did not include the olfactive tubercles amongst the cerebral nerves, because they have no processes or branches (ἐκφύσεις) like the rest, and do not project beyond the bones; but he considered them subservient to the sense of smell, and called them productions and extensions of the lateral ventricles, destined for the nose, ἐπὶ τὰς ῥῖνας ἀποφύσεις, (lib. viii. cap. 6.)[3] Of the first division of the trifacial (his third) Galen described the frontal and nasal branches; of the second division—the orbital, infra-orbital, and dental branches; of the third division—the *buccinator*, *gustatory*, and *inferior* dental nerves. He particularly states that the tongue derives its sensibility and faculty of taste from the gustatory, which is distributed to its mucous membrane; while the hypoglossal supplies it with motion, and is distributed only to its muscles. Of our seventh pair (his fifth) he distinguished the *portio mollis* from the *portio dura*, which he followed through the temporal bone to the parotid gland, the muscles of the temples and cheeks, (lib. ix.) Of the *vagus*, which formed part of his sixth, he described with considerable accuracy the different course on each side, and was the first to discover the *recurrent* branch and its distribution to the muscles of the larynx; but he confounded the *lateral cord* of the *sympathetic* with the *vagus*, of which he considered it a branch.

For many centuries after the death of Galen, the pursuit of practical anatomy once more fell into neglect. The Arabians, at the taking of Alexandria, in A.D. 640, while destroying its famous library, spared

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"Υπέρκειται γὰρ ἁπάντων τῶν μερῶν ἐν τῷ σώματι ἡ κεφαλὴ, καθάπερ τις ὄροφος οἴκου θερμοῦ. παντ' οὖν ὅσα λιγνυώδη τε καὶ ἀτμώδη περιττώματα τῶν ὑποκειμένων ἀναθέει, δεχομένη ταῦτα δαψιλεστέρας χρῄζει τῆς κενώσεως." De Usu Part., lib. ix. cap. 1.

[1] De Usu Part., lib. xvi. cap. v.

[2] Ibid., lib. ix.

[3] Theophilus, a Greek anatomist of the ninth century and a Christian, was the first to include the olfactory processes amongst the cerebral nerves. De Corp. Hum. Fab., lib. iv. cap. xii.

and secured the works of the Greek anatomists, which they translated, but disfigured, and followed implicitly, without adding anything of importance to their stock of knowledge.[1] Nor was it till the commencement of the fifteenth century that this, like the other sciences, was cultivated with renewed energy. Mundinus, a native of *Milan*, was the chief restorer of practical anatomy in Europe. His work, published in 1478, excited the interest of Gabriel de Zerbis, Achillinus, Carpus de Berenger, and Charles Etienne. But even these anatomists made but slight advances in the anatomy and physiology of the nervous system, and were too often content with but little more than a brief and simple exposition of the opinions of Galen; or were more remarkable for their absurd speculations on the functions of particular parts of the brain, than for any real contribution of knowledge. Thus Mundinus, whose work was for a long time a text-book in the schools of Italy, sums up his whole description of the lateral ventricles and their contents, by stating that in their anterior angles is lodged the *phantasy*, which retains the species of things; in the posterior angles, the *imagination*—"quæ apprehensiva est harum specierum in fantasia retentarum. In medio vero est *sensus communis*." These opinions, authoritatively announced, without any grounds for their adoption, were repeated as established facts by the anatomists who succeeded him, during several centuries.

But a much more accurate and particular account of the brain was soon after given by Vesalius and Vidus Vidius, who retained, however, the physiological doctrines of Galen. Eustachius, also, about the same time represented in his plates, the *corpora pyramidalia* and *olivaria*, which had not hitherto been pointed out. Not long after, Arantius described the *hippocampus*,[2] and gave a particular account of the fourth ventricle and "cisterna," or cavity in the cerebellum;[3] in which account, however, he was preceded by Vesalius.[4] Varolius had also published an admirable exposition of certain parts of the encephalon—of the *pons* which bears his name; of the dimensions and form of the three lobes of the cerebrum and the lateral ventricles; of the optic nerves and the olfactory lobes.[5] He believed the cerebrum to be the seat of the sense of vision; and that the cerebellum being, according to the doctrines of Galen, *drier* and *harder*, is *therefore* the origin of the auditory nerves, and subservient to the sense of hearing.[6]

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[1] Unless Avicenna be considered as an exception. It is known that the physical sciences met with the same reception from the Arabians. "The great obligation which science owes to the Arabians, is to have preserved it during a period of darkness and desolation, so that Europe might receive it back again when the evil days were past." Whewell's History of the Inductive Sciences, vol. i. p. 244. See Book iv. of the same vol., On the Intellectual Character of the Middle Ages.

[2] Anatomicæ Observationes, cap. iii. (1595, Bon.)

[3] Ibid., cap. vii.

[4] De Hum. Corp. Fab., lib. vii.

[5] Anatomia (Francof. 1573, 8vo), and De Nervis Opticis Epistolæ (Patav. 1573), which are well worth an attentive perusal. Galen discovered the origin of the optic nerves from the optic thalamus, which he first described: *ἔνθα χὰρ ἑκάτερον τῶν προσθίων κοιλιῶν τελευτᾷ πρὸς τὰ πλάγια, τῶν ὀπτικῶν νεύρων ἐστὶν ἡ ἔκφυσις, καὶ αὕτη γε τῶν κοιλιῶν ἡ οἷον θαλάμη δι' ἐκεῖνα τὰ νεῦρα γέγονεν.* De Usu Part., lib. xvi. cap. iii.

[6] His reasoning is curious (see his Anat., pp. 7, 8). Willis, also, assigned to the

Thus, while anatomy was making considerable advances, the progress of physiology was encumbered and impeded by a thoughtless adherence to the groundless dogmas of the ancients, which with occasional modifications were taught even as late as the eighteenth century. Piccolhomini, thirteen centuries after Galen (1586), assigned to the *pineal gland* the valvular office which was assigned by Galen to the vermiform process: "Glandulæ verò pinealis—usus est, ut claudat foramen in dyastole cerebri, ne, quam spiritus animalis è tertio in quartum ventriculum fuerit ingressus, iterum in tertium regredi possit."[1] Such were the absurd conclusions resulting from implicit faith in the existence and requirements of this hypothetical nerve-fluid, which was invented by the old Greek anatomists as a medium to explain the connexions between the body and mind. This subtile, ethereal fluid was, as we have seen, said to be formed from the atmospheric air, which the ancient philosophers considered as the vehicle of a spiritus or *πνεῦμα*, or pure heat, *θερμὸν*, by which all the operations of nature were carried on, and to which, as the *πῦρ νοερὸν*, or *πῦρ ἀείζωον*—*the intellectual or everlasting fire*—the Chaldeans first, and afterwards Heraclitus, Zeno, and the Stoics, attributed the origin and intelligent government of all things. It has been already shown, as the opinion of Hippocrates, that in the brain, as its instrument, the active principle (*ἀκμὴ*) of the air, is manifested as intelligence and mind, while in other organs of the body it was supposed to preside over the organic functions. But at a later period, by Erasistratus and Galen, the *πνεῦμα ζώτικον*, or vital spirit, was supposed to be generated in the heart, and converted by the brain into the *πνεῦμα ψυχικὸν*, or animal spirit, which was only the *instrument* employed by the mind in sensation and voluntary motion—*πρῶτον ὄργανον ὑπάρχων τῃ ψυχῃ πρὸς τὸ διαπέμπειν ἐις ἅπαντα τὰ μέρη τοῦ σώματος αἴσθησιν τε καὶ κίνησιν.*[2] Sanctioned by the authority of great names, the doctrine of a nerve-fluid, for many subsequent generations, exercised, as we have seen, a powerful but pernicious influence over the minds of physiologists, and served to explain, to their own satisfaction, a variety of effects both in health and disease. Treatises were written on its nature, composition, and uses. In process of time it was made, like the blood, to enjoy a complete circulation (Rolfincius, Deusingius, Diemerbroeck); and the muscular structure which Pacchioni and Baglivi professed to have discovered in the dura-mater, provided it with a heart,—the *cor cerebri.*[3]

To Piccolhomini we are indebted for many excellent observations on the limits of the grey and white substances of the brain, and these dis-

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cerebellum the office of retaining, or remembering, musical sounds (Cerebri Anat., cap. xvii.); and recently, Oken held both the opinions of Varolius; he calls the cerebrum the "optic brain," and the cerebellum, the "auditory brain." (Physio-philosophy, translated by Tulk, pp. 418—20.)

[1] Anatomicæ Prelectiones, lect. iii. p. 259. The well-known opinion of Descartes, that the *pineal gland* is the habitation of the soul, was founded on the fact of its beings *ingle* and *central.* Descartes, De Passionibus, Art. 31-32.

[2] Galen, De locis affectis, lib. iv. cap. iii.

[3] Baglivi, De Fibra Motrice, lib. i. cap. v.

tinctions have been adopted by all his successors. With this exception, however, nothing of importance was discovered in the nervous system until the time of Willis, whose work on the anatomy of the brain, enriched by the labours of Lower, and illustrated by the pencil of the famous Wren, marked a new era in the history of this department of our science. By Vieussens[1] the discoveries of Willis were extended and improved, and the task was begun of exploring the fibrous structure and connexions of different parts of the encephalon.

Upon the origin and classification of the cerebral nerves, however, much labour was bestowed by some of the anatomists of the sixteenth and seventeenth centuries; and the slow and gradual manner in which, as we shall see, the arrangement now adopted emerged, is sufficient to indicate the difficulty of the task in which they were engaged. The majority of anatomists down to the time of Willis, either adopted, or very nearly followed, the classification and descriptions of Galen; while those —with only three or four exceptions—who differed from him, were his inferiors in both. Mundinus (1478) in his arrangement of the cerebral nerves implicitly followed Galen, but his descriptions were most superficial, not including even their origins. Like the Greek anatomist, he confounded the *sympathetic* with the *vagus*, but had the merit of first noticing the connexion of the former with the trifacial (his third pair), "quorum aliqui (rami) vadunt ad membra faciei, et unus alius descendit inferius ad loca diafragmatis et dat sensum membris inferioribus, sc. visceribus, et isti etiam veniunt ad stomachum." He describes the hypoglossal as both a sensitive and motor nerve of the tongue. Achillinus (1510) followed a similar course, and connected the sympathetic and vagus (confounded together as one nerve) with his fourth, which, like that of Galen, appears to have been a branch of the trifacial: "Quartum par," he observes, "dat sensum diafragmati, visceribus, et stomacho."[2] It is to Achillinus that Haller gave the credit of having first discovered this origin of the sympathetic; while neither by this great and learned physiologist, nor his successors, has any reference been made to the previous description which I have above quoted from Mundinus.

By Carpus de Berenger (1522) the cerebral nerves were described nearly in the words of Mundinus. Speaking of the trifacial (his third nerve), he adds, "Intra et infra os basilare unitur cum sexto pari (the vagus and sympathetic) et simul faciunt nervos descendentes ad membra ventris medii et inferioris; et ab istis oriuntur reversivi" (the recurrent laryngeal).[3] The descriptions of Gabriel de Zerbis and Carolus Stephanus (Charles Etienne, 1536), were like those of Galen. Zerbis, however, and Massa speak of the *olfactive nerves*. The great Vesalius, who effected so much in other departments of anatomy and gave a far superior account of the brain, was actually inferior to Galen in his descriptions of the cerebral nerves, and fell into many grievous errors. His third pair arose by a large and a small root: the former belonged to the second and third divisions of the trifacial; the latter to its ophthalmic division and part of the first or superior maxillary; but at its origin, he evidently confounded this his small root with our *pathetic* or fourth nerve. His fifth pair, also (our seventh), had a large and a small root. The former belonged to the *auditory* and *facial;* the latter to the sixth or abducens. Like his predecessors, he confounded together the *par vagum*, *glosso-pharyngeal*, *spinal-accessory*, and *sympathetic.*[1] Columbus, one of his pupils, described nine pairs of cerebral nerves, but committed equal blunders in regard to the trifacial, out of which he made his third and fourth pairs. His ninth nerves were our fourth, which he takes the credit of having discovered.[2]

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[1] Neurographia Universalis (1684).

[2] Anatomia, p. 12.

[3] Isagoge Breves, p. 55.

Another pupil of Vesalius, the admirable Fallopius, bestowed great labour on this subject, and gave far better descriptions of the cerebral nerves than any other anatomist previous to the time of Willis. He was the first to reduce all the branches of the *trifacial* to *one* root, and traced their distribution with considerable accuracy. He also showed the *separate* origin of the fourth or *pathetic*, of which he considered himself the discoverer;[3] but it would appear that Columbus, Fallopius, and Eustachius (Ossium Examen, &c.), each discovered it about the same time, and independently of the others. The dissections which Fallopius made of the *auditory*, and particularly of the *facial* nerve, were excellent. He was the first to give a *separate* account of the *glosso-pharyngeal*, which he called the small root of his sixth (our eighth) pair; and although he considered it, together with the *spinal-accessory* and *sympathetic*, as a portion of the vagus, he gave a superior description of all. Eustachius, a cotemporary of Fallopius, and a consummate anatomist, appears (from his excellent plates) to have had an almost perfect knowledge of the origins of the cerebral nerves, although we have no means of judging of his classification and descriptions; for the plates were lost during more than a century and a half, and were accidentally found without explanations or figures. In the eighteenth plate the sympathetic is clearly represented as distinct from the eighth pair, and in connexion with the sixth (abducens) cerebral nerve. No previous anatomist had made this distinction, although the sympathetic, as a division of the *vagus*, had been shown, as I have already stated, first by Mundinus, and afterwards by Achillinus and Carpus, to be connected with the superior maxillary branch of the *trifacial.* It is an error to say with Portal,[4] that Charles Etienne first made the distinction in question; for the intercostal nerves of Charles Etienne are evidently the *spinal* intercostal, and not the *sympathetic*, which was only *subsequently* known as the "intercostal nerve."[5]

The immediate successors of Fallopius profited but little by the labour which that great anatomist had bestowed on the cerebral nerves. Laurentius (Dulaurens)[6] and Riolanus[7] described only seven pairs, and in a manner inferior even to that of Galen, again making *two* distinct

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[1] De Corp. Humani Fabricâ, lib. iv. and fig. 1 (1543).

[2] De Rê Anatomicâ, lib. viii. cap. i.

[3] Observationes Anatomicæ.

[4] Histoire de l'Anat., &c., tom. vi. p. 386, and tom. ii. p. 633.

[5] Carolus Stephanus, De Dissect. Part. Corp. Hum., lib. ii. cap. 49.

[6] Historia Anatomica, lib. iv. cap. xvi. (1600).

[7] Anthropographia, lib. iv. (1618).

pairs of nerves out of different branches of the *trifacial;* as did also Caspar Bauhinus,[1] Adrian Spigelius[2] Johannis Veslingius,[3] and even Gaspar and Tho. Bartholinus.[4] Veslingius joined our sixth or *abducens* with the third or *motor oculi,* to form his second nerve. Of his third nerve, one root was formed of the first and second divisions of the *trifacial,* and the other of our fourth or *pathetic;* while the remainder of the *trifacial* was his fourth. The lateral cords of the sympathetic were the internal branches of the vagus. He seems to have been acquainted with the semilunar ganglion, of which, however, Willis gave the first good description. Van Horne[5] made his fourth and fifth pair out of the *trifacial.* Rolfincius[6] was the only anatomist before Willis who followed Fallopius in referring *all* the branches of the trifacial to *one* origin. Tho. Bartholinus,[7] one year before the appearance of Willis's 'Cerebri Anatome,' gave the following classification and description of the cerebral nerves: 1, Olfactory; 2, optic; 3, motor oculi; 4, the pathetic, which he confounded with the ophthalmic division of the *trifacial,* for he describes the frontal, lachrymal, and nasal nerves as its branches. He says, "Nostram distributionem proponunt Vesalius, Columbus, Platerus et Bauhinus." His fifth pair consisted of only the second and third divisions of the trifacial, described after Vesalius. His sixth pair were Galen's fourth—viz., the spheno-palatine portions of the same nerves. His seventh pair were the abducentes or sixth of Willis. His eighth pair were the auditory and facial. His ninth pair consisted of our eighth, and the lateral chains of the sympathetic, as their internal branches. His tenth pair were the hypoglossal. The plates are those of Vesalius. Such was the state of this department of anatomy when Willis began his labours on the cerebral nerves.

On the *functions* of nerves, a new doctrine was introduced by Piccolhomini in 1586.[8] He considered the nerves to be, by their very nature, either sensory or motor, indifferently, according to the organs and tissues through which they are distributed; and he even appeared to think that the organs of special sense are themselves the seat of sensation. "Ergo nervi non sunt instrumenta tangibilia sentiendi, sed sunt canales et instrumenta per quæ tota anima sentiens, idioque ejus facultas tangendi devehitur in membranas, *propria sensus tactus instrumenta.* Nam si in manus esset quoque instrumentum, videndi, ut oculis; gustandi, ut lingua; et reliqua; profecto manus videret, gustaret, auderet, imaginaretur."[9] And as the membranes are the instruments of sensation, so the muscles are those "quibus motrix facultas in motu sensibili edendo utatur."[10] These opinions were warmly supported by Laurentius,[11] Sennertus,[12] Bauhinus,[13] and Highmore;[14] and believing, as the last-mentioned anatomist observes, "Omnes partes quæ sentiunt, membranarum beneficio sentire," they naturally concluded that the sensory property of the nerves resides in their envelopes, or investing membranes. The experiments of Haller,[1] however, subverted this theory. In 1796 Reil published an excellent treatise on the structure and functions of nerves.[2] Their functions, he believed, depend on certain chemico-vital changes in their medulla, the nutrition and normal composition of which is maintained by their vascular membrane or pia-mater, which he considered "tanquam organum secretorium medullæ nervæ." Still, even by later anatomists it was thought that every motor nerve is endowed with the faculty of transmitting sensation; and it was reserved for Charles Bell to clear up the mystery which for more than two thousand years had taxed the ingenuity and exhausted the speculations of physiologists.

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[1] Theatrum Anatomicum, lib. iii. cap. xvii. (1605).

[2] De Humani Corp. Fabricâ, lib. vii. cap. ii. (1627).

[3] Syntagma Anatom., cap. xiv. (1641).

[4] Institut. Anatom., lib. iii. cap. i.

[5] Microcosmus, Sect. liv. (1660).

[6] Dissertat. Anatom., lib. vi. cap. xxxv. (1656).

[7] Loc. citat. lib. iii. cap. i.

[8] Anatomicæ Prelectiones.

[9] Op. citat. Lect. v.

[10] Ibid.

[11] Hist. Anat., lib. iv. quest. x.

[12] Institut., cap. ii.

[13] Op. citat.

[14] Op. citat.

According to Galen, the spinal cord commences at the lower border of the pons Varolii. In this description he was followed by all his successors to the time of Sylvius and Vesalius. The former traced its origin from the whole base of the brain;[3] the latter, from the corpora quadrigemina.[4] Columbus,[5] Varolius,[6] Spigelius,[7] Laurentius,[8] Riolanus,[9] Highmore,[10] described its origin by two roots—one large, from the base of the brain; the other small, from the cerebellum. But Piccolhomini[11] introduced a better mode of describing these parts, by restricting the term medulla spinalis to the extra-cranial portion; while to the rest he applied the term "medulla oblongata"—a distinction which is retained at the present day, although many anatomists differ with regard to the limits of the latter. Under the term "medulla oblongata," Willis comprehends the whole base of the brain, from the *corpora striata*, which he calls "medullæ oblongatæ apices," to the foramen magnum; and in this view he is followed by Vieussens, Winslow,[12] and others. Rolando[13] used the term in a more limited sense; while Ridley[14] substituted for it the term "isthmus;" and Chaussier[15] that of "mesocephale," which comprehended the pons, tubercula quadrigemina, with the superior peduncles of the cerebellum, and not the pons only, as is usually stated. Flourens[16] again, on the ground of his experimental inquiries, which led him to conclude that the seat of excitability begins or ends with the corpora quadrigemina, limits the medulla oblongata between these bodies and the eighth pair of nerves. In this country the medulla oblongata is generally understood to extend from near the points of the anterior pyramids to the lower border of the pons Varolii.

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[1] Element. Physiol., tom. iv.

[2] Excitation. Exercitationum Anatom., Fasc. i.

[3] Isagoge (1556), lib. iii. cap. xxii.

[4] De Hum. Corp. Fab., lib. iv. and vii.

[5] Loc. cit., p. 367.

[6] De Nervis Opticis.

[7] De Hum. Corp. Fab., p. 295.

[8] Historia Anatomica, p. 529.

[9] Anthropographia, lib. v.

[10] Corp. Hum. Disquisit. Anat., lib. i.

[11] Anatomicæ Prælectiones, lib. v. sect. 3 and 5. The whole of the description is excellent, and very far superior to that of any previous anatomist.

[12] Exposition Anatomique.

[13] Recherches Anatomique sur la moëlle allongée.

[14] Anatomy of the Brain.

[15] De l'Encephale, p. 106.

[16] Recherches expérimentales sur les Propriétés et les Fonctions du système nerveux, pp. 21 and 239.

ART. III.

Pathological Investigations in a Case of Paraplegia. By J. LOCKHART CLARKE, F.R.S., &c.

JOSEPH S—— was admitted an in-patient of the Hospital for Epilepsy and Paralysis, under the care of Dr. Brown-Séquard, May 9, 1863, for paraplegia. The following account was furnished me by Dr. Hughlings Jackson, assistant-physician to the hospital:

"The patient was a well-built man, forty-nine years of age, and was in general healthy. He had been ill seven months, but had only been away from work five. His first symptom was a sharp pain in the right side of the abdomen, and then a pain and tightness round the abdomen. These symptoms he had had ever since. Five weeks ago, he first found that his legs were weak. He had no pains in them, and no cramps, but slight 'pins and needles.' This last symptom he had not had lately. Sometimes he had had a little difficulty in passing his urine, but nothing marked, and had never had incontinence. He had had no blow on the back.

"He was carried into the ward, being quite unable to walk for the shortest distance, both legs being almost totally paralysed as to motion. The following note was made as to sensation: 'there is considerable feeling in the right leg, especially in the foot, but not at all in the left. In the left he could not feel tickling, nor distinguish the points of the compasses at any distance, and the feeling of pain was but little marked. On the right thigh he felt the points of the compasses at two and a half inches, and felt tickling perfectly well.'

"On the fifteenth of the month he began to have pain in the left side of the chest, and retention of urine. Although the chest was repeatedly examined, there was never any dulness or friction sound. He had much pain across the abdomen, was much exhausted, and his breathing was hurried. He died exhausted on the morning of the 19th, ten days after admission.

"At the autopsy there was considerable effusion of lymph between the pleural layers on the left side. The other organs of the thorax and the abdominal viscera appeared healthy."

The whole of the brain, the medulla oblongata, and the spinal cord, from the fourth cervical nerves downwards, were forwarded to me for examination by Dr. Hughlings Jackson.

Externally, the brain presented nothing unusual except a fine granular deposit of lymph on nearly all the convolutions on each side of the vertex, with some opacity of the arachnoid. All its internal parts were free from disease.

The medulla oblongata was in a perfectly healthy state.

The cord along the lower half of the dorsal region was unnaturally soft to the touch, the softest portion being opposite the fifth, sixth, and seventh nerves. In the lower part of this and the whole of the lumbar region, the surface of the cord was interspersed with numerous osseous plates of small but variable size, and embedded in the arachnoid. This is the locality in which these osseous plates are usually found.

Internal Condition of the Cord.—Concerning that portion which gives origin to the *second* and *third* cervical nerves, I am, of course, unable to afford any information, since, as already stated, it was not removed from the body. There is reason to believe, however, that it was not altogether healthy; for opposite the upper roots of the *fourth* cervical nerves certain indications of disease were found in both the white and grey substances. Of the *grey* substance, the principal lesion was along the outer side of each lateral half, between the anterior and posterior cornua, and therefore along the part which I have named the *tractus intermedio-lateralis.* This was much softer than in health; and when macerated in chromic acid, was more friable than usual.[1] Around the spinal canal, and in the transverse commissure, a large number of corpora amylacea were also observed. In the surrounding white columns, especially the posterior and the lateral, many of the nerve-fibres had evidently suffered from either partial destruction or complete disintegration of their white substance. At the *lowest* roots of the same pair of nerves—the fourth cervical—the principal lesion was in the neighbourhood of the posterior commissure—in which were observed two or three small spots of transparent granular degeneration—and in the anterior decussating commissure, the front of which, at the bottom of the anterior median fissure, was partially replaced by a heap of fat globules; but many of these bodies were changed from the perfectly spherical form by compression against each other, and in this shape bore a strong resemblance to aggregated particles of the white substance of nerves. In other regions of the cord similar particles were found in different parts of the grey substance, collected into groups, or arranged in strings like beads, and appearing like nerve-fibres composed of series of particles of their own white substance.

At the upper roots of the *fifth* cervical nerves a large proportion of the decussating fibres of the anterior commissure were also destroyed by a triangular mass of this fatty substance, as represented at *a*, fig. 1. This substance was lodged in the bifurcation of the bloodvessel *b*, which entered through the anterior median fissure *c*, and of which the branches distributed to the lateral grey substance *d d′ d″* were enlarged, while the tissue immediately surrounding them was slightly affected by granular destruction, and contained some fat particles. In one or two parts of the posterior commissure the tissure surrounding the cut ends of longitudinal bloodvessels had undergone a similar change.

Nearly the same condition of the grey substance was found through the greater part of the *sixth* cervical nerves, the *left* side being sometimes affected in the same way as the right. But in two or three

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[1] It will be interesting in reference to the diminished power of respiration in this case, to recollect that the lower rootlets of the spinal accessory nerves, as I have elsewhere shown, pass into and *through* the *tractus intermedio-lateralis.*

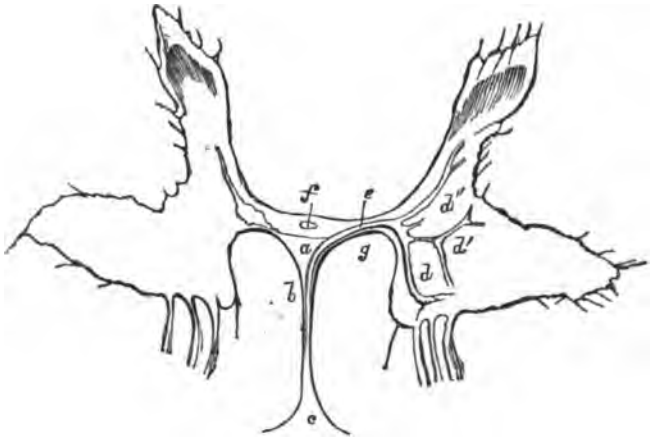

Fig. 1. (Preparation 61''.) × 9.

sections at the lower roots of these nerves the lesion on the right side was very considerable, as represented more highly magnified in fig. 2.

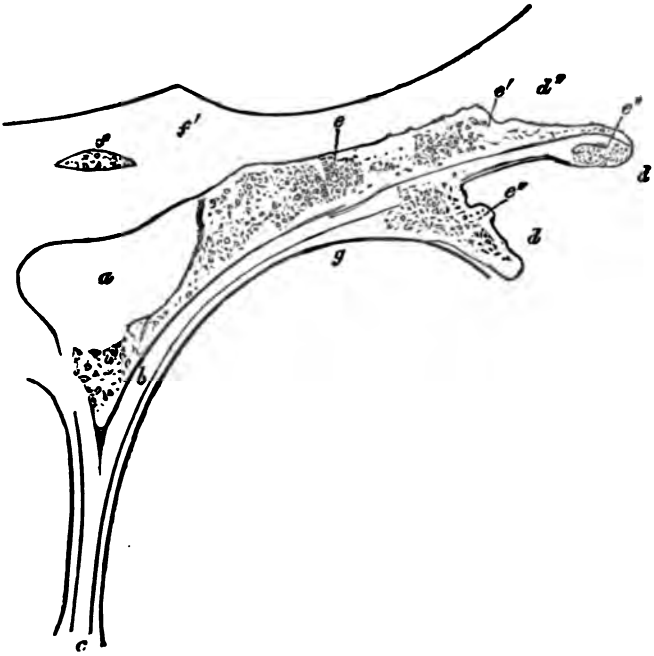

Fig. 2. (Preparation 60.) × 34.

The letters respectively indicate the same parts as in fig. 1. Here we find that a number of the decussating fibres of the anterior commissure have been replaced by a dense and dark mass of fatty particles, *a*, which projects more to the right than the left side, and has consequently caused a kind of tilting of that side of the posterior commissure, *f'*. From the right angle of this opaque mass a broad but more transparent tract of the same kind of degeneration, *e e' e''*, extended around the anterior column, *g*, as far as the root of the lateral grey substance, *d*, *d' d''*, destroying nearly all the nerve-fibres which come from that substance round the anterior column, *g*, to decussate through the anterior commissure. In this morbid tract the fatty particles were in some places collected into dark groups, as at *e*; in others they were more or less diffused through a semi-fluid and molecular substance, which was of the same nature as the larger bodies, and passed insensibly into them. The right division, *b*, of the bloodvessel which enters through the anterior fissure, *c*, was partially destroyed in its passage through the morbid space to the point, *e''*. The anterior roots of the nerves within the white columns were also affected, their white substance in some places appearing as clusters of globular masses, mixed with some oil-globules. In the white columns themselves, especially the anterior and lateral, many of the fibres suffered in a similar way; and at numerous points their white substance was resolved in granules, and presented a multitude of small transparent spots of molecular degeneration. A similar degeneration of tissue was observed around the cut ends of longitudinal bloodvessels.

Through the *seventh* cervical nerves the grey substance was in a much more healthy condition, the anterior and posterior commissures being normal; but a few points of fine granular degeneration were found on both sides.

Through the *eighth* cervical nerves the cord was nearly in the state just described, with the addition of a little granular destruction at the sides of the anterior commissure, and a slight extravasation of blood in the centre of the left anterior cornu, on a level with the highest roots of these nerves.

Through the first *dorsal* nerves the cord generally was nearly healthy; but a small accumulation of the fatty particles appeared in some sections at the bottom of the anterior fissure, in contact with the front of the anterior commissure, and extended in others along the *left side* of the anterior commissure, much in the same way as is represented in fig. 2.

Through the *second* dorsal nerves, however, nearly the whole of the posterior grey substance was soft and unhealthy, presenting numerous streaks and spots of transparent granular degeneration, which extended to the base of the posterior horns, and to the *tractus intermedio-lateralis*. Various parts of the white columns, especially the posterior parts of the lateral, were affected in the same way.

This condition of the cord continued through the *upper* roots of the *third* dorsal nerves, but on a level with the *middle* roots the grey substance was soft, and on the right there was seen the cut end of an enlarged bloodvessel (fig. 3, *h*), on the outer side of which was a considerable extravasation of blood, *i*, that extended into the *tractus intermedio-lateralis*, *k*.

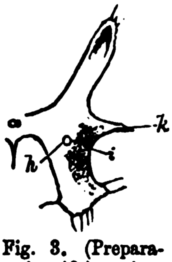

Fig. 3. (Preparation 49.) × 9.

Through the *fourth* dorsal nerves the posterior grey substance, in some sections on the right only, in others on both sides, was softer than natural, and affected in different parts by small pellucid spots of granular degeneration.

Through the *upper* roots of the *fifth* dorsal nerves the whole diameter of the cord was much increased, and the grey substance was enlarged, and somewhat altered in shape. Fig. 4 A represents a transverse section of the grey substance at this point. It had a peculiarly dark, opaque appearance, except at particular spots, which were unusually transparent. Examined under a power of from 200 to 400 diameters, the opacity and increase of size were found to result from the exudation of a granular substance, which in some places was collected into groups and irregular patches, with intervening areas of a much more transparent and more finely granular structure, as seen in the anterior cornu, *l*, on the right side, and along the whole of the anterior and lateral grey substance on the left side, *l′ l″* as far backward as the *tractus intermedio-lateralis*, *k*. Around the cut ends of the bloodvessels on each side of the central canal another form of degeneration was observed, which was more or less round or oval, but generally the latter,

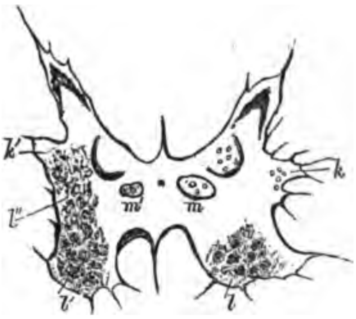

Fig. 4 A. (Preparation 41.) × 9.

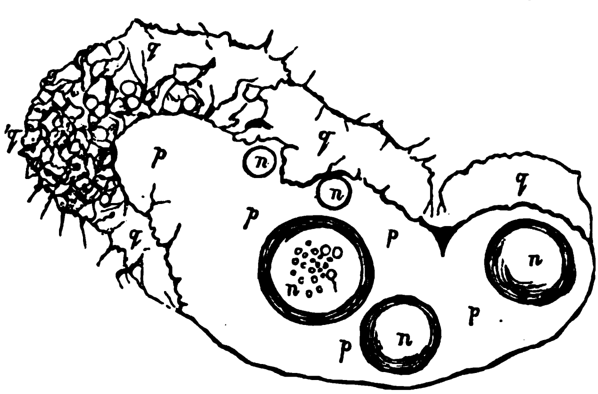

Fig. 4 B. (Preparation 42.) × 220.

as seen at *m m'*. At fig. 4 *n*, *n*, *n*, *n*, *n* represent five of these bloodvessels, of different sizes, magnified 220 diameters, and held together by a dense layer of connective-tissue, *p*, *p*, *p*, *p*, *p*, of a brownish hue, and thrown into folds around the walls of the vessels. Beyond this, on one side, the natural tissue of the cord was replaced by a transparent and kind of honeycomb substance, *q*, *q*, *q*, *q*, the structure of which is represented at '*q*. Smaller spots of a similar nature were seen at other parts of the grey substance, chiefly around bloodvessels; and an exudation of much the same kind, and somewhat resembling scaly epithelium, thickly encrusted the bloodvessel in the anterior median fissure. The white columns, although they appeared healthy under a low power, were found to be everywhere affected by this kind of degeneration. From the middle to the lowest roots of the same nerves (fifth dorsal) the lesions of both the grey and white substance were very great, but very different in appearance. Fig. 5 represents an en-

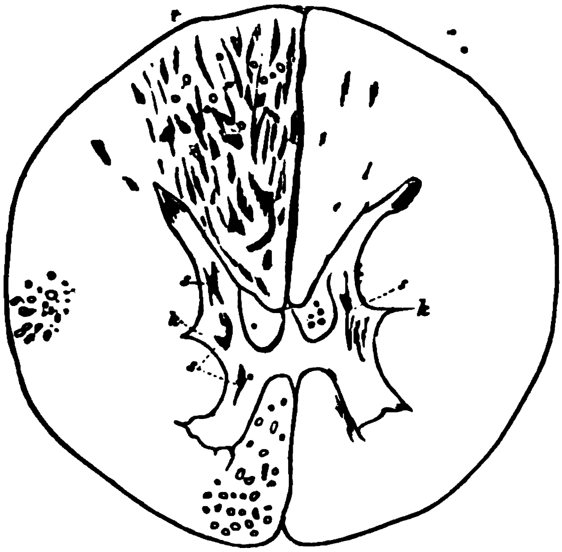

Fig. 5 A. (Preparation 89.) × 9.

tire transverse section of the cord from this part. Here we find the grey substance slightly altered in shape, somewhat unsymmetrical on the two sides, and everywhere more or less affected by extremely transparent spots and patches of degeneration. These, however, were most extensive along the outer parts of the lateral grey substance between the anterior and posterior cornua, *s*, *s*, *s*, involving a portion of the *tractus intermedio-lateralis*, *k*. The white columns—particularly the lateral and posterior—were also thickly interspersed with numerous and comparatively large patches or streaks of hypertrophied connective-tissue, forming with each other and with smaller streaks a kind of imperfect and irregular network, as represented in the posterior column of the left side at *t*, fig. 5 A. Many of these streaks and patches enveloped enlarged bloodvessels, and contained some fat-globules. Moreover, all the white columns were more or less damaged by another form of degeneration already noticed, and arising from partial or entire destruction of the whole thickness of the nerve-fibres. In transverse sections the morbid parts assumed the appearance of round or oval and perfectly pellucid areas, sometimes crossed by remnants of the white substance, and sometimes perfectly vacant; in some places sharply circumscribed and isolated by the surrounding tissue, in others coalescing to form a sponge-like structure with irregular or uneven outlines. Fig. 5 B represents some of these spaces in the lateral column magnified 220 diameters. The largest, most vacant, and most advanced spots of degeneration were nearer the surface of the columns, as indicated by the small circles in the white substance of Fig. 5 A.

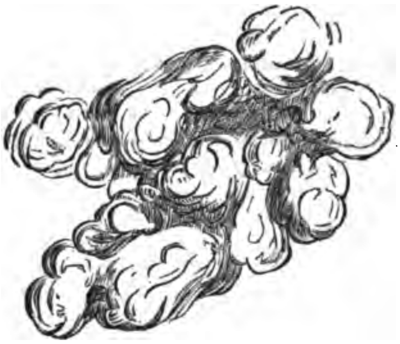

Fig. 5 B. (Preparation 28.) × 220.

This condition of the cord extended along the *upper* roots of the *sixth* dorsal nerves, but at their *middle* roots the posterior white columns — especially their deeper portions between the bases of the cornua—were very much softened and disorganized, small fasciculi of partially destroyed nerve-fibres being surrounded and separated to an unusual extent by connective-tissue into which a fluid or semi-fluid substance had been poured. The softening involved a small portion of the posterior commissure and of the posterior vesicular columns. Different parts of both the anterior and posterior grey substance were more or less softened by the same cause, and frequently reduced to finely granular, extremely transparent, or perfectly pellucid spaces, which were sometimes crossed, or only partly crossed, by the remaining fibres of nerve and connective-tissue. At the *lowest* roots of these nerves the two lateral halves of the grey substance were not only everywhere softened and affected in the manner just described, but began to approach each other in a very unnatural way.

At the upper roots of the *seventh* dorsal nerves both the grey and white substances were less soft and less damaged by granular degeneration; but the two lateral halves of the grey substance approached each other still more closely, so that the entire grey substance presented a striking resemblance to that which is found in the same region of the cord among the greater number of mammalia, as represented in fig. 6.[1] Nearly the same appearances extended through that portion of the cord which gives origin to the *eighth* dorsal nerves.

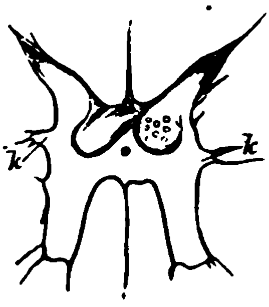

Fig. 6. (Preparation 26.) × 9.

At the *upper* and *middle* roots of the *ninth* dorsal nerves the whole of the grey substance was considerably softened and damaged by disease. It was also larger than natural, and somewhat altered in shape (fig. 7). On the right side there were three large, exceedingly transparent patches, *v v*, in which the natural structure was completely destroyed; the two outer involved the *tractus intermedio-lateralis*. In front of the canal was a large mass of fatty substance, *w*, which destroyed the greater number of the decussating fibres of the anterior commissure. At the *lower* roots of these nerves, as well as through most of the *tenth* pair, the grey substance resumed its normal size, and nearly its normal shape, although it was still affected by minute spots of transparent granular degeneration. The white columns were much more healthy. At the *lower* roots of the *tenth*, and through the eleventh nerves, the posterior and middle portions of the grey substance again increased in size, and became softer. In some sections at the tenth nerves there were found at the sides of the canal, and around the bloodvessels, a sponge-like form of degeneration similar to those at *m*, *m*, fig. 4 A, and *q*, *'q*, fig. 4 B; and at the *eleventh* dorsal nerves a large triangular mass of fatty substance, similar to that represented at *w*, fig. 7, destroyed a great proportion of the decussating fibres of the anterior commissure.

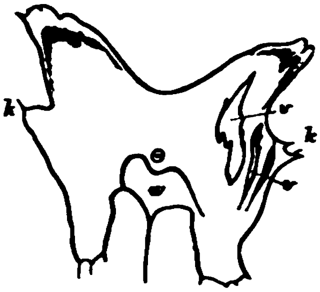

Fig. 7. (Preparation 19.) × 9.

The lumbar enlargement, from the second lumbar nerves downward, was much less damaged than the dorsal region. In shape and size the grey substance was perfectly normal, but minute spots and patches of granular destruction could readily be discovered at various parts, under a sufficiently high power. Throughout the whole of this region the anterior commissure was more or less damaged. At the lower end of the enlargement the cord was nearly healthy; but the canal which is here naturally large was closed, as it was in all the other regions, by a confused heap of epithelium, which sometimes assumed a kind of bilobed appearance.[1]

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[1] See my Researches on the Grey Substance of the Cord. Phil. Trans. 1859. This is the second time that I have found this curiously abnormal form of the grey substance. See a case of "Wasting Palsy" in Beale's Archiv. No. xiii. vol. iv. fig. 13.

At the upper part of the *conus medullaris* the old form of granular degeneration began to reappear, accompanied by another kind of degeneration in the form of atrophy or shrinking of both the white and grey substances. The left lateral half was much smaller than the right, which was normal in size. Fig. 8 represents a transverse section of the cord at the upper part of this region; $x\,x$, the white columns on the right side; $x'\,x'$, the same on the left; y, the posterior horn or grey substance on the right side; y', the same on the left; $z\ z'$ are the anterior cornua on the opposite sides. It will be seen that on the left side the size of the *grey* substance is much less than on the right. The central canal, which, as already stated, was filled up by the epithelium, was surrounded by numerous streaks of transparent degeneration, z'', extending for some distance on each side into the grey substance. Through the rest of the *conus medullaris*, as far as the *filum terminale*, the whole of the grey substance was more or less injured by streaks and patches of the same description.

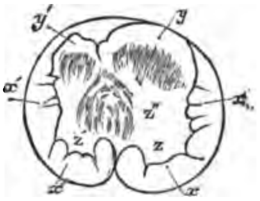

Fig. 8. (Preparation 4.) × 9.

Throughout the dorsal region the nerve-cells contained a larger amount of pigment than usual, and in the middle and lower parts of this region numbers of them were completely filled with dark-brown pigment-granules, which either obscured or entirely concealed their nuclei; but neither the cells nor their processes were shrunk to any appreciable extent.

We see, then, that the morbid appearances of which I have now concluded the description not only present us with an interesting variety of the lesions of structure to which the spinal cord is liable, but afford a full explanation of the symptoms and functional derangements recorded in the history of the case. But they are more than sufficient for this purpose, for in certain parts of the cord we discovered structural changes to which no corresponding impairment of function is recorded in the history. We have seen that at the level of the *fourth* cervical nerves the anterior and posterior commissures, and, in some sections, the *tractus intermedio-lateralis* were injured by disease; while in the white columns many of the nerve-fibres had suffered either partial or entire destruction. Now, the only recorded symptom to which we can point as likely to result from these lesions is the laboured respiration; for this might have been partly due to injury of the *tractus intermedio-lateralis*, which, as already stated, is connected with the lower roots of the *spinal-accessory* nerve. Through the brachial or cervical enlargement we found that the decussating fibres of the anterior commissure were destroyed to a considerable extent by fatty degeneration; that bloodvessels in the neighbourhood were enlarged and diseased; that the anterior roots of the nerves, in some sections, were more or less damaged; and that many of the fibres of the white columns, especially the anterior and the lateral, had suffered in a similar way. And yet, notwithstanding these various lesions of structure, we have not a single corresponding symptom or impairment of function recorded in the history of the case. There is no mention of any abolition whatever of either motion or sensation in the upper extremities; and on making further inquiries upon this point I was informed that the patient had apparently the perfect use of his arms, and employed them in the usual way in eating his meals. Yet there can be no doubt that there was some abnormal condition of the arms and hands; and it is extremely probable that a closer scrutiny during life would have revealed its nature. In a former case of "wasting palsy" published in this Journal,[1] it was at first stated that the patient had the perfect use of his legs, and walked apparently quite well; but it was afterwards discovered that his "right foot was affected in some way," and that "there was no spring in the instep." These facts show, as I before insisted on, the importance of a thorough investigation of the patient's condition during life; and it is just in those cases in which only one or two particular spots of the cord are affected that the detection of some obscure or lurking symptom would be valuable in regard to the physiology as well as the pathology of this centre.

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[1] This blocking-up of the canal by a heap of broken epithelium has led some modern microscopic anatomists to deny the existence of a canal in the adult cord, and to regard this heap of epithelium as a mass of nerve-cells. Kölliker called it the *substantia grisea centralis*. Both before and since this period, however, I pointed out the real nature of this structure. See Phil. Trans. 1851-53, 1858, 1859, 1861.

Of the spinal cord in this case I have made, and retain by me, upwards of one hundred preparations, including those from which the above sketches are taken, and displaying a great diversity of morbid appearances which it would be impossible, and indeed unnecessary, to describe or represent in this communication.

ART. IV.

Observations on Sterility in Man; with Cases. By T. B. CURLING, F.R.S., Surgeon to the London Hospital, and Examiner in Surgery to the University of London.

STERILITY is a condition which has usually been restricted in its application to the female, or in the male has been confounded with impotency; and until recently our knowledge of the impaired functions of the male reproductive organs has not warranted any distinction being drawn between an incapacity for sexual intercourse and an inability to procreate. The object of this paper is to show that a want of aptitude to impregnate may co-exist with the capacity for sexual intercourse; or, in other words, that man is subject to *sterility*, independently of *impotency*. The subject is not altogether new, MM. Gosselin, Follin, Godard, and others, having published some important facts in relation to it, but opportunities of making the necessary inquiries are extremely rare; and as doubt exists of the soundness of the conclusions which have been arrived at, facts of a contradictory character having been adduced, I have been led to examine the question, and my practice has enabled me to collect some interesting observations bearing upon it. The importance of the subject, as respects the happiness and results of married life, must be my apology for giving details, the recital of which could not be avoided.

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[1] October, 1863.

Sterility in man may arise from the following causes:—

1. Malposition of the testicles. 2. Obstructions in the excretory ducts of the testicles. 3. Impediments to the escape of the seminal fluid.

1. *Sterility from Malposition of the Testicles.*—The opinion of John Hunter, "that when one or both testicles remain through life in the belly, they are exceedingly imperfect, and probably incapable of performing their natural functions," has been the subject of much comment, and in my work on 'Diseases of the Testis' I expressed my adhesion to the views of Professor Owen, who, in differing from Mr. Hunter, remarks that there is nothing in such a situation which necessarily tends to impair the efficiency of the testicles, since in many animals they constantly form part of the abdominal viscera; and in those in which the testes naturally pass into the scrotum, their continuance in the abdomen is accompanied only with a difference of size or shape. Now, we may readily suppose that this may influence the quantity, but not necessarily the quality of the secretion. The facts which I am about to adduce have corroborated Mr. Hunter's opinion in a remarkable manner, and have led me entirely to change my own views on the point in question.

That a cryptorchic person, or a man with both testicles in the abdomen or in the groin, may have a masculine development, passion for women, and the power to copulate, is beyond question, being satisfactorily established by several well-authenticated instances, although there have been many cases in which such persons were impotent, and had not fully exhibited the external characters of the male sex. When the testicle has not passed into the scrotum, the gland is nearly always small in size; generally it is healthy, but undeveloped; that is to say, it has not undergone the enlargement and change in structure which take place at puberty. In some instances, especially when seated in the inguinal canal, it is withered and atrophied, having undergone fibrous and more rarely fatty degeneration, and exhibiting no trace of glandular structure. But the question to be considered is, whether a testicle which has not passed into the scrotum can secrete a fertilizing fluid—a fluid which, when emitted in sexual intercourse, is capable of impregnating the female. I assume, as quite established, that to possess this property the semen must contain zoosperms.

Professor Goubaux, a distinguished French veterinary surgeon, was the first who noticed in horses, not only that the testicles detained in the abdomen were soft and small in size, but that the fluid in the corresponding vesiculæ seminales was destitute of spermatozoa. In 1851 M. Follin briefly alluded to three instances of detained testicle on one side in the human subject, in which he found the fluid in the vesicula seminalis of the same side destitute of spermatozoa, though they were present on the other side.[1] In 1855 I described[2] the examination of a man aged thirty-six, whose right testicle was in the abdomen, small and undeveloped. There were no spermatozoa in the efferent ducts nor in the right vesicula seminalis, but the left contained them in abundance. In 1856 Messrs. Goubaux and Follin, in a joint memoir, 'Sur la Cryptorchidie chez l'homme et les principaux Animaux Domestiques,' read at the Société de Biologie, adduced several instances in man and animals in which testicles remaining in the abdomen were small, and did not secrete sperm. They furnished also a few examples of animals which, though possessing the desire and power to copulate, were quite sterile. The late M. Godard, in a memoir read at the Société de Biologie on the same evening as the above, mentioned the cases of three cryptorchic married men who had no children, and affirmed that such persons were always sterile. This earnest and indefatigable pathologist, whose recent death is a loss to medical science, in a more recent work, supported this opinion by additional facts, to which I shall have occasion to refer. The proofs adduced, however, by these observers, were not sufficiently cogent and numerous to establish the law that cryptorchics are infertile; and it could not be expected that assent should be given to results so remarkable and unexpected without evidence of the most convincing character. Opposite opinions continue to be entertained, and Dr. Taylor, in the last edition (1861) of his work on Medical Jurisprudence, after briefly noticing some recent observations on this subject, states that when the power of sexual intercourse exists, "this imperfection does not offer any bar to marriage, nor is it a ground for divorce."

Case I.—In 1859 a gentleman, aged thirty-eight, consulted me under the following circumstances:—His testicles had never properly descended into the scrotum, and though not deficient in copulative powers, he had been married eleven years without his wife becoming pregnant. He was desirous of knowing whether this was owing to any fault in himself. In external development, this gentleman had all the attributes of the male sex. On examination, I found his penis normal, and his testicles small in size, the right being less than the left. Both were lodged in the groin, just outside the outer ring. The right could be easily pressed up into the inguinal canal, through rather a large external ring. Pressure on the left caused it to recede into the upper part of the thigh, just below Poupart's ligament, where the integuments were loose. When the left testicle became thus displaced, which occurred occasionally, the patient felt uneasiness, referred to the navel. The scrotum was small and imperfectly developed; the left testicle could be depressed into it by a little force. He stated that he performed the sexual functions about twice weekly, and when younger had done so more frequently. The fluid emitted in intercourse was carefully examined by myself and Dr. Andrew Clark separately, on three occasions, at intervals of about a week. It was found to be entirely destitute of spermatozoa. With the view of forcing the left testicle into the scrotum, and retaining it there, I recommended his wearing the moc-main lever truss, but this treatment was not persevered in.

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[1] Archives Générales de Médecine, 4e Série, t. xxvi. p. 265.

[2] Diseases of Testis, 2nd edit., p. 27.

CASE II.—In 1852 I was requested to see an inmate of a charitable asylum, a youth, aged eleven, whose testicles had not passed into the scrotum. The right was lodged just outside the external ring; the left was not discernible at all. He had no scrotum. In 1861, at the age of twenty-one, he again came under my notice. He was rather short in stature, but had a masculine development. He wore a moustache, and had abundance of hair on the pubes. His penis was rather large. He held a clerk's situation in the city, and had been married twelve months. He stated that he had frequent intercourse with his wife, followed by ejaculations. She had not become pregnant. Some fluid obtained from the urethra immediately after sexual intercourse was sent me on two occasions, the second being after an interval of eighteen months. It was carefully examined by myself and others, and found to be destitute of spermatozoa.

CASE III.—In April, 1861, I saw with Mr. Duchesne, of Woodford, a gentleman, aged forty-six, a married man, who had serious disease of the left testicle, which had commenced about a month previously. The gland, being quite disorganized, was removed by me on the 22nd. The wound healed favourably. During my attendance I noticed that the right testicle had not emerged from the abdomen. After his recovery, and quite two months after the operation, he had intercourse with his wife. The fluid emitted was examined, but no spermatozoa could be discovered in it.

CASE IV.—In March, 1863, I was consulted on the propriety of marriage under the following circumstances:—A gentleman, thirty-nine years of age, stated that about fourteen years ago he was in the habit of frequent sexual intercourse, when one night after connexion the left testicle was attacked with violent inflammation, which was followed by a gradual wasting of the gland. The right testicle was small and had not fairly passed into the scrotum. The sexual appetite was keen, and coition was effected with ease, the emission being fairly copious. My patient was healthy and moderately robust. The left testicle was reduced to the size of a pea; the right was properly formed and tolerably firm, but quite small, like an undeveloped testicle before puberty. Some fluid emitted in sexual intercourse was sent me on two occasions. In both instances it was thin and destitute of spermatozoa. I consequently gave an opinion adverse to his marrying, on the ground that he was unfit to procreate—that his wife would be barren.

In Table I. I have added to these four cases five others, well authenticated, in which the fluid ejaculated by men with retained testicles was submitted to examination and found to be wanting in spermatozoa.

In confirmation of the results obtained in these cases, I may adduce some interesting observations made upon the lower animals. The following are related in Messrs. Goubaux and Follin's 'Memoir.'—A horse, twelve years of age, though presenting all the characters of an

TABLE I.

No.	Age.	Position of Testicles.	Sexual Condition.	State of Ejaculated Semen.	Authority.
1	38	. . Both in the groin, outside the abdominal rings.	. . Married eleven years; copulative powers satisfactory; no children.	. . Destitute of spermatozoa; examined three times.	. . Mr. Curling.
2	21	. . Right, outside abdominal ring; left, within the abdomen.	. . Married two years; powers satisfactory; wife had not become pregnant.	. . Destitute of spermatozoa; examined twice at an interval of eighteen months.	. . Mr. Curling.
3	46	. . Right, in the abdomen; left, removed by operation.	. . Married; powers satisfactory; no child.	. . Destitute of spermatozoa; examined once.	. . Mr. Curling.
4	39	. . Right, outside inguinal canal; left, completely atrophied.	. . Single; powers satisfactory . .	. . Destitute of spermatozoa; examined twice.	. . Mr. Curling.
5	44	. . Left, outside inguinal canal; right, in the scrotum, but epididymis obstructed after orchitis.	. . Married; competent, but weak.	. . Destitute of spermatozoa; examined once.	. . Godard: Études sur la Monorchidie et la Cryptorchidie, p. 103.
6	22	. . One in iliac fossa; the other in inguinal canal.	. . Single	. . Destitute of spermatozoa; examined several times.	. . Godard: Ibid., p. 147.
7	24	. . Both in the abdomen.	. . Single; had contracted gonorrhœa.	. . Destitute of spermatozoa; examined three times.	. . Godard: Ibid.
8	Not stated.	. . Both in the inguinal canal.	. . Married; powers satisfactory . .	. . Destitute of spermatozoa; examined several times.	. . Puech: Gazette Hebdom., Dec. 1856.
9	24	. . Both in the abdomen.	. . Single; had desires, erections, and emissions.	. . Destitute of spermatozoa; examined four times.	. . Partridge: Path. Trans., vol. ii.

entire horse, bore the well-known marks of castration on the right side, but on the left side there was no trace of cicatrix, and no scrotal sac or testicle. Erections were manifested in the vicinity of mares. After covering one, the fluid emitted from the urethra was examined and found to be destitute of spermatozoa.—After the purchase of a horse six years old, a question arose between the buyer and vendor whether the animal could be used as a stallion. The veterinary surgeon could discover no testicles, although the skin of the region presented no marks of the animal having been castrated. In presence of a mare the horse manifested undoubted signs of the influence of the approach. He was allowed to cover her, but accomplished the act with more difficulty, and especially slowness, than usual with a vigorous stallion of the age. The examination of the fluid emitted on three occasions, at intervals of several days, afforded no trace of spermatozoa.[1] M. Godard relates[2] that a cryptorchic dog covered a bitch in heat four different times in March, 1856. The fluid ejaculated on each occasion was found destitute of spermatozoa. In February, 1857, the same dog, who was addicted to coition, again covered the bitch, and the sperm emitted was also found to contain no zoosperms.

I have already alluded to a few instances in which the fluid found after death in the substance of a retained testicle, in the epididymis or vas deferens, or in the vesicula seminalis on the side corresponding to the malplaced gland, has been examined and found destitute of spermatozoa. Many other examinations have been made. They are collected in Table II. (following page), which includes three observations of my own. Spermatozoa have not been discovered after death in the spermatic ways of a detained testicle in any one instance that I know of.

Gosselin and Godard make mention of several instances of cryptorchics who were married but had no children; and I know of another instance in addition to those related in this paper. Though it is most probable that in all these cases barrenness was owing to the absence of a fecundating property in the semen, yet, as it was not examined microscopically, no scientific value can be attached to these observations. It would be objected that the cause of sterility might *possibly* have been in the female.

The facts which have been adduced, as opposed to the conclusion that cryptorchics are sterile, are chiefly instances in which they are reputed to have procreated children. Mr. Poland relates that a man, aged twenty-nine, once in the Dragoons, was admitted into Guy's Hospital on account of an omental hernia. His testicles had not descended, and there was no scrotum. The penis was well developed, and he had all the other signs of virility. He married when he was twenty, had two children by his first wife, and had been married two

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[1] Professor Spooner, of the Veterinary College, informs me that he has examined several testes taken from the abdomen of horses after death, and in all of them the gland was small in size, and without spermatozoa.

[2] Ibid., p. 147.

TABLE II.

No.	Age.	Cause of Death.	Position of the Testicles.	Condition of Malposed Testicle.	State of Seminal Fluid.	Authority.
1	36	. . Recent injury.	. . Right, within the abdomen; left, in the scrotum.	. . Healthy, but undeveloped; weight, 110 grs.	. . No spermatozoa in right vesicula seminalis, and ducts of right testicle.	. . Curling: Diseases of Testis, 2nd edition.
2	27	. . Intestinal obstruction.	. . Left, outside abdominal ring; right, in the scrotum.	. . Small and undeveloped.	. . Spermatozoa in left vesicula seminalis; no spermatozoa in left vas deferens and vesicula.	. . Curling: Patholog. Trans., vol. ix.
3	25	. . Disease of the heart.	. . Right, in the inguinal canal; left, in the scrotum.	. . Healthy, but small; weight, 132 grains.	. . Spermatozoa in right vesicula seminalis; no spermatozoa in right vas deferens and vesicula; spermatozoa in abundance in left vas deferens and vesicula.	. . Curling: Patholog. Trans., vol. xii.
4	24	. . Recent injury.	. . Right, within the abdomen; left, in the scrotum.	. . Healthy, but small.	. . No spermatozoa in right testicle, epididymis, vas deferens, and vesicula seminalis; spermatozoa in left epididymis, vas deferens, and vesicula.	. . Godard: Études sur la Monorchidie et Cryptorchidie, p. 54.
5	26	. . Meningitis.	. . Right, in the inguinal canal; left, in the scrotum.	. . Healthy, but small.	. . No spermatozoa in right vesicula seminalis; spermatozoa in left vesicula.	. . Godard: Ibid., p. 61.
6	50	. . Strangulated hernia.	. . Both outside abdominal rings.	. . Healthy.	. . No spermatozoa in the testicles, vasa deferentia, and vesiculæ.	. . Godard: Ibid., p. 124.
7	30	. . Peritonitis.	. . Both in inguinal canals.	. . Healthy.	. . No spermatozoa in the testicles, vasa deferentia, and vesiculæ.	. . Godard: Ibid., p. 127.
8	42	. . Strangulated hernia.	. . Both in inguinal canals.	. . Healthy.	. . No spermatozoa in the testicles.	. . Debrou: Jour. Hebd. de Médecine et de Chir., tome viii. p. 3.

years to a second wife.[1] Mr. Cock has mentioned to me the case of a man whose testicles had not descended, and in whom the virile functions were perfect. He had married twice, and had children by each wife. He was a man of dissipated habits, and had served in a public-house. Mr. Durham has communicated to me the particulars of the case of a man with double oblique inguinal hernia, and with both testicles lodged in the inguinal canals. He was a well-grown, healthy labourer, aged thirty-two, and was operated on by Mr. Durham, in Guy's Hospital, in consequence of strangulation of the hernia on the left side. The patient recovered favourably. The left testicle was exposed and handled during the operation. It was smaller than usual. He had a masculine development,[2] was married, and his wife had borne him two children. He stated that since puberty he had experienced strong sexual desires, and had always been competent. No opportunity was afforded for the examination of his seminal fluid, and the man scouted the idea of his testicles being inefficient.

I feel no little hesitation in calling in question the claims to paternity in instances of this kind; but it is remarkable that as yet no case has been found in which a retained testicle has been fully proved to be capable of secreting a fertilizing fluid. The observations collected in this paper seem sufficient to show that as a rule they do not; and although I see no valid reason why there should not be exceptions—and Mr. Durham's case may possibly be one—still, the evidence is wanting to establish the exception in either of the instances of reputed paternity which I have mentioned. Dr. Debrou (d'Orleans) relates the case of Lebert, a man aged forty-two, who died in the Hôtel Dieu, at Orleans, of strangulated inguinal hernia on the right side, after four days' illness. After death both of his testicles were found in the inguinal canals, the scrotum being wanting. The body in other respects was that of a robust, well-formed man. He had been married, and was said to have liked intercourse with his wife, who had borne him a son, then eight years of age. His testicles, which were normal in structure, were carefully examined by Gosselin and Godard separately. They were unable to discover spermatozoa in either of them;[3] Debrou adduces this case as an argument to show that the spermatozoa are not necessary to the fertility of the semen; but as their presence is admitted by the best physiologists to be essential, and as they are constantly found in the testicles of robust men, we may fairly conclude that the impregnation of the wife was most probably due to another source than the legitimate one.

It has been suggested that the testicles may secrete spermatozoa at one time and not at another, and that although they were not discovered in the foregoing cases, impregnation may have occurred at a period when the testicles were performing their functions properly. Numerous observations on the spermatic fluid by myself, Dr. Davy, and others, have fully shown that in healthy adults the vesiculæ seminales and vasa deferentia almost invariably contain spermatozoa, whilst there is no evidence whatever to show that the testicles secrete a fluid at one time perfect, and at another time destitute of its essential element. There is no analogy to be found in the periodic intermissions in the sexual functions of the lower animals, since in them, when the secretion of the testicles is suspended, the power to copulate is also in abeyance.

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[1] Guy's Hospital Reports, Second Series, vol. i. p. 162.

[2] I visited the man in Guy's Hospital, and can bear testimony to his manly appearance.

[3] Gazette Hebdomadaire de Médecine et de Chirurgie, t. viii. 1861, p. 3.

As a malposed testicle does not secrete a fertilizing fluid, we have a strong additional reason for promoting the passage of the gland into the scrotum; and in early life, when the testicle has emerged from the abdomen, much may be done by gentle manipulation to obtain this end.

It has not been ascertained satisfactorily why a retained testicle does not perform its secreting function. One cause, no doubt, is imperfect development; for, as I have already remarked, the malplaced glands are small in size, and frequently have not undergone the change which takes place at the approach of puberty. But in several instances, mentioned by Godard, this must have occurred, for he states that the tubuli could be completely unravelled, which is not the case in an undeveloped gland.[1]

2. *Sterility from Obstructions in the Excretory Ducts of the Testicle.*—In 1853, M. Gosselin made known some curious researches in relation to this subject. He carefully examined the semen in twenty men who had been attacked with double epididymitis after gonorrhœa. In fifteen of these cases which were comparatively recent, a callosity existed in the tail of the epididymis at the time they seemed to be cured. In all, the genital functions appeared fully restored and the sperm normal. The semen was repeatedly examined at intervals of several weeks, but no spermatozoa were detected. M. Gosselin lost sight of all but two cases, and in these the return of spermatozoa in the semen occurred after some months, and coincidently with the complete disappearance of the induration in the epididymis on one side. In the remaining five of the twenty cases the double epididymitis had occurred several years previously. One man, aged forty-five, had been attacked twenty years before, but the left callosity no longer existed, and spermatozoa were found in the semen. In another man the disease dated back five years, and had left a considerable induration at the lower part of each epididymis. The general health was good. No spermatozoa could be detected. In the three other cases

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[1] With the view of ascertaining what influence simple position might have on the functions of the testicle, I commenced some experiments on animals. It is well known that in certain rodents the testicles remain in the abdomen until the season of heat, when they descend into the scrotum and secrete semen. My experiments on the adult guinea-pig did not answer, for the domestic animal was always in heat. I attempted to close the abdominal ring of the young animal with sutures, in order to prevent the testicle escaping at all from the abdomen, but the parts were so fragile and delicate, that the sutures soon came out, and the object was not attained. I refer to these experiments, because they indicate a course of inquiry which might still be followed out with advantage.

the disease had occurred ten, six, and four years before. There was hardness on both sides. The testicles were otherwise unaltered. The indications of virility were quite satisfactory, and the semen presented its usual appearance. The individuals had all been married several years, but had no children. The sperm was carefully examined and found destitute of spermatozoa. One of them had had children by a former wife before the attack of double epididymitis.[1] Since the publication of the preceding observations, M. Gosselin has met with two cases of men who, after suffering from bilateral epididymitis during their youth, had retained an induration on each side. They had been married several years and had no children. In both the virile powers were not, apparently, weak, but the sperm was entirely wanting in spermatozoa.[2]

The following cases which have occurred in my practice, show the importance of these inquiries:

Case V.—A stout, well-built man, aged forty-two, a widower, desired to obtain my opinion on the propriety of marriage. In early life he had indulged freely in sexual intercourse, and at the age of twenty-eight contracted a gonorrhœa, which was followed by double orchitis. This did not cause any loss of power, and at the age of thirty, he married a young healthy woman. His wife had no children, and died ten years after the marriage. He then formed an illegitimate connexion with a young woman who had previously borne a child, but his acquaintance with her did not lead to her becoming pregnant. He stated that his sexual powers had declined slightly within the last two years, but he was quite efficient. He had repeatedly experienced uneasiness in the testicles the day after sexual intercourse. The question submitted to me was his ability to procreate children, as he contemplated a second marriage in the event of a decision in the affirmative. I found the right testicle of fair size, the left somewhat small, and both rather flaccid. In the lower part of the epididymis of each testicle there was a firm induration a little tender on pressure. Some discharge emitted in sexual intercourse was brought to me for examination. It was whitish, turbid, and glutinous. There was no trace of spermatozoa or spermatic granules. I gave my opinion that, in the event of marriage his wife would be barren.

Case VI.—In 1860, a strongly-built man, aged forty-four, who had just arrived from a distant colony, consulted me in the following difficulty:—Twelve years ago he married a healthy young woman, who bore him a child, now eleven years of age. Two years after marriage he got a chill after a long fatiguing ride in wet boots. He was seized with pain in the loins and bladder, had turbid urine and an urethral discharge, and was afterwards attacked with double orchitis. He became weak and emaciated, and was laid up five or six weeks. On recovery from this illness he found his sexual powers diminished, but he stated that they were still strong, and he was capable of indulging two or three times a week. His wife, however, had not conceived again. She was dissatisfied, desiring to increase her family, and believed he was at fault. It was arranged between them that he should seek advice in the mother country, and in the event of his returning without the ability to beget children, that they should separate. His testicles were rather small and flaccid. At the lower part of the epididymis of each gland there was a distinct induration, and the swellings were morbidly sensitive. The fluid emitted during erotic dreams was examined on two occasions. It was thin, and entirely wanting in spermatozoa. After experiencing sexual desires he had uneasiness in the testicles. I gave the opinion that he was incapable of procreation; but I also ventured to intimate that, however great might be the desire for children, sterility acquired after marriage was not a sufficient ground to justify a separation, especially as he was able to gratify his wife, though not to make her a mother. He gave me to understand, notwithstanding, that the arrangement would be carried out.

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[1] Archives Générales, 5e Série, t. 11.

[2] Note to the French translation, by M. Gosselin, of my work on Diseases of the Testis, p. 288.

CASE VII.—A medical gentleman of my acquaintance, aged forty-five, moderately robust, contracted syphilis twenty-five years ago, and the next year had an attack of acute orchitis on the left side. This was followed by complete atrophy of the testicle, the organ being reduced to about the size of a French bean. He suffered at the same time from epididymitis on the right side. Slight secondary symptoms occurred during nearly ten years, but since then there has been no trace of the disease. He married thirteen years ago. His right testicle is of fair size, but there is decided enlargement and induration of the epididymis. He has never been deficient in virile power, and the emissions have been abundant. His wife has never become pregnant. Between three and four years ago, he had occasion to examine the urine of a patient containing spermatozoa, and for the sake of comparison placed some of his own semen in the microscope. He was surprised to find it entirely destitute of spermatozoa. Since then he has frequently searched for them in the fluid emitted in sexual intercourse, but had never succeeded in finding any.

CASE VIII.—In 1858, a gentleman, thirty-eight years of age, consulted me under the following circumstances:—He stated that, in India ten years before, after excitement from drink and excessive indulgence in sexual intercouse, he was attacked with violent inflammation of the prostate or parts around. He was obliged to embark for England, and was unable to obtain advice on board the ship. An abscess formed and burst in three situations—into the rectum, into the urethra, and in the perinæum. After his return to England, an elastic catheter was retained in his bladder for the cure of the urinary sinuses. This caused inflammation of both testicles. He discontinued the instrument and went to the sea-side, where, after many months, the sinuses closed, but he has since been subject to a mucous discharge in his urine. His health was good, and he was robust and active. Seven years ago he married, but his wife had never become pregnant. His desires were strong and his powers sufficient. In intercourse no distinct emission took place. He had the sensation of ejaculation with uneasiness at the neck of the bladder, but no discharge followed. His urine had been repeatedly examined after coition, but no spermatozoa had been discovered in it. He had no stricture nor enlargement of the prostate. There was a distinct induration in the lower part of the right epididymis. The testicles were, in other respects, sound and of fair size. The absence of emission led me to conclude that the inflammation and abscess had caused an obstruction in the ejaculatory canals. I recommended a prolonged course of the iodide of potassium, and the application of tincture of iodine to the perinæum, without any sanguine hope of absorption of the supposed source of obstruction after so long an existence. No change ensued. In March, 1863, this gentleman, for the first time, called my attention to a small tumour, about the size of a large pea, in the vas deferens, about an inch and a half above the left testicle, which, it was supposed, might obstruct the passage of semen. Being very anxious to acquire the power to impregnate, he requested me to remove the tumour. Though not anticipating a satisfactory result, I consented to perform this slight operation, April 4th. Sensibility having been annihilated by a freezing mixture, and the vas deferens fixed by a clamp, I cut upon the duct, and avoiding the veins around, opened it just below the tumour, and introducing a fine probe, found the canal completely obstructed by the swelling. It consisted of a cyst containing a soft whitish substance like sebaceous matter. This was removed, and an opening made into the duct both above and below. The small wound in the scrotum was closed with a single suture. Matters went on very well for three days when gout attacked one foot, and was shortly followed by orchitis on the left side, with considerable swelling and thickening of the spermatic cord. Under purgative treatment with colchicum the gout subsided, but the orchitis proved indolent. The patient's general health was a good deal disordered. Suppuration occurred in the spermatic cord, and the part did not heal for three weeks. There has been no restoration of the passage for the semen.

M. Godard has recorded an interesting case (Case V. in Table I.) of a strong, vigorous man who had the left testicle in the groin, and the right one, of full size, in the scrotum. When young he was much addicted to women, and became the father of a child. At the age of twenty-one the testicle in the scrotum was attacked with gonorrhœal orchitis, which became chronic, and left a deposit in the tail of the epididymis. This was followed after five years by stricture in the urethra, and a second attack of orchitis in the right testicle. At the age of thirty-three he married, but his wife never became pregnant. She died at the end of five years, and at the age of thirty-nine he married again, but had no children. The ejaculated sperm was examined by M. Godard and others, but no spermatozoa could be detected in it. This is a case of sterility arising from a double cause—from malposition of one testicle, and obstruction in the excretory duct of the other.

The preceding observations show that epididymitis, especially when double, should not be regarded as a trivial and unimportant affection, and that the treatment of it should be prolonged until the effused matter is absorbed and all induration has disappeared, for if the disease be allowed to pass into and remain in a chronic state, permanent obstruction of the excretory duct is liable to ensue. It has been found that under careful treatment callosities obstructing the canal have disappeared at the end of many months, leaving the course of the semen free. M. Godard has related a case in which he had cured sterility from this cause that had lasted eighteen months.

The passage of the semen from the testicle may be prevented by congenital absence of the vas deferens. M. Gosselin examined the sexual organs removed from a man about twenty years of age. The funicular and inguinal portion of the vas deferens was wanting on the right side. The right testicle was healthy, but the ducts of the epididymis were gorged with yellow fluid which contained a quantity of dead spermatozoa. The testicle, vas deferens and vesiculæ seminales on the left side were normal, and contained abundance of spermatozoa. There were none in the right vesicula. John Hunter, in dissecting a male subject, found the vasa deferentia wanting on both sides. The testicles which were in the scrotum were sound and of good size. There are other instances on record of a double imperfection of this kind, the testicles being sound. In such a case the man would of course be sterile. Many years ago I made experiments on animals which were confirmatory of the observation that the testicles may be properly developed though a physical obstacle to the elimination of their secretion is present from birth; and that so long as these organs exist entire, the individual acquires and preserves all the marks of the male sex.[1]

The excretory duct of the testicle is liable also to be interrupted by tubercular deposits in the epididymis. It is well ascertained that this part is much more frequently the seat of tubercle than the body of the gland, and is often extensively diseased whilst the substance of the testicle remains sound.

Case IX.—A young man, aged twenty-eight, moderately robust, was under my care on account of large tubercular deposits in the epididymis of both testicles. Although the disease had existed seven years, and had softened down and suppurated, there was not the slightest indication of morbid change in the substance of the glands, which were of moderate size. His general health was good, and he had no symptom of tubercular disease elsewhere. He had fair sexual powers, but the emitted fluid was small in quantity and contained no spermatozoa.

This cause of sterility did not escape the searching inquiries of M. Godard. In a letter written to me in November, 1860, he remarks, "J'ai toujours constaté que les individus avec double affection tuberculeuse du testicule entraient en erection, pouvaient avoir des rapports sexuels, mais éjaculaient au plus une à deux gouttes de semence absolutement privée de spermatozoïds."

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[1] Treatise on Diseases of the Testis, first edit., p. 63.

The capacity for sexual intercourse may exist, though in diminished force, in extensive chronic disease of both testicles when the secreting structure is almost entirely destroyed, such as in old-standing strumous orchitis. This will not appear remarkable when it is recollected that coition may be performed for a time even after double castration.

CASE X.—A gentleman, aged thirty-two, of robust frame, married, and the father of two children, came under my care on account of strumous orchitis, producing great enlargement of the right testicle. His left testicle had been excised for a similar affection seven years before. He still continued sexual indulgence. The disease having resisted all remedies, I removed the remaining testicle. On examination I could find no trace of tubular structure, the enlarged organ consisting of a mass of lymph with scrofulous pus in the centre. There were no spermatozoa in the epididymis and vas deferens. He had intercourse with his wife a week only before the operation.

3. *Sterility from Impediments to the Escape of the Seminal Fluid.*—It is well known that a close stricture in the urethra so completely interrupts the passage of the seminal fluid, that in ejaculation it regurgitates into the bladder, where it mixes with the urine. In erection of the penis the urethra becomes narrowed, so that a stricture which offers but a slight obstacle to the flow of urine may under congestion be sufficient to impede the emission of semen. I have grounds for concluding that sterility from chronic stricture in the urethra exists to a greater extent than is commonly supposed, being in some instances little suspected by the patient himself. The semen not having been ejected, dribbles afterwards from the urethra as erection subsides, and so misleads the patient. As this is a condition which is in most cases remediable by the cure of the stricture, it is unnecessary to say more than to call particular attention to it as not an uncommon source of infertility. In describing Case VIII., I have mentioned that the absence of emissions in copulation led me to conclude that inflammation and abscesses near the prostate gland had occasioned obliteration of the ejaculatory canals, so that there was apparently a double cause for sterility, the excretory ducts also being obstructed. But sterility originating in a closure of the ejaculatory canals is a subject which needs further investigation. They must be liable to injury in lithotomy, and sterility might be the result of a bilateral operation.

Accoucheur physicians have informed me that, in seeking for the cause of sterility in their married patients, they have observed an absence of spermatozoa in the fluid removed from the vagina after sexual intercourse, and they have ascertained that the true cause of barrenness has in many instances rested with the husband. It is supposed that in men exhausted by early excesses the testicles do not secrete the emitted fluid consisting of the secretions of the vesiculæ and prostate. No doubt this is sometimes the case, for in several weak patients I have detected an absence of spermatozoa. In advancing atrophy of the testicles, before the capacity for intercourse is wholly lost, the glands cease to supply the essential element.

CASE XI.—A gentleman, aged forty-seven, a married man of robust appearance, consulted me on account of wasting of both testicles, with failure in sexual power. The wasting had been going on gradually for eighteen months. It commenced during a voyage at sea when he was separated from his wife. I found the testicles soft, and reduced to one-fourth their natural size. They were extremely sensitive. He still enjoyed connexion, but at long intervals. On examination of the fluid removed from the urethra shortly after intercourse, I could find no trace of spermatozoa.

But when the desire and capacity for intercourse are strong, I believe that spermatozoa are never absent from the ejaculated fluid, except from causes which I have described in this communication. When the testicles cease to secrete them, there is defective power of copulation, and the absence of spermatozoa is an indication of incompetency for marital duties.

Two important and delicate questions arise out of these inquiries: 1. Whether a man who has the inclination and power to copulate, but who is nevertheless sterile, is justified in contracting marriage—should such a person be condemned to celibacy? 2. Whether this condition is a sufficient ground for divorce?

1. That a man who is unable to fulfil the command "to be fruitful and multiply" is right in disappointing the hopes and perilling the happiness and perhaps health of a woman cannot, I think, be maintained by any casuist, and in some of the foregoing cases I have felt it my duty to give advice in accordance with this opinion.

It cannot be doubted that in women ready for conception frequent sexual excitement without impregnation is very likely to prove injurious to health. Dr. West mentions the occurrence of chronic ovarian irritation and chronic congestion of the womb leading to hypertrophy of the uterine substance and profuse bleeding from its lining membrane in cases where marriage is sterile.[1] It has been supposed that more important diseases of the female sexual organs, of a chronic character, have owed their origin to irregular and unfruitful excitement. In Case VII., the sterile patient, a medical gentleman, informed me that after six months of married life his wife suffered from some of those obscure symptoms of irritable cervix uteri called chronic inflammation, and he believes that his wife's troubles were caused by non-impregnation. I know also that the wife of another patient whose case is described in this paper, a fine healthy woman before marriage, has since been constantly under the care of accoucheur physicians.

2. The second question is one upon which a surgeon is scarcely called upon to pronounce an opinion. But I may venture to remark, that as sterility in women is not considered an adequate cause for divorce, so the man ought not to pay such a penalty for unsuspected unfruitfulness.

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[1] Diseases of Women, Part I., p. 55. Dr. Priestley remarks:—"It is highly probable that sexual excitement which is not followed by the occurrence of pregnancy leads in many cases to permanent congestion of the ovaries, and this may readily be lighted up into more active disease." (Clinical Lecture on Menorrhagia, Medical Times and Gazette, vol. i., 1863, p. 445.)

Art. V.

On Construction and Degeneration. By T. Clifford Allbutt, B.A., M.B. Cantab., Physician to the Leeds General Infirmary, the Leeds Fever Hospital, &c.

It is my purpose in the present paper to make some attempt to comprehend under terms of greater generalisation various physiological phenomena whose mutual relations seem little recognised. So strong is the tendency to establish types of morbid action, and to figure typical morbid products in physiology, that separation of the unlike is sought for almost exclusively, to the neglect of those transitional phenomena which mark the passage of the unlike into the like. Hence the almost universal belief in an essential difference between various types of disease, and in the permanent specific nature of the chief morbid products.

This tendency, however natural in those who practise physiology as an applied science, or however valuable for purposes of clinical record and instruction, is nevertheless eminently unscientific. Desirable as it is to force upon the attention of students certain broad features of morbid physiology—i.e. of the retrograde vital processes; desirable as it is for the rough and ready work of routine practice to mark out certain prominent lines of thought for average minds—it must, at the same time, be injurious to the better interests even of applied science to ignore the subtler affinities which weave all vital actions into a continuous web. By forcing the intelligence along definite grooves, many minds are entirely shut off from perception of the more intimate correlations of vital phenomena in their genesis and evolution. As long as a system of teaching goes on which elevates the term "pathology" into term of permanent scientific value, that is, as long as this word and many of its fellows are not recognised as merely convenient expressions, so long will partial conceptions of physiology prevail. Now what is for the true interests of the pure science must also be for its true interests throughout its applications. Setting aside then, as inadmissible, any such boundary-wall between physiology and so-called "pathology," we shall see that life partly consists in the elevation of matter to certain high degrees of molecular complexity, which elevation is necessarily and intimately connected with an opposite process of reduction. It is not that the one process simply follows upon the other, so that when construction has reached a certain degree, it is abandoned to destruction; but that the one process accompanies the other *pari passu*, the one being a condition of the other; so that the two are only separable in thought. If the integrating process be more vigorous than the disintegrating, then we observe some positive result, and call it evolution or growth; if the disintegrating process be the more active, we observe some inferior result which we speak of relatively as disease. Thus "pathology," or the rational observation of disease, is but inverse physiology. Those stages of life which, when observed in an ascending series, we include under the term "physiology," are identical with those which, when observed on a descending series, we comprise as "pathological." Pathological phenomena, therefore, become *physiological* when observed in their due subordination as parts of the scheme of living nature, and are only *pathological* when observed in a special relation to the physiology of man. Now, a classification formed on special relationships can at *best* be but a convenience—i.e., accidentally useful at a particular time, and must give way to a classification founded on general relationships. The contemplation of science under its special relations to man is perhaps only a little less mischievous in the present case than it is now known to have been in several others. I think that few readers will here accuse me of pretending to have discovered the truths which I insist upon; but it will perhaps be universally allowed that the prevalent tendency of our text-books is that which I undertake to oppose.

In returning to our partial definition of "life," as bearing upon the particular purpose of this paper, we find it spoken of as the elevation of matter through successive stages to higher degrees of molecular complexity. I believe that many persons who are not much given to accurate appreciation of their own conceptions, look upon living creatures as material constructions, *pre*-formed after certain patterns, and then set to work by the inspiration of an imponderable entity called "vital force," which keeps them going as bellows keep up a forge fire. This may be a somewhat rough way of representing their idea of the process, but it practically comes to much the same results. The less philosophical of these talk of the "breath of life" as if such an expression was not wholly figurative; the more philosophical talk of the "vital force" being strong or weak, just as they would talk of the current of a blast-pipe being hard or soft. We are, perhaps, obliged to accept as a condition of thought some antithesis between force and matter; but this ultimate position being once left behind, it becomes fecund of monstrous error to consider the agency of force apart from the molecular construction which forms its vehicle. We may speak of Force in the abstract as opposed to Matter; but from the moment we begin to contemplate Force under any modification whatever—as gravity, for example, or vitality—at that moment we are bound to consider the accompanying modifications of Matter, with which Force can only become conditioned, or for us existent. And this without any reserve. It will not do to take a certain aspect of Matter, as the physical, chemical, or vital, and, positing this, to use such assumed state as a universal factor within those particular limits; but the subtler modifications of molecular constitution within such limits must be known, if any true solution of problems is required. There may be a certain gross truth attainable by stating the difference of molecular state between bodies subject to chemical change only, and those subject also to vital. It may do to say, in a certain loose and general way, that "force acting upon matter" in the one state produces such and such results; or that acting upon it in the second state, it produces such and such other results. But we must go beyond this. Late researches in the simpler phenomena of chemistry show every day more strongly the necessity for a knowledge of the various degrees of molecular constitution; and in the more complex phenomena of life such knowledge is absolutely indispensable. An energy peculiar in kind and degree is manifested; but such energy can only rise out of and is wholly conditioned by equally peculiar molecular combinations. To take an extreme instance. We observe certain energies in the leaf of a tree, and certain energies also in the animal body, but these energies differ so widely in degree as to have seemed to many different in kind; yet we call the energy in both cases Vital Force. The difference is to be explained by demonstration of the difference, not in morphological only, but also in molecular construction; which molecular construction advances by small and continuous increments of complexity from the one point to the other. That medical phraseology is therefore to be condemned which bears constant reference to "deficiency of vital power," and the like: it is not without truth, but is that aspect of truth which has perhaps the less value in the search after positive knowledge; it errs in the preference of derivative to primary truths. This proem has been long, but I hope not tedious; for as the manifestations of force depend upon the coexistent material conditions, so my thoughts, be they more or less forcible, demand their peculiar foundation.

If, then, we observe, on contemplation of the scheme of living nature, that manifold and various energies are evident in their results; so must be the molecular groups manifold and various, which establish and modify such energies. Moreover, if we pass by all consideration of *direction of development*, we find that the massive results of these energies are of such variety that they may be classified in an ascending series of complexity. In such a series each member, massive or molecular, differs from its predecessor by some small increment of complexity, and one of the conditions necessary for the realization of each such member is found to be fulfilled in the pre-existence of the simpler member preceding it. Suppose any vibrations, say of light or heat, to be incident upon any member n (n being a group of molecules taken in a living body), the direction and quality of such vibrations will be changed in accordance with the given heterogeneous arrangement of the molecules of the group. Suppose the group not to be destroyed or disintegrated, the re-arrangement of the molecules will be along certain lines of higher complexity—the product of the pre-existing vibrations of n and of the incident vibrations. A new group will be the result, which we may call n'. The incidence of further forces, as evolution proceeds, transforms n' into a still more complex group, which we may indicate as n''; and so on to infinity. This is true for the whole scheme, which, the implicated disintegrations apart, may be looked upon as a progressive evolution of members in a series of increasing complexity. But the diversion of forces along ever new lines of action, although these lines sweep upward in vast curves towards a completion of the scheme as a whole, yet in process results in a desertion and consequent disintegration of parts. Such is the concomitant destruction with release of force, which is essentially implicated in progressive constructional processes. A series of intermediate and partial completions are necessary, as a means for the completion of the whole. The completion and death of the cell is necessary for the attainment of the higher animal structures, as their completion and death in turn is necessary for still higher evolution and consummation. We may observe construction and disintegration on a vast scale in the whole of nature, or on a smaller and mimic stage in the individual.

Now, taking the individual, for example, we seek a regular and orderly progression from lower to higher combined with the necessary destruction of parts: i.e., from the mass of jelly which precedes the ovum we pass to the ovum, and from the ovum through the whole gamut of increasing complexity to the highest structures. But the corresponding destruction may be of three degrees—1. That degree which is necessarily implicated in the upward process, and subservient to it by liberation of force. This is excretion; or rather, excretion is the ultimate step of such destruction relatively to the body. 2. That degree in which destruction is seen to be superior to the upward processes of construction; and, being more or less superior in activity to these, results ultimately in death of the individual or part. This degree we see in cancer, tubercle, &c., and in such diseases as diabetes; or 3. That degree in which some incident force disproportioned to the delicacy of vital movements brings about more or less immediate destruction. Such a result we see in sun-stroke, death by lightning, and the like; and in less severity in fatal congestions caused by intropulsion, and so on. In these latter cases, when death is not rapid, we observe phenomena of the second degree, as in slow deaths from infections; or again, if death be finally averted, we observe these phenomena limited as to place or intensity. The equilibrium mobile, though disturbed, in time regains its first degree of stability. In short, molecular destruction may be carried out with more or less rapidity. If the destruction be slow, a molecular group of high complexity may not be finally resolved into elements without gradual retrogression through each and every stage which marked the progress of its construction. Thus n''' may, according to the character of the destructive process, be at once resolved into its elements and into a simple molecular disintegration; or may gradually pass through the intermediate stages of n'', n', and n, on its way to such ultimate resolution. Now, in the slow disintegrating processes, which in the healthy body we know as "waste;" in these the retrograding substances probably in many cases pass through all intermediate stages of molecular complexity before ultimate resolution. In some parts—as, for instance, in the lungs—the process may be more rapid and direct. In disease, as the parts attacked are more or less high in the scale of complexity, and again as the disease is more or less rapid, so do the molecular groups pass more or less directly into an elemental state. In chronic abscess the retrogression is more orderly than in rapid gangrene. In the latter case the molecular cohorts betake themselves wholly to flight; in the former their retreat is conducted through a series of strategic recessions from post to post.

The two principal concrete conclusions which I hope to attain are these: Firstly, taking the processes known as normal, and also, on the other hand, those as anormal, and within these limits comparing phenomena, we shall see that all phases of anormal degeneration are strictly related, being correspondent or different stages in one movement of retrogression; and that the same is true also for those of normal degeneration. Further, that all processes of reduction in any organism are to be regarded as essentially alike, and to be included in one class, which may be called "degeneration." Thus the unfair limit set up between healthy and morbid will be thrown down. For example, we have a process of degeneration in a cellular mode going on in the human kidneys, and we have again a cellular mode of degeneration in chronic abscess. The fundamental relation between these two processes is not one of unlikeness, but of likeness. The three principal stages by which increasing molecular complexity is manifested to the eye, are: 1, the amorphous; 2, the cellular; 3, the fibrous. Within these limits we may infer that there is an infinite variation in molecular complexity. Groups of molecules by progressive differentiation form themselves into "compound atoms" or "units" of higher and higher elaboration. Thus, according to the lately-proved laws of energetics, we get higher and higher degrees of "potentiality;" i.e., higher and higher degrees of vital intensity, ranging upward from the indifference of the elemental state through the amorphous stage of vitalization[1] and the cellular stage up to the fibrous. As we proceed in complexity, so in accordance with all analogy we proceed in specialization. In the amorphous stage, energy is manifested in no special direction. In the cellular, a spherical arrangement of matter about certain centres marks a co-ordination of energy about certain special points. In the fibrous state we find a still further degree of specialization, and the lines of force, instead of acting uniformly about a given centre, act along a given diameter. One axis out of many is selected and specialized. The intermediate stages need scarcely be pointed out In cells of lower vital intensity the spherical shape is more or less uniform, and any existing want of uniformity is formed rather by collapse than by extension, as in the imperfect cells of unhealthy pus. In cells of higher vitality, however, as in encephaloïd cancer, or in ganglionic cells, we find many or few "polar" extensions, until we reach the final specialization of a single axis, as in a nerve-fibre. It now remains for us to point out the causes, or some of the causes, of such molecular processes. A weight hauled up to a certain height represents a corresponding potential value. Molecules built up to a certain degree of complexity represent in like manner an equivalent potential value. The position of the weight is due to an application of power from without; and so, in like manner, is the building up of a molecular structure. Now, the power in the latter case seems to be generally due to the influence of neighbouring matter undergoing similar transformation. By a process resembling induction, organized matter, at a certain stage of intensity, seems able to induce a like state in neighbouring matter more or less akin to it. It would seem that the passive matter, to accept this special influence of the active, must be of the same order or within a stage or two of the same state; that their vibrations may enter, as it were, into harmony. What seems probable *à priori* seems proved *à posteriori*. Nothing is commoner than in cases of cancer to find, on post-mortem examination, that an escape of cancer-cells—say, into the peritoneal cavity, or elsewhere—has set up little foci of cancerous action at points corresponding to the fall and rest of such cells. As the torula induces changes in the molecular constitution of certain organic mixtures, so by a sort of inductive process does organized matter in the body tend to propagate its own state in any neighbouring matter. The action of animal poisons again seems to be something of this kind. On this interpretation nothing can be simpler than an explanation of the progressive vitalization of matter in an animal organism—say in man. The ingesta are received and taken up by the lacteals, veins, &c., at a certain point of complexity—i.e., in the third or amorphous stage of vitality. Passing on, they are subjected to the inductive action of parts in the higher or cellular stage. In the higher animals this takes place to a great extent in parts specialized for the purpose, and known as ductless glands. Such glands might be known as "constructive glands," and they carry on an intensely active cell-generation, inducing the same in the matter submitted to their influence. On arriving at the true blood, we find matter elevated to the fibrous stage, where it is known as fibrine. Whether this condition be immediately precedent or immediately subsequent to tissue-formation is yet scarcely determined; but whether its formation be the first of the falling or the last of the rising degrees, or both, matters not here. The constructive glands we find, as we should expect, in the interior of the body, having genetic relations with the connective-tissue. The glands which we next observe—i. e., in the downward process—are, as we should in like manner expect, involutions and modifications of the external tissues. In them organized matter falls to its lower stages, that is, through the cellular to the molecular. These glands of excretion, as opposed to the "constructive glands," we may call the "degenerative glands." We may find, as we ought to find, an exact parallel to this process in the individual on searching the animal kingdom. We find in the lowest animals—the Protozoa—organized matter in its amorphous or earliest stage—granular or colloïd. As we rise from the amœbiform animals to higher protozoa, we find the cellular stage attained; and as we pass on through the Cœlenterata, we note a gradual achievement of the fibrous condition. So in the genesis of individuals. The lowest form is a colloïd gemma; a higher form is the cellular ovum, which ovum passes through many phases of cellular evolution before reaching a fibrous constitution. Once more: suppose we take the reparative process as occurring in parts of an individual. There is, say, a lesion of structure; in the healthy condition of neighbouring parts, the organised matter thrown out for repair passes with rapidity from its first or amorphous state through the cellular into the fibrous. In a less healthy state, the fibrous elevation is only partially attained, and much of the matter falls through the cellular stage in that state of degeneration known as "purulent." In a less healthy state still, the degeneration falls a degree below the cellular, and runs down into the amorphous or granular phase, as seen in the contents of scrofulous abscesses, &c. Here falling matter passes through precisely the same stages that it does in the cellular processes of the kidneys, mammæ, and the rest. In some cases, as in the mammæ, and perhaps the liver, the matter is caught as it falls, and again, by subjection to vitalizing influence, is re-edified and brought back to its former high state of potentiality. Lastly, let us look at the whole body, in health and disease. In the healthy body, nutritive matters are kept up to their due levels by orderly vitalizing processes. We find a firm blood-clot always; if any active reparation be going on, we find more than this in an excess of fibrine. But the unhealthy body leaving this height, or preserving it with difficulty, tends to fall into sundry forms of fibrous or of cellular degeneration—forms which vary according to the conditions of access, the rapidity of the process, and the peculiarities of the parts which may be the first to sink. I need not prolong my paper by enumeration of the obvious and multifarious illustrations of this position deriving from the phenomena of tumours, of cancer in its several forms, of phthisis, rheumatism, pyæmia, the generation of confervoïd aphthæ, and a host of other diseases.

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[1] I would not use so barbarous a word as "vitalization," could I meet with any other accurately synonomous.

Numberless and attractive are the bye-paths which open out around our present position. To follow out some of these in future papers may be a pleasure to come. Now I will content myself with a survey of what I have already investigated. It amounts to this—that products can only be called healthy or morbid in a strictly relative sense—viz., as bearing upon the greater or less perfection of a particular organism, or part of an organism; that precisely the same process which, when going on in the kidney, is called "healthy," that is, normal, when going on in the areolar tissue, is called "morbid," that is, anormal. Pathology, or the recognition and classification of products as morbid, is only the marking off of a special aspect of physiology; and in the course of teaching, this narrow doctrine has probably done as much harm as good. The survey of the organized world, as also that of the processes in individual structure, shows a gradual elevation of matter through stages of organic and organized elaboration; which morphological stages are probably the obvious results of higher and higher complexities of aggregation in the ultimate molecular groups or units. The manifestation of energy closely depends upon the attainment of these successive degrees of greater potentiality or tension. Coincidently with such elevation goes on also a corresponding falling process, which by destroying tension gives rise to energy, and this destruction manifests itself by repetition in a retrograde series of the stages which had previously marked the ascent. In the healthy body such rise and fall takes place in organs specialized to such purpose. In the unhealthy body degenerations take place in anormal situations, and are seen as products ranging from fibrous tumours down to rapid gangrene, and also as systemic cachexiæ, various in their phenomena as are the conditions which determine their manifestation. A careful classification of degenerative products according to their essential relations, in place of that founded on the more accidental facts of their genesis in place and time, seems to me the only way to a more rational appreciation of their import.

P.S. On a last reading of this paper, it has seemed well to say that I disclaim any purpose of treating physiological problems by an *à priori* method. In the present immature state of the science, such a method cannot be of much real value. My process has been, by previous and careful examination of the phenomena, to ascertain their most general law; and a knowledge of these phenomena I suppose in my readers. Certain laws of less generality will naturally come on in subsequent papers. The supposition, however, of "certain physiological units, intermediate in complexity between the chemical and the morphological, and modifying the mutual play of forces," though long familiar to my own speculation, will be found more clearly expounded in the last number of Mr. Herbert Spencer's "Principles of Biology," just come into my possession. While this paper was in the hands of the Editor, I also happened to read the very able essay in the October Number of this Magazine, entitled 'The Theory of Vitality.' It is perhaps not presumptuous in me to trace some resemblance between the speculations of the author of that essay and my own. I only regret that one who knows Bacon so well should have passed a judgment on Auguste Comte; which shows, as it appears to me, on the other hand, a deficient familiarity with the 'Positive Philosophy.'

PART FOURTH.

Chronicle of Medical Science

(CHIEFLY FOREIGN AND CONTEMPORARY).

HALF-YEARLY REPORT ON MICROLOGY.

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Part I.—Physiological Micrology.

NERVOUS SYSTEM.

The Ganglia in the Glands.—W. Krause writes: "There is a group of glands with efferent ducts, which furnish larger quantities of secretion only when their nerves have been excited in some way or other. These are the salivary and lachrymal glands, the anatomical structure of which agrees in all essential points. They agree also in that their nerves have always two different sources. The fibres, acting directly upon the salivary glands, pass off from the third division of the Trigeminus, having regard to the chorda tympani, and receive, in various ways, sympathetic fibres, from ganglia or plexuses; those of the lachrymal gland coming from the ciliary ganglion. In the glands themselves plexuses of pale fibres of Remak are seen, entering, running along, and branching with the arteries. Like all nerves of vessels, they carry single, narrow, double contoured nerve-fibres, which are probably for sensation. The larger stems of the directly acting nerves generally pass into the glands by the side of the efferent ducts, spreading round them in a wide-meshed web. These little stems show almost exclusively somewhat broader double-contoured fibres, and lie at some distance from the wall of the efferent duct in the loose connective tissue. The efferent ducts are everywhere composed of loose connective tissue, with numerous elastic elements passing lengthways or obliquely. In man, smooth muscle-fibres do not occur in them, with the exception of a thin layer on Wharton's duct (Kölliker), which I and all modern observers can always testify to. It is also certain that the forces which expel the secretions in question from the efferent passages are the same which promote them in the terminal vesicles of the glands. Wherever pale nerves are found on the efferent ducts, they appear to be destined for the arterial vessels in the neighbourhood, or on Wharton's duct for the small muscular fibres.

"In the following description of the course of the double-contoured nerve-stems, the parotid of the dog is particularly alluded to. The relations are here just the same as in the submaxillary gland of the hedgehog.[1] In the nerves on the efferent ducts ganglion-cells are found imbedded, indeed, beyond the gland substance. In the gland itself the nerve-stems divide and anastomose. Everywhere ganglia-cells are seen, in large numbers and having various arrangements. On the efferent duct and its primary branches they are large, the ganglia being often visible to the naked eye. Such an one was 1·5 mm. in length by 0·5 mm. in thickness. Mostly spindle-shaped, they are either so composed that the nerve-fibres, everywhere uniformly distributed, pass between the cells, whose number exceeds several hundreds, or the heaps of cells lie sideways in a concavo-convex lens-shaped figure on the nerve-stems. From these largest there appear all possible transitions to the smallest ganglia of 8-20 cells. Separate cells are often found in a linear row between the fibres. There spindle-shaped ganglia are also found, as well as the nearly globular ones, having many rays, and into which two or three nerve-stems enter, anastomosing, whilst just as many pass out of them again towards the periphery of the gland. In this way so dense an accumulation of nerve-fibres and ganglion-cells is made, that these plexuses are of the parts of the body richest in nerves. With the finer ramifications of the efferent ducts between the lobules the number of the individual ganglion-cells decreases; still at last there are found groups of 2–4 cells. From them the nature of the separate cells may be best studied. It offers nothing particular. An outer, nucleated, connective tissue sheath connects it with the interstitial connective tissue of the nerve-fibrils. The cells are commonly oval, the largest 0·04 mm. long (0·048 in the hedgehog), 0·025 broad; and their contents often show yellow fat globules. The nuclei are, without reagents, clearly visible as bright vesicles of, on an average, 0·01 mm. long (0·017 in the hedgehog), 0·006 broad. They have large, strongly refracting nucleoli (of 0·0045 in the hedgehog). At first sight the cells almost all appear a-polar, sometimes pear-shaped. In the larger ganglia nothing of their connexion with the nerve-fibres can be shown. In the smallest groups, however, it can frequently be shown that bi-polar ganglion-cells exist as in the intestinal ganglia. In man, ganglia-cells are found likewise, although much more sparingly, in the interstitial connective tissue of the gland-lobules. Research is here, however, rendered difficult, from the abundance of fat-cells in the connective tissue. Single groups of ganglion-cells are found in the parotid, submaxillary, sublingual, and lachrymal glands. In the submaxillary gland the relative sizes of the single ganglion-cells are the same as in the dog. In the lachrymal gland they are smaller, 0·034 mm. long and 0·028 mm. broad. In the parotid the length of the ganglion-cells was found to be 0·023-0·038; their breadth, 0·015–0·017: the length of the nuclei, 0·006–0·008; their breadth, 0·0037: the diameter of the nuclei, 0·001.

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[1] W. Krause, Göttingen Nachrichten, 1863, No. 18.

"In other mammals the arrangement is the same as in the hedgehog and dog. This has been shown by my researches hitherto, also, regarding the submaxillary gland of the dog, horse, sheep, calf, and rabbit. We must positively assume that, in all mammals, the arrangement will be analogous. Ganglion-cells are found also on the efferent ducts. On Steno's duct I have seen them in the dog and rabbit; on Wharton's duct in the hedgehog, dog, rabbit, horse, sheep, and calf. They are nowhere present in the vessel-nerves. It is also noteworthy that, in the little nerves in the glands of the animals investigated, are often found dichotomous divisions of doubly contoured fibres. In man they can be shown in the parotid and lachrymal glands."—*Zeitschrift für Rationelle Medicin*, 3rd Series, vol. xxi. part 1, p. 90.

On the Termination of the Nerves of the Muscles.—W. Krause has published three articles on this subject, from which we extract the following: "If each muscular fibre receives only one end-plate, it follows that the apparently varying abundance of nerves in the muscles must depend on the following circumstances. Very short muscles, of whatever kind they may be, will be richer in nerves than longer ones. Besides this, however, we shall find in a given square surface of muscular tissue as many more nerve-fibres as enter into it from the stems, and the lesser muscle-fibres are dependent on a single nerve-fibre of the stem. This latter comparison can only be made directly between about equally long muscles of the same animal. Through these considerations the known facts of observation—namely, the apparently greater abundance of nerves in the muscles of the eye, as well as in the short muscles in general, is completely explicable. . . . . The muscles of the eye have been particularly employed because it was presupposed that, in these short muscles, the course of the nerves would not have to be pursued to any great distance. It is best to observe, with a magnifying power of 450 diameter, an individual dark-edged fibril, leaving one of the smallest nerve-stems, and soon after apparently traceable no further; and it is best to select frogs of the largest size. It now results that the double-contoured nerve-fibrils, which have perhaps 0·002–0·004 mm. diameter, finally lose their medulla, passing into pale fibres covered with a nucleated neurilemma (end-fibres of Kölliker). These have at first a diameter of 0·0005–0·0012, and are connected with one or more end-plates, in case that by dichotomous divisions several end-fibres had proceeded from the original one, to one or more neighbouring muscular fibres to which the end-plates are attached. Sometimes, however, the branches altogether pass to the same muscular fibre. The length of the end-fibres, from the disappearance of the double contour to the entrance into the end-plate belonging to it, may vary from 0·01 to 0·4; their thickness finally amounts to only about 0·0005. If they are very short, in case the end-plate does not by chance appear in an exactly profile view, it very likely appears as if the darkly contoured nerve-fibril became directly attached to the sarcolemma, which in rarer cases even goes quite close to the end-plate, and then the relation is just as in mammals or birds. . . . . The end-plates of the frog are almost circular or oval discs of 0 015–0 02 mm. in diameter, and 0·0019 mm. thick. In the Sartorius somewhat larger ones, of 0 026 mm., appear. They consist of a membrane of connective tissue, which, on the edge of the disk, is attached to the sarcolemma, and of a finely granular, very thin mass, situated between the connective-tissue membrane and the sarcolemma. The insertion of the end-fibres is mostly found to be in the centre of the plate, and the neurilemma of the pale contoured nerve-fibre passes over continuously into the connective-tissue membrane of the end-plate. The latter generally have no nuclei, or one at most; and it is often a question whether we ought to consider the nucleus at the ends of the nerve-fibre to be of the end-plate, or reckon it with the neurilemma of the end-fibre. The nuclei themselves behave alike to the different reagents; in fresh preparations they are to be shown by the addition of acetic acid; and they have about 0·007 mm. length, by 0 004 mm. breadth, and 0·002 mm. thickness.

"The end-fibres are perhaps not to be considered as axis cylinders, covered with neurilemma; but, like all pale nerve-fibres, they contain the fatty element of the double-contoured nerve-fibres, only in less relative quantity, as may be inferred from their optical bearing in different circumstances. As to the Remak fibres of the intestinal canal, the proof is most easily pursued by placing pieces of small intestine of the larger mammals in a 33 per cent. potash solution. Physiologically, therefore, it is very unimportant whether the pale end-fibres are somewhat shorter or longer. For the anatomist, however, there is a difficulty in their various lengths, inasmuch as we can never know how far we shall have to pursue an end-fibre before we arrive at its end-plate. The end-plates of the frog are generally more difficult to find than those of the mammals, because of their want of nuclei and smaller diameter. Apart from morphological differences, there is one of importance—viz., that many muscular fibres receive at least several double-contoured end-branches, and consequently end-plates. On this point all the existing observations (of Wagner, Kühne, Kölliker, Margo, and Reichert) agree. Besides this, it has been determined by Kühne and Kölliker, that several pale end-fibres proceed from a double-contoured fibril (Kölliker represents six of them); and, after a short course, these may again divide, so that two end-plates hold to the same muscular fibre. Probably, however, many muscular fibres receive even more, perhaps from 4 to 6 end-plates. This important difference of the nervous extension of the frog's muscular fibres may probably be explained by its development-history; and perhaps it is of importance that the muscular fibres of the animal have such considerable thickness in proportion to the bodily size of the animal, as well as to the diameter of the end-plates belonging thereto, that they, within the contractile substance, possess nuclei, &c. From this it is inferred, that the muscular fibre of the frog represents, as it were, several muscular fibres of the mammals. It may be considered, in the same sense, that in the pectoral muscle of the pigeon the thicker muscular fibres contain nuclei in the contractile tissue, and not the thinner ones (Rollett), and also the occurrence of the nerve-buds of Kölliker in the muscle of the skin of the breast of the frog, which, at least in the months of March to June, can be very easily shown, and testifies strongly to the fact that, even in the largest frogs, individual muscular fibres can enter into further developments, increase by longitudinal division, &c. As to the nerve-fibres which form the nerve-buds, they obtained their considerable transverse diameter only just before their entrance into the nerve-bud; in the plexus they showed the ordinary diameter of fibres which have proceeded from first or second divisions of the Reichert stem-fibres."

The results of my investigations hitherto on the termination of the nerves of muscle may be thus briefly stated:—

1. The motor nerve-fibres terminate in the striped muscles with *motor end-plates*.
2. In men and mammals, birds and scaly reptiles, the end-plates consist of a connective-tissue membrane, with from 8 to 20 nuclei, a finely granular mass, and pale nerve-fibres terminating in a club-shaped way. They appear as round flat disks lying upon the sarcolemma.
3. The cross-striped muscular fibres, even of the longest muscles of the extremity in mammals, are structures from 2 to 4 centim. in length, spindle-shaped, at the ends more rarely rounded or branching; larger than the smooth muscular fibres, they are otherwise perfectly similar to them in form. An isolated striped muscular fibre, with a magnifying power of twenty-five diameters, looks exactly like a smooth one with a power of two hundred and fifty diameters.
4. Each striped muscular fibre, in mammals, receives only one end-plate about the middle of its length. The diameter of the end-plate amounts to about one-third of the circumference of the muscular fibres which they enclose. The nerve-fibrils entering the muscles, by frequent divisions, provide several, perhaps ten, muscular fibres.
5. In new-born mammals the end-plates are completely formed.
6. After cutting across nerves, the pale terminal fibres in the interior of the end-plate degenerate, whilst the finely granular substance and the nuclei remain unchanged. The degeneration begins in the doubly contoured primitive fibres, which singly pass off, and progresses from these towards the stems.
7. In frogs and fishes the end-plates are smaller, and have but few nuclei. The muscular fibres of these animals receive more than one end-plate, probably from 4 to 6, to which pass pale nerve-fibres covered with neurilemma.
8. In invertebrate animals motor end-plates are also found.
9. The ends of the muscles, to a length of from 1 to 2 centimetres, are without end-plates. Such nerveless pieces, taken from the living animal, contract, in consequence of external mechanical or chemical influences. The muscular fibres are therefore independently irritable.
10. The contraction of the muscular fibres is effected, on the part of the motor nerves, probably by an electric stroke. For the motor end-plates are

distinguished principally only by their small size from the electric end-plates, which compose the electric organs of the torpedo. The discharge of the motor end-plates possibly produces the so-called positive primary fluctuation of the muscle stream in the excitation of the nerves of voluntary muscles.

11. The muscles also contain nerves branching with the vessels, which convey, for the most part, pale, probably sensitive fibres, destined for the muscular structure of the vessel, and sparingly double-contoured.

12. The muscular fibres of the higher vertebrate animals have the value of a single cell. The contractile cellular contents consist of a substance more or less strongly refracting, on which the cross striping depends. Both substances are found in a solid state, and compose the fibrils of the muscular primitive bundles. The fibrils are separated by a liquid intermediate substance, which, as a rule, contains fat-drops.—*Zeitschrift für Rationelle Medicin*, 3rd Series, vol. xxi. part 1, 1864, p. 87.

VASCULAR SYSTEM.

On the Vascular System of the Penis.—As to the communication of the arteries with the spaces of the cavernous tissue, the injections of C. Langer in the first place confute the opinion which J. Müller had, indeed, himself already abandoned, that the helicine arteries open into those spaces. But the author does not even allow them to be blind diverticula of the arteries. Many of them appear as imperfectly injected loops or narrow knots, and those in which this could not be proved, do not, nevertheless, differ in anything from those in which it can be developed. If the imperfectly injected framework could be stretched, one might sometimes pursue the continuation of the vessel, now through a line of injection, then through the peculiar structure of the coat of the empty vessels. The unfolding of the tortuous vessels was not easily done, and if successful they immediately, after remission of the force employed, resumed the loop form. From this Langer concludes, that even before death the branches had been bent, and that they continued in the death rigidity. Sometimes the injection was found in the framework in an apparently equally thick course, bent hook-like, and diverted with its crooked end towards an edge of the trabeculæ.—*Zeitschrift für Rationelle Medicin*, 3rd Series, vol. xix. part 1, p. 125.

LYMPHATIC SYSTEM.

The Lymphatic Radicles in the Kidneys of the Mammalia.—C. Ludwig and Th. Zawarykin, in a brief introductory note, state that the lymphatics of the kidney arise from the connective tissue described by Bowman, Goodsir, and Henle, that is, out of the spaces, which, in all parts of the cortical and medullary substance, lie between the blood and urinary vessels. These spaces cannot only merely be injected from the larger lymphatics by taking certain precautions, but they are also always and equally distended when, in the living kidney, the larger branches are filled with lymph, and an œdematous swelling has been produced. The urinary and blood vessels are imbedded in the root spaces of the kidney-lymph, just as is the case with the seminal and blood vessels in the like-named spaces of the testicle. The transference of the lymph from the roots into the little stems is also effected in the kidney as in the testicle; this can be especially well shown in the lymphatics which take their origin in the capsule. Here also the layers and fibres of the connective tissue serve as auxiliary means to conduct the finest chasms into closed "vessel-courses." The difference in this respect, between the capsule of the testicle and that of the kidney, seems particularly to be due to the lesser thickness of the covering of the latter. Inasmuch as, in consequence of interruptions in the currents of urine and blood, the lymph-work of the kidney swells considerably, and the whole kidney is greatly enlarged, and inasmuch as in such cases also the primitive lymph-spaces may be filled with blood, and lymph-corpuscles with fat and pigment, the lacunar system, which, after our experiments and injections, we must consider as the lymph-roots, has not been overlooked by the pathologists. Thus, among others, is Beer, who, in his monograph on the connective substance of the human kidney (Berlin, 1859), gives representations of these lacunæ, filled with the matter above mentioned, but there is no question that they are in connexion with the lymph-vessels.—*Zeitschrift für Rationelle Medicin*, 3rd Series, vol. xx. p. 189.

On Connective Tissue—Contributions to the Anatomy of the Origin of the Lymphatics.—Tomsa makes the following observations: If the lacunæ of connective tissue were injected by means of punctures or from the lymphatics, the connective-tissue corpuscles were never suspended in the injection mass itself, but generally in close vicinity to the cavities, indeed very frequently as if intergrown with the coat of the pale, connective-tissue limit. If the nuclei yet showed themselves imbedded in larger number within the bundles of connective tissue, then it was to be considered that what is called a coarser bundle may be divided into thinner partial bundles, to the interstices of which the connective-tissue corpuscles stand in an equal relation. Whilst an abundant injection of the cavities unfolds these relations, the connective-tissue corpuscles are rather heaped up together in empty lacunæ, as if imbedded in the interior of the connective tissue of a coarse bundle.—*Wien Sitzungsberichte*, vol. xlvi. p. 324.

CARTILAGE.

Formation of the so-called Intercellular Substance of Cartilage, &c.—Dr. Beale has written to prove the following propositions:—" 1. That the so-called intercellular substance of cartilage and other tissues is never formed independently of *cells*, or, more correctly, masses of living or germinal matter. 2. That the intercellular substance does not possess formative power, and that physical and chemical changes alone take place in it. 3. That in all cases the masses of germinal matter are continuous with the so-called intercellular substance, and that the latter was once in the state of germinal matter. 4. That in the development and growth of these tissues, the pabulum becomes (*a*) germinal matter; the germinal matter becomes (*b*) the formed material (intercellular substance), which accumulates and gradually undergoes condensation."—*Quarterly Journal of Microscopical Science*, Oct. 1863, p. 95.

MISCELLANEOUS.

The Blood-Corpuscles of Man and Beasts.—Dr. H. Welcker, of Halle, has a long paper on their size, number, volume, surface, and colour. As to their *size*, he says that, "relying upon Harting's statements, Cryptobranchus is generally supposed to be the animal which has the largest blood-corpuscles. The author, in thirty measurements, found them on the average larger than did Harting in ten measurements (in which he agrees with the results of the old measurements of Van der Hoeven), but yet, for the Proteus, his measurements gave far more considerable dimensions; so that, he says, undoubtedly the latter animal has larger blood-corpuscles than the Cryptobranchus. He says their larger size may be seen at once with the microscope, without taking their comparative measurements. The author's averages were for the Cryptobranchus japonicus 0·051 mm. in length by 0·032 mm. in breadth, and for the Proteus anguineus 0·058 mm. in length by 0·036 mm. in breadth, of the dried corpuscles."—*Zeitschrift für Rationelle Medicin*, 3rd Series, vol. xx. part 3, p. 257.

On the Markings on the Bodies of the Spermatozoa of some Mammals.—Dr. Valentin publishes a paper on this subject, of which the following is a part. He says: "The investigation of the semen of the bear with the old lenses of Schiek, and the employment of yellow lamplight, had shown me, some time since, a series of internal globular formations in the body of the spermatozoa, which, according to the stand-point of that time, I thought I could only interpret as organs. Schiek's new object-glasses, and a microscope made with great care by the late Kellner, furnished with orthoscopic eyepieces, were at my command when I again received fresh semen of the bear. These more perfect means of examination soon showed me that, even in daylight, more markings could be made out in the bodies of the dried spermatozoa than I had seen the first time. The comparison of the pictures which the above-mentioned lenses by Schiek and Kellner, as well as the best microscopes of other makers, led me to the conclusion that the dried spermatozoa of the bear formed one of the best tests for the defining power of a microscope. The testes of the second bear made use of by me had been lost, by an unfortunate accident, after having preserved but few preparations of the spermatozoa. Only professional colleagues who visited me could, therefore, be convinced that the bodies of the spermatozoa of the bear had such an appearance as one could not have expected from the almost universally-assumed simplicity of the seminal elements; inasmuch as, last spring, I again received a testicle of the bear with mature semen, from which I could make a store of preparations, of which I could give some away. The animal, five years old, from which they came, had been shot in the last half of April, whilst the time of copulation of the bears kept in the bear-garden of Berne falls in June and July. The two testicles, one of which still remained in the cavity of the belly, were of very unequal size. Only the vas deferens and the epididymis of the larger one contained perfectly-developed spermatozoa.

"We see the markings of them in many quite fresh spermatozoa. But if we have preserved the mobile seminal elements of the bear or other creatures in a dried condition, they may, even after many years, serve to demonstrate these shadowy bands. I caused very fine streaks of the semen immediately collected or obtained with a brush from the vas deferens, or from the epididymis, to be dried on a very thin glass; and I preserved this separately, or after having fastened it upon a larger glass plate. The former has the advantage that, at a future time, we can immediately undertake the examination on the side on which the semen is found, and are not, therefore, disturbed by the injurious optical influences of a covering-glass. A larger quantity of semen collected together, or even a larger drop dried up, gave less favourable pictures. Only the spermatozoa dispersed on the edge, or at some defective part, are then preserved in a serviceable condition. The principal mass is then not transparent enough to allow us to see all the details.

"The drying-up process has proved of great service to me in the case of spermatozoa of the most various classes of the vertebrate and of the invertebrate animals, and also in those of the cryptogams. The only exception I have hitherto met with concerns the barrel forms, so peculiar and immovable, and set with hair, of the semen of the river crab, which we are accustomed to take for the perfectly mature spermatozoa of this kind of animals. The inner parts of the central body here become more indistinct when dried up. I know no better means of convincing oneself of the presence of a ciliary membrane on the tails of the spermatozoa of the tritons, than to undertake the investigation of dried specimens. The several parts of the ciliary layer separated by sections may, after many years, be recognised with a moderately good microscope.

"The body of the spermatozoa of the bear and other mammals, to be referred to afterwards, contains in succession three streak-like, roundish formations, which, for brevity's sake, we will indicate by the names of the front, middle, and hinder band, because the first lies on the foremost end, the second in the middle, or somewhat distant from the former, and the third lies quite behind, near the tail. The middle band may also, as we shall see, be exceptionally manifold. Each spermatozoon of the bear immediately shows a middle band, the hinder one placed just before the root of the tail. The one in front often shows itself only as a dark shade, or a bluish-black globular formation; frequently also, on the other hand, as a broad crossband, close to the anterior edge of the body. Inasmuch as it exactly follows this, it is not always straight, but more or less hollowed out in front. Many spermatozoa, when seen, leave us in doubt of the real existence of this foremost band. The whole then has the appearance of a mere streak of shadows, or of an indefinite, somewhat darker object. These pictures present themselves in the complete spermatozoa, as well as in those which have lost their tails. If we use No. 1 eyepiece and object-glass, No. 2 of Kellner's microscopes, the middle and hinder, and often also the front band, always appear sharp and of bluish-black colour. Under all other microscopes with which I have examined these formations, the bands appear with less sharp edges, brighter, and rather bluish-grey. The advantage which Kellner's instrument, in our case, affords, does not consist in the orthoscopic eyepiece, but in the system of lenses of the object-glasses. If, for instance, I change the former for the largest aplanatic, or the next stronger eyepiece of Schiek, then the appearance of the crossbands becomes even more beautiful than when making use of the orthoscopic eyepiece, because those eyepieces of Schiek's have a larger field of vision, and consequently also more brightness. The necessary subduing of the light, on the other hand, with the diaphragm, makes all the formations appear more distinct than in too bright a light. The union of those eyepieces by Schiek with the more recent object-glasses of the same artist, shows the three bands in a satisfactory way, although only bluish-grey, and with not quite sharp edges. This picture is, next to that of Kellner, the best which has ever come before me.

"Later observations have taught me that the spermatozoa of the bear are not the only ones which show the crossbands, with the assistance of the lenses of Kellner, Schiek, Nachet, or Hartnack. The middle crossband, first of all, struck me, in some dried spermatozoa of the ram, during a lecture demonstration. It is often narrow in proportion. Faint shade-specks, or very narrow crossbands, are frequently found on the front and on the hinder ends of the body. These are, however, much paler, and of fainter colour than in the bear. A close study of the dried-up spermatozoa of the dog generally shows most easily the spots on the hinder end of the body, and indeed as, in proportion, tolerably dark masses. We see also in the bodies of many a bluish middle band; in others there is also a broader bluish-black streak on the anterior end of the body."—*Zeitschrift für Rationelle Medicin*, 3rd Series, vol. xviii. pp. 217–20.

Contribution to the Anatomy of the Little Rods of the Retina.—Dr. Schiess, of Basel, writes thus:—"From my observations, therefore, I proceed to the confirmation of Ritters' view, that the rods of the frog have a threefold composition, consisting of an envelope, medullary contents, and a central fibre; that the medulla of the rod certainly has qualities like that of the nerve, and possesses considerable toughness. The opinion that the central fibre is an artificial product has been already sufficiently refuted by Ritter and Manz; and when Braun asserts that the rent in the rod envelope represents the thread of Ritter, we may reply that undoubtedly such longitudinal rents occur in the envelopes, but that it is not difficult to distinguish them from fibres; there are still other sources of deception, such as when two envelopes partly cover each other, then the side contour of the one may be easily taken for a fibre in the other, but even this source of error can be easily avoided by precise examination, and the isolated appearance of the rod-fibres in connexion with the rod-nucleus, appears to me an irrefragable proof for the constancy of this formation.

"It is easy enough to draw a parallel between rods and nerve-fibres, and to consider the central fibre as a formation homogeneous with the axis cylinder, to which Manz also points. But there does not appear to me to be any proof of the nervous nature of the rods in this analogy.

"I have tried also to find the central fibre in the other classes of the vertebrate animals, and, particularly in the fowl, I have found just the same appearances; the medullary body also can easily be shown in this animal. Also, in the retina of the goat, I have seen, changed like a vesicle, a fibre, appearing out of the rod. In fishes there are also similar appearances.

"I have now only to say a word on the division of the rods into inner and outer members, as Krause takes them for the rods of man and the frog, Braun for those of the frog. Braun, in this, takes his stand upon the difference of the carmine colouring; I have repeated the experiment, but, like Manz, I cannot convince myself of the existence of the inner members. I take these inner members to be nothing else than the medullary cones divested of their rod envelope; I believe, that in certain circumstances, even an insignificant pressure is sufficient to strip back the envelopes over the medulla, and in this way to expose this part, which is more capable of imbibition, to the arbitrary selection of the carmine. That the medulla is very easily coloured, whilst the envelopes retain their yellowish tint, I have easily been able to show. What bearing this may have with regard to man I do not venture to decide without investigation of fresh normal eyes."—*Zeitschrift für Rationelle Medicin*, Third Series, Vol. xviii. Parts 1 and 2, p. 134.

PART II.—PATHOLOGICAL MICROLOGY.

NERVOUS SYSTEM.

Atrophic Degeneration of the Posterior Columns of the Spinal Cord.—Professor Friedrich, of Heidelberg, in his concluding paper on this subject, gives the anatomical examinations of three cases in which, attacked by typhoïd fever, the symptoms were as like as were the lesions found in the spinal marrow after death. Cut across, this organ appeared atrophied at the posterior part, especially in the dorsal and lumbar regions. "In place of the nervous cylinders was substituted a very delicate connective-tissue of fine fibres, parallel to the axis of the cord, and formed, at least in part, by the shrunk envelopes of the primitive nerve-tubes. The elements of this fibril-tissue were immersed in a greyish fundamental substance, finely granular, becoming transparent by the action of acetic acid, and then showing a pretty good number of round or oval nuclei, of medium size, and generally containing from two to four nucleoli. The number of normal nervous cylinders had diminished in proportion to the abundance of these abnormal tissues; they moreover presented all the characters of simple atrophy; their medullary substance had undergone a progressive diminution, and the envelope of the fibre, in fact, alone subsisted. On the other hand, one nowhere found the least trace of fatty degeneration. The degenerated substance contained also considerable masses of rounded or oval starchy corpuscles, which offered the characteristic reactions with sulphuric acid and iodine. There was no alteration in the antero-lateral columns, or in the grey substance of the spinal marrow. Yet, in two subjects, the alteration had extended to a part of the lateral columns. The alterations in them were like those described. The posterior roots of the spinal nerves were evidently shrunk, atrophied, flattened, hardened, whilst no change existed in the anterior roots. Microscopic examination showed in the interstices of the nervous fibres an abundant development of resisting wavy connective-tissue, in which, by addition of acetic acid, one perceived an abundant proliferation of oval or elongated, fusiform nuclei. The nervous elements were notably everywhere shrunk, thinned; their medullary substance was coagulated in the form of lumps of various size, yet without presenting the characters of fatty degeneration."—*Archives Générales de Médecine*, March, 1864, p. 302.

URINARY ORGANS.

On Changes in the Kidneys from Lead Poisoning.—Dr. Lancereaux argues that in the elimination of this poison a change altogether peculiar is produced in the kidneys—a true Bright's disease. The author alludes to cases of fatty degeneration of the kidneys in poisoning by phosphorus, by the acid nitrate of mercury, and by sulphuric acid. In the latter case, he observed that "the cortical substance of the kidneys, of about normal consistence, was dotted with red. In the field of the microscope, there was absence of fat in the tubuli, but destruction of most of the epithelial cells, which formed a finely-granular greyish mass. The walls of the canaliculi appeared intact, but the interstitial connective-substance was altered, and in course of proliferation (nephritis)." The author says that the renal affection coexistent with lead poisoning is not a simple coincidence—that he has always found its characters very analogous, if not identical. He thus sums up the results of his investigations in three cases: —There was, 1st, an advanced stage of lead poisoning, with cachexy; 2nd, a lesion of the kidneys, always characterized by inequality of their surface; atrophy of their cortical substance; hyperplasy of the connective-substance; destruction or even the disappearance of the cellular elements, with albumen present in the urine. This renal affection only succeeds to an advanced stage of lead disease.—*L'Union Médicale*, No. 150, Dec. 15, 1863, pp. 513–22.

BILIARY ORGANS.

The Porous Channels in the Membrane of the Cells of the Rete Malpighii in Man.—According to Dr. Otto Schrön, of Turin, if we treat with glycerine very fine sections of an epithelial cancer which has been hardened in alcohol, and examine it with a magnifying power of about 500, there may be seen in the membrane of the cells, from out of the more or less thickened Malpighian mucous layer, a fine radiated striping; that is to say, bright lines alternate with dark ones, both of which stand vertically upon the cell-membrane, and appear so much the more distinctly as we accurately examine the cell-membrane in profile. If we then look at the thickened membrane of such a cell on the surface, placed in the dark field, we find it interrupted by bright points standing at a determinate regular distance from each other, or we find the bright ground set with dark points, which at the edge of the cell pass into indistinct stripes. If we observe larger groups of cells, and view the edge of the cells, the dark lines of the one membrane passing over into the dark lines of the membrane of its neighbouring cell appear at the points of contact of two cells. In the cells also from the Malpighian layer of the sound skin the striping described is to be observed, but in it the author was never able to follow the streaks beyond the inner boundary of the cell-membrane, whilst in the cells of epithelial cancer the stripes appear to be continued by the spots through the cell-contents as far as the envelope of the cell-nucleus. The streaks described were found even more distinctly than in the cells of the external skin, in the cells of the mucous membrane of the lip and palate. The author believes stripes of the cell-membrane to be the optical expression of the existence of porous channels, such as exist, for instance, in the epidermis of Petromyzon. According to him, it is probable that the streaking is something normal, inasmuch as it was found equally developed in the cells of the skin of six different subjects, even in a fresh state, and still more distinctly when the preparations have been treated with certain chemical substances. With those pathological processes in the skin which are distinguished by a plastic hypertrophy of the Malpighian layer, we find the porous channels in a very high degree of development. They appear most marked in the middle portion of the Malpighian layer, whilst in those cells which are near the papillary, and vertically upon the same, they are to be seen with difficulty. Towards the horny layer the porous channels completely disappear.—*Untersuch. zur Naturlehre d. Mensch. v. Moleschott*, ix. 1, p. 95; Schmidt's *Jahrbücher*, 1864, p. 155.

Melanosis and the Melanotic Matter.—An interesting case of Professor Monneret's, of melanosis of the two lungs, of the peritoneum, mesentery, sub-peritoneal cellular tissue, mesenteric glands, and small intestines, is reported. The professor has said that melanosis is a true product of secretion, formed by a solid granular black matter, *identical* with the choroïdal pigment. "Anthracosis, or carbon of the lungs, is the black matter which normally exists in the pulmonary tissue and the bronchial glands, where it often accumulates in tolerable quantity, under the influence of disease." Yet, in the present case, M. Monneret has not refused the name of melanosis, although chemical analysis and microscopic examination agree in showing that the melanotic matter was of a carbonaceous character. The case was that of a man who had been laid up with a cough for a year or more, then hemoptysis, dyspnœa, dropsy, and cyanosis. After death, the lungs were found superficially to be of an intense black, and sections showed all the pulmonary tissue to be equally impregnated; moreover, a black matter like Indian ink could be squeezed out. It did not exist in the bronchi, and was not accumulated in any form. There was no trace of tubercular matter. Microscopically, with a power of 100 to 200 diameters, the black matter appeared as a granular layer, the granules being free and not enclosed in cells, like the pigment matter of the skin of the negro, or that of the nipple or scrotum; like the pigment of the choroïd, the iris, and ciliary processes. This, then, was a case of melanosis by morbid accumulation, in the midst of the pulmonary tissue, of carbon *of composition.*—*L'Union Médicale*, No. 144, Dec. 1, 1863, pp. 417-25.

Malignant Tumour of the Left Gluteal Region.—"This tumour originated from an injury received by a fall nine years previously. It weighed upwards of nine pounds. The patient was a man of middle age, of a strumous aspect. The glands in the groin became affected, and the pain was intolerable. The tumour was examined, after the patient's death, by Mr. Collis, who furnished the following report:—'It presents undoubted marks of genuine cancer throughout every portion. At the hard part, pointed out as the original site of injury, I find, along with much fibrous tissue, an abundance of what are commonly called cancer-cells, of the type generally found in scirrhus. In the portion which is of a reddish colour, and in texture somewhat like erectile tissue, the cells are very various in size—the majority are the ordinary lymph-cell, or cell of connective-tissue; a comparatively smaller number are cancer-cells; and I find a great many intermediate forms between these two extremes. In the large subcutaneous loculi, filled with a soft granular matter, I find plenty of fat and oil, and a quantity of very large cancer-cells, evidently of the acute or encephaloïd type, and recently deposited. Here and there throughout the tumour there are spiculæ of bone; in the erectile portion there is a quantity of blood and much fibrous tissue, with some yellow elastic fibre. To sum up, this was evidently a scirrhous tumour of long standing, the result as cancer generally is, of an injury. It slowly progressed, until near the end, when a rapid deposition took place in the lax areolar tissue, outside the original seat of the tumour. The state of parts in what I call the erectile portion proves to my mind incontestably, that all these varied forms of morbid cells, however widely they differ in size and outline, have a common parentage—namely, what Virchow calls *the cell of connective-tissue*, and what I call by the more familiar name of *the lymph-cell*, for in this part of the tumour we find cells of every variety as to size, from the small and apparently healthy lymph-cell up to the monstrous and unshapely cancer-cell—a morbid alteration of natural structures.' "—*The Dublin Quarterly Journal*, vol. xxxv. pp. 466–7.

MISCELLANEOUS.

On the Regeneration of Tendons.—The substance which again connects separated ends of tendons is, according to Jobert de Lamballe, developed from the discoloured coagulations of the blood effused between the stumps; the author seems to lay down an immediate transition of the fibrine fibres into connective-tissue fibrils. On the other hand, Demeaux observes, in a preliminary communication to which a more detailed one is to follow, that the regeneration might take place also without blood effusion, that in this case the connective-tissue sheath of the tendon exudes the plastic lymph requisite for the purpose of the regeneration, and had the same bearing to the tendon as the periosteum has to the bone.—*Comptes Rendus*, March 31st; *Ibid.*, April 21st.

Fatty Degeneration of the active Elements of the Liver, the Kidneys, and the Muscles of Animal Life in Poisoning by Phosphorus.—Dr. Lancereaux says: "Fatty degeneration of several organs, and alteration of the composition of the blood, producing a kind of hæmorrhagic diathesis, are the anatomical disorders consequent upon the absorption of phosphorus paste. These lesions may be summed up as partial or general fatty degeneration of the active elements of the liver, the kidneys, the heart, and voluntary muscles, without any concomitant alteration of the cellular or tubular elements of the brain-centres, or nervous cords; numerous hæmorrhagies, without apparent alteration of the blood-globules, or the presence of foreign matter in the blood; diminution in the quantity of fibrine." To this the author adds finally, that "there exists a manifest affinity of causation between phosphorus poisoning and degeneration of the hepatic cells, of the epithelia of the kidneys, and of the muscular fibres of animal life. This degeneration depends on the direct action of phosphorus on the histological elements in question; it is not the consequence of either a lesion of the nerves, because the nervous system remains intact, or of a modification of the blood, because this liquid shows no appreciable change other than a diminution of fibrine coming on in the last stage of the disease."—*L'Union Médicale*, Sept. 1st, 1863, pp. 71 and 73.

HALF-YEARLY REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIENE.

By Benjamin W. Richardson, M.A., M.D.,
Senior Physician to the Royal Infirmary for Diseases of the Chest.

I. Toxicology.

On a Source of Error in Marsh's Test for Arsenic.—M. H. Gaultier de Claubry furnishes an important communication on a serious cause of error in searching for arsenic by Marsh's process. He commences by stating that the very delicacy of the method of Marsh is an actual reason for the dangerous and even fatal conclusions to which it may lead the experimentalist. In fact, says he, if it be preferable that a criminal should escape justice, rather than that an innocent man should be punished, it is not less true to assert that it behoves the public seriously to study every source of error which might lead to the one or the other of these results: science points out the means of guarding against them.

On the one hand, as regards the causes of error, he quotes from numerous works upon the testing for arsenic, all that has been done in regard to the decomposition of poisoned substances by means of sulphuric acid; and the necessity, proved by the Academy of Sciences, of interposing between the apparatus for the production of hydrogen and the tube where the arsenic rings are obtained, some amianthus intended to retain every substance which might be carried over with the arsenic by the current of gas.

On the other hand, he points out a cause of error arising from the employment of concentrated sulphuric acid for the liberation of hydrogen. This may produce hydro-sulphuric acid (as MM. Fordos and Gélis have shown), by means of which the arsenic becoming transformed into sulphuret, can no longer appear, as nascent hydrogen does not decompose this product.

M. Blondlot has proved that the formation of the sulphuret may take place during the destruction of poisoned substances by means of sulphuric acid, and that by the aid of ammonia it is possible to extract from the product a certain quantity of this compound.

Previously it has always been the practice to treat the complex organic matter, designated as "sulphuric carbon," with weak proportions of nitric acid or aqua regia, for the purpose of restoring to a soluble compound the arsenic which the carbon has reduced to a metallic or metalloïdal state; but its transformation into a soluble compound being much easier than that of the sulphuret, it is to be understood that a portion of this latter might escape during the reaction, and that the ammonia might remove it from the residue. As in cases where the arsenic only exists in almost inappreciated proportions it might have entirely passed into a state of sulphuret, it is indispensable always to search for arsenic in "sulphuric carbon," by means of ammonia.

Orfila long since showed the necessity of driving off from those liquids in which arsenic is sought for by Marsh's method the nitric acid which they might contain; and supported his opinion by the fact that, in the presence of nascent hydrogen, nitric acid may be partially decomposed, giving rise to compounds capable of producing a detonation when the hydrogen is set on fire for the production of the metallic spots.

M. Blondlot has also considered the question from an entirely different point of view, and the results at which he has arrived deserve the greatest attention: experiment having shown that, under the influence of sulphuric and hydrochloric acids, habitually used for disengaging hydrogen, arsenic, in the form of a soluble compound, furnishes a *gaseous* hydruret, there is apparent reason for concluding that it would be the same in the presence of every other acid. Experiment also has demonstrated that it is quite the contrary in the presence of a very weak proportion of nitric acid. When water and arsenious acid are brought into contact with zinc or iron, nitric acid gives rise to a *solid hydruret*, composed of two parts of arsenic and one of hydrogen, the production of which is hindered by a very small proportion of lead. M. Blondlot adds—"Let a very weak solution of arsenious acid be made in distilled water; and, after being divided into two equal portions, let the one be acidulated with pure sulphuric or hydrochloric acid, and the other with nitric acid; let a strip of zinc be plunged into each. In contact with the liquid acidulated by the sulphuric or hydrochloric acids, the metal preserves its polish; in the liquid acidulated by the nitric acid gas bubbles are given off, and the zinc is covered with a coating first of yellow, and then of brown and almost black hydrate of arsenic."

At first, it would appear that these effects arise from an oxygenating action determined by the nitric acid; but the chloric and chromic acids, which act so powerfully as oxidizers, produce nothing similar. It is known that, by acting upon zinc and tin, in liquids properly diluted, nitric acid produces ammonia. It is precisely this kind of action which is exercised here; and as the solid hydruret of arsenic is acted on with great difficulty by nitric acid, it may be formed in the presence of this acid, whilst the portion of nascent hydrogen, which should constitute the gaseous hydruret, may react upon the nitrogen, as when in contact with zinc and tin, and produce ammonia.

The formation of this solid hydruret might serve to discover very weak proportions of arsenic in some solutions; but M. Blondlot has himself acknowledged that Marsh's method is the most preferable. Is this important reaction produced in the presence of other acids, the action of which would be limited to disengaging hydrogen, and uniting with the oxide formed? Experiment has proved that, mixed in varied quantities with different acids, nitric acid always gives rise to this reaction. M. Blondlot has shown this experimentally.

Concentrated nitric acid, mixed with different proportions of various acids, was used in making to act one of Marsh's small apparati, which contained from twenty to twenty-five grammes of distilled water, together with two or three drops of solution of arsenious acid and some strips of pure zinc, in contact with some platinum threads, intended to assist in the disengagement of the gas. Traces of arsenical rings were thus rarely found in the heated tube, whilst arsenic was presented in brown flakes, some loose, some adhering to the zinc. The precipitation of arsenic as a solid hydruret is so complete in this reaction, when an excess of sulphate of zinc is employed, that after having successively replaced such of the strips as might be supposed to have left off exercising any action, the liquid tested, either by hydrosulphuric acid or by Marsh's process, no longer yielded evidence of arsenic.

If the proportion of powerful acids (sulphuric or hydrochloric) be considerable, or the giving off of the hydrogen be accelerated by the use of strips or sheets of zinc, the hydruret may detach itself from the surface of the zinc, and float on the liquid in flakes; and if a blade of zinc, covered with hydruret of arsenic, be plunged into this liquid when strongly acidulated with sulphuric or hydrochloric acids, together with nitric acid, hydrogen gas is disengaged abundantly from the surface of the zinc, the hydruret remains in suspension in the liquid, and in that case the gas does not carry away the least trace of arsenic.

If the zinc covered with the hydruret of arsenic be exposed to the air, the latter is transformed by degrees into arsenious acid; hence it would ensue that if this zinc were made use of in testing for arsenic, with the previously-acquired conviction of its purity, arsenic might be found in the treatment of products which would not contain it. On the other hand, the gaseous hydruret of arsenic, while traversing a liquid composed of four or five cubic centimetres of distilled water, slightly acidulated by a mixture of two parts of sulphuric acid and one of nitric acid, furnishes solid hydruret; but the proportion does not represent the arsenic of the disengaged gas.

When placed in a solution of nitrate of silver, and when the silver is precipitated by a slight excess of hydrochloric acid, if, after having diluted the liquid, some drops only of nitric acid be added to it, and a strip of pure zinc, the latter is very soon covered with solid hydruret, which is easily separated by dissolving the zinc in sulphuric acid.

In order to characterize distinctly what M. Blondlot has described, it is important to make known the properties of the solid hydruret of arsenic produced under these conditions. This product is blackish-brown, flaky, without sign of crystallization, and insoluble in water; it furnishes no arsenious acid by being boiled in water. It is insoluble in cold concentrated or diluted sulphuric and hydrochloric acids, but in hot sulphuric acid it yields arsenious and sulphurous acids, and with hot hydrochloric acid, chloride of arsenic and gaseous hydruret. Nitric acid and chlorine dissolve it rapidly when cold.

Heated in an open tube it furnishes arsenious acid and water. It has been already shown that the hydruret is easily separated from the surface of zinc by dissolving the latter in sulphuric or hydrochloric acids more or less diluted; the arsenic also may be easily carried off by plunging the zinc into pure concentrated sulphuric acid, heated from 212° to nearly 400° Fahr., in which case the arsenic passes into the state of arsenious acid, the existence of which is determined by the ordinary methods. M. Blondlot thought that a process analogous with that of Reinsch might be founded upon the properties of the solid hydruret of arsenic, but he soon discovered that very slight traces of lead, tin, copper, &c., prevented the production of the hydruret. The same obtains with organic substances, among which may be mentioned principally those which in testing for phosphorus by Dussart's method (the production of phosphuretted hydrogen) prevent the formation of the hydruret.

The facts observed by M. Blondlot offer much interest in testing for arsenic; in fact, although the necessity for driving off the nitric acid met with in the products, in order to avoid the inconveniences stated by Orfila, has been constantly noticed, the very small proportions which the sulphuric or hydrochloric acids used in the experiments may contain have not been considered; and yet almost infinitesimal proportions may react in a sensible manner.

When, in the employment of the process for destroying organic matters by means of sulphuric acid, traces of nitrous compounds are left, if very pure sulphuric acid and zinc are used for disengaging hydrogen,—without facilitating the disengagement by strips of platinum,—the whole of the arsenic, if the destruction of the organic matters is complete, may pass into the state of solid hydruret. On the other hand, by the use of distilled sulphuric acid which may retain hitherto neglected traces of nitric acid and, as well as the zinc, some traces of arsenic also, no trace of arseniated hydrogen being evolved, the expert would conclude, and apparently with reason, that, if he obtained some spots or rings by Marsh's method on introducing the suspected products into the apparatus, these products contained arsenic, and yet that which he obtained might arise solely from the acid and the zinc, because the organic substances would not have been completely destroyed.

This fact is easily demonstrated by introducing into the apparatus for the production of hydrogen, supplied under the conditions indicated, a small quantity of sugar and water which immediately insures the production of a ring. Nitric acid gives rise to the results which have been pointed out, without the necessity of operating under any special condition; the other acids furnish them, under a pressure of more than two atmospheres, but in this case ammonia is not produced, and the presence of organic matters does not prevent the formation of solid hydruret of arsenic.

The facts above stated deserve the attention of chemists charged with judicial labours, but the consequences which result from them require to be developed, and this cannot be done better than by quoting from M. Blondlot's paper. He says:—"In the researches of judicial chemistry, it is the rule to employ pure reagents only, but in fact those are looked upon as pure which are absolutely exempt from the poisonous substances which are to be tested for. As regards arsenic, commercial zinc and sulphuric acid are looked upon as quite pure enough when they are free from that metal, without troubling about any other foreign matters which they habitually contain; never notably, when Marsh's method has been used, has any one until now busied himself about the presence of any traces of a nitrous compound, either in the sulphuric or hydrochloric acids used, or in the suspected liquids, and arising in this latter case from reagents, which have co-operated in the destruction of the organic matters; but it results from the preceding facts, that the nitrous compounds present here a double danger.

"Let us suppose that a chemist, after having disorganized the suspected substances by the most usual method—at least in France—namely, by means of sulphuric acid, may not have completely expelled by heat the smallest traces of nitric acid, with which he is recommended to treat the carbonized matters obtained; let us also suppose, which is not the less admissible, that this same chemist, by an act of prudence which is assuredly blameless, judged it fit to supply his apparatus with sulphuric acid and zinc, both purified, without provoking the action of the zinc by contact with platinum. What would inevitably occur? If the carbonization has been sufficiently complete, so that no trace of organic matter remained in the suspected liquid, a part, or perhaps even the whole, of the arsenic would remain in the apparatus in the form of a solid hydruret, and might thus escape all researches. This is moreover a fact of which I have experimentally assured myself.

"The reverse error, which is still more serious, might also be produced. Let us admit, in fact, that a toxicologist thinks it right, as before, to use only purified zinc, and distilled sulphuric acid. The process of purification, depriving neither of the arsenic they might contain any more than it deprives the acid of the nitrous compounds which accompany it, a fatal combination of circumstances is brought about. At first, the chemist of whom we are speaking will not in any way occupy himself with the nitric acid which his sulphuric acid may accidentally contain, a small trace of a nitrous compound being, until now, considered unimportant. As to the arsenic which might be contained either in the zinc or the sulphuric acid, it must be experimented on at a white heat to verify the fact. Now, what will happen? The arsenic, if it exists, passing to a state of a solid hydruret, cannot yield traces of a ring in the tube in which it is disengaged; if then, considering the reagents to be pure, he introduces the suspected liquids into the apparatus, and if unfortunately this latter, incompletely carbonized, still contains some traces of organic matters, the reactions change suddenly under their influence; the remaining arsenic forms into a gaseous hydruret, and will produce a ring which may be easily attributed to the suspected matter. I have myself verified this fact. When the experiment at a white heat has not produced any ring, or in some cases when a ring is scarcely perceptible, I have found it suffice to introduce a little sugar and water into the apparatus in order instantly to produce an arsenical ring incomparably more decided."—*Annales d'Hygiène Publique*, Jan., 1864, No. 41.

Arsenical Poisoning by Absorption.—Dr. Giovanni Polli records a case of a boy who was suffering from chronic bronchitis, and for whom he ordered an ointment containing tartar emetic. The ointment was applied, and the characteristic pustular eruption appeared. In order to lessen the irritation, Dr. Polli prescribed a poultice of linseed-meal with a piece of muslin interposed between it and the skin. The boy for some hours experienced much relief, but in time presented symptoms which were not in accordance with the course of the disease. He suffered from great prostration, pain sometimes in the forehead, sometimes in the nape of the neck, and a sense of dryness in the fauces. The tongue was foul and the lips were dry, there was a constant desire to micturate, with itching in the urethra, discharge of very pale urine, and slight intermittent delirium. The poultice was regularly renewed every twelve hours; on the second day, the boy having complained much of pain, Dr. Polli raised the poultice to look beneath, and found all the pustules empty, and exhibiting a little ulcer with a white base, as if a cautery had been applied; this differed from what should have been presented, inasmuch as the tartar-emetic pustules leave black crusts. At the same time he observed that a green muslin had been placed between the poultice and the skin. The clear greenness of the tint led the author to suspect that Scheele's green was present. Having taken a piece of the muslin, on examining it by Marsh's test, he discovered that this suspicion was correct, arsenic being detected in large quantities. On removing the muslin altogether, the arsenical symptoms quickly subsided.—*Annali di Chemica Applicata alla Medicina.* December, 1863.

On the Prevention of the Poisonous Effects of Anæsthetic Agents.—M. Simonin, in treating on the collapse of the circulatory and respiratory organs during the employment of anæsthetic agents, states that the two most important points to be observed are the insensibility of the temporal regions and the narcotism of the masseter muscles. In speaking of the peripheric insensibility resulting from the inhalation of anæsthetic agents, and from their use *per anum*, he says that all parts of the periphery of the body do not become insensible at the same moment: thus it takes several seconds before anæsthesia is produced on the skin of the forehead and the temporal regions, and several minutes to produce the same result on the skin of the hands and the feet. The time which elapses between the narcotizing of the extremities of the limbs and that in which the skin of the frontal and temporal regions ceases to react is rather longer, when, instead of the vapour of chloroform, the patient inhales that of ether. This time is longer still, when ether is introduced *per anum.* To discover in time the anæsthesia of the various parts of the periphery of the body, the action of the anæsthetic agents must be decreased, and punctures be made on the different parts above-mentioned, about every ten seconds or oftener. The disappearance of these phenomena takes place in an inverse order to that of their appearance. With regard to the action of the same agents on the muscular system, M. Simonin opines that the contraction of the masseter muscles appears last of all during the excitation of the muscular system, often when the rest of the system is relaxed. This local rigidity is the indication of a very near collapse of all the organs, especially those of the circulation and respiration. Anatomy points out the cause of these facts, and the explanation shows the importance of their observation during anæsthesia. It is the fifth pair of nerves which gives sensibility to the skin of the temples; it is the same pair which furnishes ramifications to the masseter muscle. This fifth pair arises from the lateral and anterior part of the medulla oblongata, and as soon as the parts to which it distributes itself, either the organs of sensation or of movement, show the commencement of narcotization. The movements of the respiration and circulation soon become disturbed, for the vital point is in its turn about to be influenced. The author also remarks that the sensitive action of the nervous filaments pertaining to the skin is extinct before the motor action ceases. This normal absence of synchronism shows that there is no reason to be uneasy even when the sensibility of the temples ceases to exist. This is an important fact, and is the result of researches made at Nancy, where it was shown that subcutaneous anæsthesia did not exist anywhere so long as sensibility remained in the temples. To this rule M. Simonin only found one exception during sixteen years' observation. In many cases collapse of the masseter muscles may be seen without life being compromised; uneasiness should, however, arise in the mind of the practitioner with this last period of muscular insensibility. The permanence of muscular rigidity which the contraction of the jaws produces is a favourable physiological limit, which he must try not to overstep, whenever the opening of the mouth is not one of the conditions of the operation to be performed. Trismus has always reassured the experimentalist, when several other symptoms of profound intoxication during anæsthesia have alarmed him. It is thus important to ascertain the disappearance of sensibility in the temporal regions, and to be assured of the state of the elevating muscles of the lower jaw, since the observer has then under his eyes, and with the greatest ease, the course of the progress of the intoxication of the medulla oblongata, and in the generality of cases, while ceasing to employ a poisonous agent, he has often the power to prevent the last and dreaded phases of anæsthesia—namely, collapse of the circulation and of the respiration—in a word, death.—*Revue des Sociétés Savantes.* June 26th, 1863.

[We have pleasure in bearing testimony to the accuracy and importance of M. Simonin's observations. Our experience, like his, and derived from long research, is that the muscles which raise the lower jaw are the last that collapse under the use of anæsthetics. Hence we have seen more dangerous symptoms during profound anæsthesia for operations on the mouth, such as extraction of teeth, than under any other circumstances. The exposition of M. Simonin is also in our opinion exceedingly sound and common-sense, and we specially recommend his advice respecting the necessity of observing the contraction of the elevators of the jaw to those who are learning how to administer narcotic vapours with scientific judgment and knowledge.—B.W.R.]

On the Prevention of Danger from the Use of Leaden Pipes for the Conveyance of Water.—A ministerial circular has recently been issued in France relative to the conveyance of water containing only small quantities of lime through leaden pipes, and to the dangers which the apparatus for distilling sea-water, now becoming of common employment in the service, subjects those who afterwards partake of such water. It often happens that these apparatus are fitted up with tubes or joints of lead, and the distilled water which passes over has frequently been found so charged with lead as to occasion serious accidents. To meet these dangers, M. Schwartz, of Breslau, proposes to fill the pipes of lead before they are used with a concentrated solution of an alkaline sulphuret, and to prolong the contact of the solution with the pipe for ten or fifteen minutes. In this way, he asserts, a layer of sulphuret of lead is formed on the inner surface of the pipe, which acts as a perfectly protecting varnish and completely prevents every ulterior reaction of the water upon the lead. M. Bouchardat, who briefly comments on the above proposition, states that he has taught for many years past that waters containing traces of sulphuretted hydrogen do not become injurious when held in reservoirs of lead.—*Répertoire de Pharmacie*, Nov. 1863.

On the Poisonous Properties of Thallium. By M. Lamy.—M. Lamy dissolved five grammes of sulphate of pure thallium in milk, intending to give this to two puppies, each two months old, and each weighing three kilogrammes. After tasting the liquid the animals refused to take any more of it. The next day, in the afternoon, the door of the kennel in which the dogs were shut up was inadvertently left open, and all the milk disappeared, taken doubtless, as the result will prove, by two chickens, six ducks, and a bitch of middle size.

Some hours after the disappearance of the poisoned milk, the bitch became dull, uneasy, and refused to take her food. In the night she was seized with sharp pains, sudden and rapid in their attacks, which made the animal utter unceasing cries. In the morning the pains had neither decreased in frequency nor intensity. The creature refused both drink and food; the expression of her face was changed; her back was curved from the intensity of her sufferings, her sides were compressed, her breathing oppressed, and the saliva abundant. The posterior limbs were first convulsively agitated, and afterwards by degrees partially paralysed. The seat of suffering was evidently the intestines; it was momentarily calmed by pressure or friction of the stomach.

Under the influence of the preconceived idea that thallium in such small quantities could not produce such poisonous effects, M. Lamy did not at first think of administering iodide of potassium as a counter-poison. The whole day elapsed without any apparent diminution of the pains. The next morning paralysis had made great progress; the animal was in a state of perfect prostration; she still, however, recognised him, and tried to show she was pleased when he went near her. She succumbed on the third day, sixty-four hours after having taken the poison. Neither vomiting nor alvine ejections were noticeable during the illness.

In the evening one chicken and six ducks were found either dead or dying. In such of the birds as were alive when the accident was first perceived, paralysis of the posterior limbs in a more or less degree had set in.

The two young dogs which had only tasted the poisoned milk had become quiet, and seemed fatigued; they very soon trembled convulsively, and could with difficulty support themselves on their hind legs; sharp pains then came on, and finally death, four days after the intoxication, and in spite of the efforts which had been made to save these dogs by a normal regimen two days before.

At the post-mortem no lesions, no serious inflammatory processes, were discoverable. The gall bladder of the bitch was extremely distended, and in some ducks various serous membranes, that of the liver in particular, were of a granulated colour.

Spectrum analysis quickly and easily showed the nature of the poison. In fact, when examining with the spectrum some small pieces of the different organs of the dead animals, the thallium was immediately recognisable by its distant and characteristic green streak. The intestines contained the metal in greater abundance than the muscular flesh, and the bones; the white serous membrane of the liver held more than the substance even of that organ. A tooth, as might have been expected, contained no trace of thallium.

Eight days after this accident a second chicken was taken ill; its wings drooped, and it could with difficulty stand on its feet; curiously, when it wanted to eat it could not stretch out its neck sufficiently, and its beak could not reach the food. For three days it languished in the same state. It was then killed, and thallium was found in the intestine; but it was in very small quantities, and in the other organs no traces of it were visible. To be better convinced of the strength of this poison, one decigramme only of sulphate was given to a young dog about the same age as the others; this animal died forty hours after having taken the poison.

The preceding facts show the sulphate of thallium to be an energetic poison, and that the two principal symptoms of poisoning which it excites are, in the first place, pain, the seat of which is in the intestine, and is exhibited in sharp shooting pains, succeeding each other with rapidity, like shocks of electricity; and in the second, by tremblings, and a more or less complete paralysis of the lower members. To these might be added constipation, contraction or depression of the stomach, and the absolute failure of appetite. The two first, however, are the most important symptoms.—*Répertoire de Pharmacie*, Oct. 1863.

On an Antidote to the Poison of the Viper.—M. Soubeirain has lately made known a new remedy for the bite of the viper: it consists, in all cases, in immediately applying a ligature sufficiently broad to prevent injury to the parts above that are poisoned, but yet so firm as to interrupt all communication between the trunk and the bitten part, and to prevent the absorption of the poison. Suction should be applied to the part, and an endeavour made to excite bleeding; if necessary, scarification should be resorted to, and afterwards a cautery. In addition to these recognised methods M. Soubeirain recommends the use of iodine or bromine, but especially the solution of MM. Broinard and Green, and which is composed of water, iodide of potassium, and pure iodine. This solution should be injected into the wound by means of the flask, with a pointed stopper, invented by M. Viaud-Grand-Marois. At the same time ammoniacal draughts should be administered internally, and the limb and environs of the wound should be rubbed with liniment of ammonia, followed by the application of a poultice. M. Regimbeau, who writes the report on this subject, remarks, that if the solution of MM. Broinard and Green can exercise a specific action on the poison of the viper, by causing its decomposition independently of any slight caustic power with which it may be endowed, the use of this solution ought to be adhered to rigorously, as iodine and bromine have the property of combining with hydrogen, and may thus also have the power of destroying even the living organic poison. Presuming this to be true, it should follow that the effects of the poison of the viper should be annihilated everywhere. He further observes that the same remedy, on the supposition that it acts in the same way on other organic poisons, should be administered internally in cases of hydrophobia; and he suggests that, in districts where rabid animals exist, places should be set apart, and experiments made on them, to determine if this line of treatment is of any avail.—*Ibid.*, Feb. 1864.

Further Researches on Bibron's Bromine Mixture as an Antidote to Snake Poison.—Dr. Charles Hughes contributes a case in support of the value of Von Bibron's solution. The case was that of a German soldier, aged twenty-two, who entered regimental hospital on July 13th, 1863, having been bitten a few hours previously by a genuine specimen of the reptile known in the Western States as the Copperhead. The seat of the bite was the extremity of the little finger of the left hand.

Immediately after the reception of the bite the comrades of the bitten man applied a light ligature around the finger, a little anterior to the metacarpo-phalangeal articulation, which effectually prevented the venom from mingling with the general circulation, and dosed him with whisky, but not in sufficient quantities to produce intoxication. In this condition he was brought to the hospital, and on the morning of the 14th, the treatment to which his recovery is attributed was instituted.

Few other surgeons would have hazarded an attempt to save the finger, but would have removed it at once, and been content with the salvation of the patient's life, considering it cheaply purchased at the expense of simply a finger; the surgeons began with the design, if possible, of not only saving the whole, but of preserving the part also. To fulfil the indications which presented as necessary for the salvation of the finger, they loosened the ligature, incised the finger as for a whitlow, and immersed it in water for twenty-four hours. To this treatment they added an emollient anodyne poultice on the third day, and continued it through the fourth, when simple cerate dressings were daily applied until July 30th, at which time the man returned to duty.

This constituted all the local treatment, except the lancing of sacs of infiltrated blood and serum, and the application of anodyne and astringent lotions to the hand and forearm.

In the part all the customary symptoms of venomous snake bite were manifest. The black and deadened appearance around the bite, the sero-sanguinolent exudation, and the excruciating pain, were all present in the finger from the time the bite was received, and as soon as they loosened the ligature, became apparent in the hand and forearm. Here the swelling was great and the pain intense; the former extending to the elbow, and the latter reaching to the axilla.

Constitutional symptoms were but slightly manifest. They consisted in slight nausea, a little anxiety of countenance, faintness, and rigors, all of which very speedily disappeared as soon as the antidote had taken full effect in the system. The constitutional treatment consisted exclusively of Bibron's antidote, which was commenced soon after the ligature was removed, and the dose repeated, on the first day, every three or four hours, on the second *ter die*, and on the third *pro re nata*. In all, about twelve doses were given. No internal anodynes were exhibited, and no other internal remedies, except a copious draught of whisky at bedtime to procure sleep.

The composition of Bibron's antidote, as prepared and furnished to army-surgeons by the medical purveyor of the American army, is as follows:—℞. Brominii, ʒiiss; potass, iodidi, gr.iij; hyd. chl. corrosiv., gr.j; alcohol dilute, fʒxxx. Misce. The following directions for administration:—Give a fluid drachm diluted with a tablespoonful of wine or brandy, and repeat it if necessary. The formula given by the discoverer of the antidote, and employed by Drs. Hammond, Gross, Henry, and others in the United States, who have used it successfully, is more concentrated, and not combined with alcohol, but simply mixed with wine or brandy when administered. It is as follows:—℞. Potassii iodidi, gr.iv; hyd. chl. corrosiv., gr.ij; brominii, ʒv. Misce. Guttæ x. at a dose, repeated if necessary.

Its efficacy is, perhaps, entirely due to the bromine, and it would seem from the size of the dose that can be tolerated when the system is under the influence of the virus, that it acts as a direct antidote or neutralizer of the poison.—*American Journal of the Medical Sciences*, Jan. 1864.

II. HYGIENE.

On Recruiting in France for the Army.—Among the ancients and moderns of all nations the height of the man has always been one of the principal points for consideration previous to his admission into the army; and a careful examination of the subject shows not only the notions entertained by different nations as to the standard of perfection in stature, but suggests also the existence of a gradual decrease in stature.

M. Boudin, in an essay on the medical history of army recruiting in France and other countries shows that there is a great difference in the height thought to be necessary in the Roman soldier in the time of Valentinian compared with that of the French soldier in the year 1832. According to the law of Valentinian, the minimum height was 1·665 mm.; but according to that of France of the 11th of March, 1832, it was 1·560 mm. Why there should be such a difference in the standard heights at these two periods is a natural question, and one into which the French have entered somewhat largely. In so doing, they have supplied a statement of the height, weight, and other particulars of the recruits, conscripts, and others composing the armies of the present day.

In ancient Rome, the age required for military service was seventeen, and the time of service dated from that age, even if the soldier entered the army before attaining it. The height, as before stated, was 1·665 mm. The weight of the armour and accoutrements imposed upon him to carry was something enormous; by some authorities it has been estimated at ninety pounds, besides provisions.

The height of the French soldier in the time of Louis XIV. was required to be 1·624 mm.; from 1799 to 1803 it was 1·598 mm.; from 1804, 1·544 mm. According to the law of March, 1818, it was increased to 1·576 mm.; in December, 1830, it decreased to 1·540 mm.; and on the 11th of March, 1832, increased again to 1·560 mm. The height of the Belgian soldier is fixed at 1·570 mm.; that of the Prussian at 1·624 mm.; of the English at 1·620 mm.; of the Sepoy at 1·650 mm.; and the Sardinian at 1·541 mm.

In M. Boudin's remarks upon the height of individuals, he states the tallest to have been 2·923 mm., or 9 feet; and Pliny mentions an Arab who came to Rome in his time as having been 8 ft. 10 in. high (French measure). Also, in the sixteenth century, a giant of the same height was seen at Rome. Other examples of great height are quoted by various authors, as well as some of diminutive stature; and of this latter number a dwarf may be mentioned who lived in the reign of Augustus, and whose eyes were so small, that they were represented in the statue erected to his memory, by that monarch, by two diamonds. These, however, are the extremes of nature, and have no place in our statement.

In comparing the returns of the exemptions in France on account of defective stature, a notable diminution in the number rejected is observable during the last thirty years. Out of 10,000 examined in 1831, 929 were under height; whereas in 1860, out of the same number, only 600 were exempted on that account. In four departments of France the height was stationary; it had decreased in 19 departments, and increased in 63. The proportion of young people above the height of 1·732 mm. was below 5 per cent. in 18 departments; it increased more than 10 per cent. in 20 other departments, and varied from 5 to 10 per cent. in 48.

Another result ascertained from the inquiry is, that the stature of a population is in nowise the expression of its prosperity or misery, but simply arises from the race; in other words, height is hereditary. The number of young men of a stature which exceeded 1·732 mm. was only 444 out of 10,000 recruits from the departments of Bretagne, but it increased to more than double that number in the departments contiguous to Normandy. The increase of stature in France has been attributed by some to the cessation of the wars of the Republic and of the First Empire, whereby men of tall stature were enabled to stay at home and marry; and from this they had hitherto been excluded in consequence of the levying of the conscription and their being sent out of the country to distant lands. This explanation of the cause of the increase of stature in France corresponds with the fact, that the proportion of exemptions from default of height among individuals born from 1811 to 1816 (classes from 1831 to 1836) has constantly exceeded 800, and has even increased to the enormous amount of 929 out of 10,000 examined, as included in the births of 1811 (class of 1831); whilst, since 1817, one year and a half after the cessation of the war, the proportion of exemptions was often below 800, and even decreased to 600, and less in the two latter classes, respecting which alone official information is possessed. A height exceeding 1·895 mm. was only to be found in eighteen departments, and that exceeding 1·922 mm. in only five. In some of the English recruits for the army, the height has been stated to exceed 1·720 mm.: among the Irish, 1·707 out of 10,000; among the English, 1·903; and among the Scotch, 2·317 were found to be of the stature mentioned.

As regards weight, it has been found that 157 men out of 10,000 recruits for English service have weighed less than 45 kilogrammes; each seven-tenths of them weighed from 54 to 63 kilogrammes; and 55 only out of the 10,000 exceeded 77 kilogrammes in weight. The medium weight of a soldier in a Madras regiment of Sepoys was found to be 50·397 kilogrammes; in a Bengal regiment of Sepoys, 58·438 kilogrammes; and in a French regiment of mounted guards, 64·500 kilogrammes. The proportion of exemptions arising from insufficient stature varies in seven of the European States, as will be seen from the following statement: Out of 10,000 examined in France, 9413 were accepted, and 587 exempted; in Belgium, 8660 were accepted, and 1340 exempted; in Austria, 8598 were accepted, and 1402 exempted; in Denmark, 8494 were accepted, and 1506 exempted; in the Sardinian States, 8050 were accepted, and 1950 exempted; in Saxony, 7890 were accepted, and 2110 exempted; and in Prussia, 7626 were accepted, and 2374 exempted. In these same states the fitness for military services is on the same decrease. In France, from 1858 to 1860, out of 1000 young men, only 682 were suitable for the service; in Belgium, from 1842 to 1850, only 630; in the Sardinian States, from 1828 to 1837 only 598; in Denmark, from 1852 to 1860, only 522; in Austria, from 1857 to 1858, only 497; in Prussia, during 9 years selected from 1831 to 1854, only 283; and in Saxony, from 1845 to 1854, only 259.

III. SUMMARY.

The following summary refers to papers which, from want of space, cannot be quoted at length in this report:—

1. *On the Presence of Air in the Veins as a Cause of Death.* By James Sumner Greene, M.D. (*American Journal of the Medical Sciences*, Jan. 1864.)—Dr. Greene reviews with great care the various opinions respecting the cause of death, after the introduction of air into the bloodvessels. He refers to obscure cases of death from this cause.

2. *On Contagious Fermentative Diseases, and their Treatment by the Sulphites.* By Giovanni Polli. Milan, 1864.—The distinguished author in this monograph, supplies clinical evidence of the value of the sulphites as agents for destroying septic poisons.

3. *On Measly Pork in relation to Public and Private Hygiene.* By Professor A. Delpech. (*Annales d'Hygiène Publique*, Jan. 1864.)—The first part of a very comprehensive and promising essay, historical and practical.

4. *The Number of Victims from Hydrophobia in France.* By M. Boudin.—*Ibid.*

5. *A Medico-legal Study upon Diseases Accidentally Produced by Improvidence, Negligence, or Contagious Transmission; including the Medico-legal History of Syphilis.* By M. Tardieu, M.D.—*Ibid.*

6. *Ostreonomy. On Poisonous Oysters and Various Eatable Oysters.* By M. Ferrand.—*Pamphlet.* Lyon, 1863.

7. *On the Effects of Working in Compressed Air.* By M. Foley, M.D. (*Volume.* Paris, 1863.)—A most interesting account of the effects of compressed air on the workmen employed in the construction of the Bridge of Argenteuil. A translation of the leading facts is published in the 'Social Science Review' for February, 1864.

8. *On the Physiological Properties of Nitrobenzine, Aniline, and Fuchsine* (*Aniline Red*). By Dr. Charvet.—*Répertoire de Pharmacie*, Nov. 1863.

9. *On an Epidemic which broke out among the Workers in Aniline Red.* By Dr. Charvet. (*Ibid.*)—The affection only appeared in one manufactory, and the houses contiguous. No persons died. The disease was peculiar, as manifesting various kinds of eruptions on the skin. The digestive organs were also affected, and diarrhœa was a symptom. The nervous system was in every case influenced, and paralysis of motion and sensation, especially in the extremities, was generally observed.

10. *New Method of determining Arsenic.* By M. Zenger. (*Berliner Klinische Wochenschrift.* No. 4. Jan. 1864.)

11. *On Ophthalmia Produced in Workmen Employed in Preventing the Oïdium in the Vine by means of Sulphur.* By M. Bouisson, of Montpellier. (*Revue des Sociétés Savantes*, July, 1863.)—The author states that for some time past a species of ophthalmia has prevailed in the departments of Hérault, and other vine-growing districts in the South of France. The disease is caused by the sulphur employed by the vine-growers, for the prevention of the oïdium. The workmen attacked have red, watery, tumefied eyes. They suffer acute pain, especially towards the middle of the day, when the heat and solar radiation are intense. The sufferers complain of photophobia, and of pains radiating towards the forehead. The irritation is diminished by rest, and by bathing the head with cold water, but is renewed day by day, when the occupation is continued, until inflammation is engendered. It is rarely, however, that the

disease extends the limits of a simple conjunctivitis. The sulphur is used either as sulphur sublimated or triturated. The first kind contains a certain amount of pure sulphuric acid, the second kind only traces of the acid. Still the sublimated more frequently causes injury than that which is merely pulverized. The least injurious plan of operation is that of blowing on the sulphur with bellows, as all other methods disperse it more largely into the atmosphere. The operation is generally repeated four times in the season, and the last produces ordinarily the greatest number of attacks, on account of the heat and dryness of the atmosphere, which increase the irritating effects of the sulphur molecules. Women and children being employed more extensively than men in the work named, are the greatest sufferers.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

I. *A new Method of Operating for Strangulated Hernia.* By Professor Max Langenbeck. (Allg. Wien. Med. Zeit. 1863. Nos. 49-52.)

Professor Langenbeck observes that this procedure might perhaps be most fitly termed the subcutaneous reposition of hernia. An opening is made in the skin only large enough to admit the forefinger, not just over, but on one side of the hernia. The finger introduced into the wound easily thrusts aside the connective-tissue, glands, fat, &c., and pursues its course until it reaches the inguinal canal or the fossa ileo-pectinea, as the case may be, passing under the skin very much as a bullet does in gun-shot wounds. Having reached the point of stricture, the finger practises the subcutaneous isolation of the herniary tumour, destroying any recent adhesions and external exudative structures which may impede reposition, and which are, indeed, often a result of the employment of the ordinary means of reduction. In most cases, this isolation of the hernial tumour can be speedily and easily accomplished; and its size and degree of tension, as well as the thickness of its sac, may be approximately decided upon.

Having reached the point of stricture, we should, in femoral hernia, feel for the horizontal ramus of the pubis and Gimbernat's ligament, and in inguinal hernia for the internal crus of the abdominal ring, and gently compressing the surface of the nail against the neck of the sac, pass the finger in beside it. No great resistance is offered to this by the incarcerated hernia; and by the gentle pressure employed, not infrequently a certain amount of peristaltic action and increased protrusion of the intestine is produced, together with more or less separation of the recent exudation. In the case of femoral hernia, the finger feels distinctly with its volar surface the sharp edge of Gimbernat's ligament, and at its lower edge the horizontal ramus and pubic ligament upon which it rests. In order to produce dilatation, Gimbernat's ligament is to be ruptured through part or the whole of its extent, or separated from its insertion. The ligament yields to the pressure made by the nail with an audible cracking sound. In inguinal hernia, the end of the finger meets with a greater resistance from the inner crus of the abdominal ring than from Gimbernat's ligament. The resistance is usually, however, overcome by a steady boring-motion of the finger against the point of insertion in the pubis. When the resistance cannot be thus overcome, Dr. Langenbeck employs what he calls an "incision ring." This, constructed of wood or metal, resembles a signet-ring, made as thin as possible, and having affixed to its surface, in the direction of the long diameter of the finger, a cutting edge projecting about 1½ lines. This ring is placed on the finger-point, the cutting edge being on its volar surface, and the finger is introduced through the short subcutaneous canal which it had already made as far as the annulus. By pressing the cutting edge against the sharp edge of the internal crus, this is divided with complete safety to the intestine, which is protected by the dorsal surface of the finger. In order to prevent the slipping off the ring during the withdrawal of the finger, it should be secured by a ligature. The incision made, the necessary dilatation is accomplished by means of the finger. In the case of external inguinal hernia, the resistance of the ring is more easily overcome. As soon as the stricture has been removed, a retractile motion is set up, and the hernia, as a general rule, is spontaneously reduced. Usually it at once passes into the abdomen, sometimes waiting a minute until after the finger has removed some adhesions from around the orifice. When the adhesions are old, and very firm, it is best to rest content with removing the constriction without attempting reposition. After the finger has been withdrawn, there is but a slight appearance of injury at the seat of the operation, and the place where the hernia lay is completely covered with the skin. In none of the author's 10 cases did the operation occupy more than five minutes, and in most of them but two. The subsequent course of these cases was also highly favourable, the wound readily uniting, the patients all leaving their beds between the fifth and tenth day, a broad truss being first applied.

Dr. Langenbeck contrasts at some length his procedure with the ordinary mode of treating hernia, and maintains that, by reason of its much greater safety, and the more rapid recovery which ensues, it is infinitely to be preferred.

II. *Two Cases of Enormous Distension of the Bladder.* By Dr. SCHNEIDER. (Schweizerische Zeits., vol. ii. p. 453.)

To one of these cases we need not refer, but the second Dr. Schneider regards as unique in the amount of urine contained in the distended bladder. The patient, aged sixty-three, was brought into the hospital at Bern 11th May, with symptoms of general dropsy, from which he had suffered for some time. Although the examination of the condition of the abdomen was difficult on account of the œdematous state of its parietes, and the patient was stated to have passed urine since his admission, yet the shape of the tumefaction present seemed to indicate its being produced by a distended bladder. The catheter was, therefore, introduced, and a large stream of urine flowed out in astonishing quantity, no less than 21 Swiss *schoppen* (8 litres, or 14 English pints) being withdrawn. After twelve hours, 9 other *schoppen* were removed, and in the evening 4 more, making altogether 34 *schoppen*, or more than 22 English pints in the twenty-four hours. On subsequent days, lesser quantities were withdrawn, and the man died on the 25th. At the autopsy, there were found two large lateral diverticula in the bladder, directed backwards, and giving the organ much the appearance of a bishop's mitre. No mechanical hindrance whatever to the exit of the urine, in the shape of stricture or enlarged prostate, existed. The coats of the bladder were rather thin, and the ureters moderately dilated; the kidneys were somewhat larger, and their pelves more dilated than normal. There was dropsical effusion into the chest and brain. Dr. Schneider attributes the retention in this case to weakened muscular power occurring in an old and phlegmatic subject.

III. *On Œsophagism.* By M. NÉLATON. (Union Méd., No. 28.)

As an example of the curious nervous condition termed œsophagism, M. Nélaton called the attention of his class to a man of vigorous temperament, thirty-five years of age, and in good health, who came to the hospital under the idea that he had a foreign body in the œsophagus. A fortnight since, while picking his teeth with a thin piece of wood, he was suddenly spoken to. His attention was turned away for an instant, and at the moment he was about to make a reply he perceived a perfect sensation of a foreign body on the left side of the pharynx. A practitioner who was at once called in, recognised the foreign body at the spot indicated, and made some vain attempts to extract it. Extremely little pain followed, but as this afterwards increased, he came to the hospital. M. Nélaton suspected from the narrative, as it turned out to be the fact, that no foreign body existed; and cautioned his hearers that they should be very circumspect in making these examinations, as it not unfrequently happens that an unpractised finger mistakes the upper edge of the cornu of the os hyoides for the body supposed to have been swallowed. Usually these nervous symptoms disappear at the end of three months under suitable general treatment; but M. Nélaton referred to a case in which they have manifested much greater tenacity. A lady, about six months since, being about to drink some water sweetened with syrup, not liking the appearance of the latter, placed a single drop on the tip of her tongue, and discovered it to be a solution of potass. Immediately, and notwithstanding that the drop had never been swallowed, she perceived a pain at the lateral part of the pharynx, which was accompanied by an absolute impossibility of swallowing. The pain has diminished, but so difficult does deglutition continue to be, that the patient requires a hour to swallow a simple cup of broth, while the passage of the smallest solid body is still absolutely impossible. It was believed that she was the subject of stricture of the œsophagus, until M. Nélaton being consulted, passed down the largest bougies with great facility.

IV. *On Ranula*. By M. Chassaignac. (Gaz. des Hôp., No. 27.)

M. Chassaignac made some observations upon ranula, in relation to a case occurring in a girl ten years of age. It had only been noticed two months, and exhibited the peculiarity of varying in volume, being sometimes much larger than at others, a circumstance doubtless due to its occasionally opening spontaneously by a minute perforation, and then closing up again. On one occasion it was proposed to defer an operation until the tumour should again become very prominent, when it was found that it could be rendered so by pressure made in the sub-hyoidean region, this being therefore the form of ranula with a tendency to develope itself towards the neck. M. Chassaignac quite rejects the theory of ranula being due to an obstruction of the Whartonian canal, and is inclined to agree with Malgaigne, that it is a cyst developed in an isolated follicle of the buccal glands, or with Fleischmann, that it is a bursa mucosa. Believing that the treatment by seton acts only by keeping the cyst open, and inducing an inflammation which prevents re-formation, M. Chassaignac prefers employing for the same purpose perforated caoutchouc tubes, which allow of the fluid flowing away, and the employment of iodine injections. The tumour is traversed through and through by means of a large, straight trocar, the point of which is received on the finger placed within the mouth. As the instrument is withdrawn, a torsion movement is imparted to the cyst without inducing any laceration, and a perforated tube having been passed through the canula, the latter is withdrawn. The tube is then cut off and fastened, so that it neither impedes speech or mastication.

V. *On Tracheotomy in Croup.* By M. Guersant. (Bull. de Thérapeutique, Nos. 2 and 3.)

When the croup seems to be only one of the signs of a more general affection, acting on the economy as a poison, as manifested by deposition of diphtheritic membranes on various surfaces, by greatly enlarged submaxillary glands and by albuminous urine; when the child is under two years of age (for then recovery is quite exceptional); and when its strength has been exhausted by leeching, blistering, emetics, and other useless treatment, little expectation of success is to be expected from the operation. The mere localized extension of the false membrane into the trachea and bronchi, and the coexistence of other diseases, themselves of a serious character (as pneumonia, typhoïd, exanthems, &c.), are not necessarily contra-indications. Every practitioner should make himself familiar with the operation, as its execution may be urgently required at any moment, and to this end he should not be content with practising it upon the dead body, but should resort to rabbits and dogs, so as to get accustomed to its performance amidst blood and cries. Although, of course, the most suitable instruments must be employed, the blame which operators sometimes attach to these, and the modifications which they are so often making in them, are usually due to their want of practice and skill in handling them. M. Guersant attaches, however, great importance to the use of the double, moveable canula, having himself only saved 2 out of his first 32 patients, when he employed a single canula. Its length and diameter should vary with the age of the subject, from one of 6 millimetres in diameter and 5 centimetres in length for children of one to four years of age to one of 8 millimetres diameter for children from four to eight. The length of the incision through the skin and first aponeurosis should be longer in a child that is fat, as in a thin child the deep-seated parts are more easily reached; and after it has been made, the surgeon should trust more to his finger than his eye. By means of the finger and the director, the subjacent tissues may be divided, and the superficial vessels protected, and the front of the trachea reached without opening these. If, however, there is bleeding which masks the trachea, we must not wait until we can see this clearly, but, feeling for it with the finger, slide the bistoury along this, and puncture the tube with gentleness so as not to transfix it. The finger is to be kept upon the incised point, so as to prevent the ingress of blood, and under it the hooked end of the dilator is slid into the lower angle of the wound. The child is now raised up, and the operator, while expanding the dilator, regains the full use of his eyes. Keeping this *in situ*, a gum-elastic catheter conducting the canula is passed into the wound, and as it enters the dilator is withdrawn. On the strength of more than three hundred operations, M. Guersant feels sure that this is the correct way of passing in the canula.

When too small an opening has been made in the trachea, so that it cannot be detected by the curved point of the dilator, we must not lose time in searching for it, but make another; and when the aperture has been made on one side instead of the front of the trachea, the dilator will enable us to bring the wound towards the mesial line. If the incision in the trachea has been made too long, a long canula must be employed, or it will slip out. Arterial hæmorrhage, if considerable, must be arrested by ligature; but when the bleeding is venous, the trachea should be opened as soon as possible, as the hæmorrhage will then cease. The finger must at first be placed over the incision to prevent the entrance of the blood. When the operation has been completed, a light handkerchief should be laid over the orifice of the canula, as if the air reaches it too directly the mucosities become dried in the canula and impede respiration. The inner canula requires to be often removed, and rapidly cleansed and returned; and when it or the trachea become obstructed by false membranes, these are best removed by means of the crane-billed forceps, which follow the curve of the canula into the air-passages. Expectoration, too, when defective, may be excited by passing a fine crow's feather through the canula, or instilling a drop or two of tepid water, the atmosphere of the room also being kept moist. When the trachea is completely obstructed by mucus or false membranes, the canula should be taken out, and the obstructing body removed by means of the crane-billed forceps. If this be not done, patients in progress of cure may die asphyxiated. At the end of twenty-four or forty-eight hours the external canula should be removed, and the wound cauterized by nitrate of silver, the removal being repeated every or every other day, and the fact whether air passes the larynx being ascertained. The final removal cannot take place until after three or four days at earliest, and frequently only after eight or ten or more, and sometimes, owing to paralysis of the larynx, it has to be retained for an indefinite period. It should, however, always be discontinued as early as possible, as, owing to the nature of its curve, it often causes ulceration of the anterior wall of the trachea. The moveable canula, devised by M Roger, does much to obviate this inconvenience. Sometimes, after the withdrawal of the canula, an inability to swallow supervenes, liquids or solids passing by the larynx and wound. Sometimes patience, and the slow swallowing of only thickish aliments for a while, overcome this inconvenience; but when it continues, or when children persist in refusing to swallow after tracheotomy, we must inject liquid food three times a day through a tube which can be easily passed by the nose into the œsophagus. Above all things after the operation, the strength of the child must be maintained by an invigorating diet.

VI. *On Vesicating Cancer.* By M. Nélaton. (Union Méd., No. 17.)

In M. Nélaton's ward there is an example of the affection to which he has given the name of *cancer vésicant*. It occurs in a woman sixty years of age. The breast is of its normal size, and the affection seems to be exclusively seated in the skin. About a year since a small crust formed on the level of the nipple, and followed exactly the same course as the *noli me tangere* so often seen on the face. The nipple has gradually disappeared, and for some months past the crust has insensibly extended over the entire breast, being at the present about nine centimetres in diameter and circular in form. In the centre, where the nipple was, a superficial ulceration exists resembling that which is presented by the surface of a blister that has long been kept open. The circumference is very exactly defined. The name of *vesicating cancer* has been given to this affection, and M. Robin, from microscopical examination of its surface, has no hesitation in declaring it to be a true cancerous disease. Eczema of the breast, which a superficial examination might confound with it, presents numerous points of difference. It generally attacks both breasts, is characterised by the production of minute vesicles, is attended with itching which is sometimes unbearable, &c. Here we have an entire absence of pruritus or vesicles, a characteristic ulceration in the centre, and at the edges, where the disease is spreading, a small epidermic pellicule exactly resembling that which covers cancroïd at its commencement. Finally, there is the striking fact of a very exact limitation of the disease, so different to the irregular appearance of eczema. M. Nélaton has only met with one case in which this disease has given rise to large glandular swellings in the axilla, which followed the ordinary course of cancerous glands, and occasioned the death of the patient.

VII. *On the Cæsarean Operation.* By M. VAN AUBEL. (Presse Méd. Belge, Dec. 20th.)

M. Van Aubel, in a communication to the Belgian Academy of Medicine, proposes a modification in the performance of this operation, which he believes, by preventing the entrance of pus and blood into the cavity of the peritoneum, will materially lessen the danger of peritonitis. First, an incision is to be made along the median line comprising only the skin and fatty tissue down to the aponeurosis, from which these layers are to be separated by careful dissection to the extent of half-an-inch. The aponeurosis, with the peritoneum, is next incised, and then the uterus, care being taken during the extraction of the child to maintain the two serous surfaces in contact. This accomplished, the visceral peritoneum is to be dissected to the extent of half-an-inch, including with it as thin a layer as possible of the muscular substance, the two serous surfaces being kept closely applied to each other during the dissection, in order to prevent effusion of blood into the peritoneum. The wound and uterine cavity are to be cleansed with the greatest care. To the two dissected flaps of the uterus the sutures employed by Gély in wounds of the intestine are to be applied, serous membrane being thus brought into contact with serous membrane. The two lips of the divided peritoneum and aponeurosis are to be carefully brought into contact, and the two serous membranes united by means of the glover's suture. The external wound is to be united by the interrupted suture, and dressed as an ordinary wound. In this way the author believes adhesive inflammation and complete closure of the uterine and peritoneal cavities may be secured. To the objection that the dissection required renders the operation more difficult, the author replies that with a little additional time all proves easy enough; but at present he only speaks from dissecting-room experience.

VIII. *On Glaucoma.* By Dr. LEIBREICH. (Gaz. des Hôp., 1863, No. 152.)

In a lecture upon glaucoma, Dr. Leibreich thus expresses himself respecting iridectomy, about which we have had of late so sharp a controversy: "I say that this procedure cures all forms of glaucoma, and that in this sense it has far surpassed the hopes of Von Graefe, announced at first with much caution; but I do not mean by that to say that by its aid we can cure every case of glaucoma, no matter at what stage the disease may have arrived. On the contrary, the prognosis is very different according to the period of the disease. Iridectomy induces the disappearance, why we know not, of the inflammatory symptoms and of the increased intra-ocular pressure; and the effect of the operation is the more striking in proportion as these symptoms are more marked. Perform it in cases of hyperacute glaucomatous choroiditis, without fearing to add an artificial lesion to an existing ophthalmia, and you will find all the inflammatory symptoms disappearing, and the well-nigh extinct vision returning to its normal state—a result not obtainable by the most energetic employment of every description of antiphlogistic. But you must not hesitate nor wait until the intra-ocular compression, cutting the continuity between the retina and the nerve, works its mischief on the papilla of the optic nerve. There are cases, happily not common, in which the effect of the disease is so violent that this destruction may be produced after some weeks; and in such cases it is of importance to perform the operation, if possible, during the early days, for every day's delay exerts its effect upon the favourable result. In cases in which the inflammation pursues a slower course, after the operation you find the periodic exacerbations of the disease disappear, the patient preserving his actual amount of vision, or even regaining that which he had latterly lost, especially in the attack during which you operated.

In cases of simple glaucoma, unaccompanied by manifest inflammation, you must remain contented with preserving for the patient the vision he still possesses, and saving him from that complete and incurable blindness with which without this operation he would be certainly menaced. The prognosis is very different, then, according to the period at which the operation is performed, and it must be based especially upon two points. It will be favourable, all things being equal, in proportion to the amount of inflammatory action and intraocular pressure present; and to the remaining integrity of the field of vision, the patient only regaining that which he has but recently lost. An ophthalmoscopic sign is of great importance in deciding prognosis, and this consists in an extremely delicate line which constitutes the limit between the remainder of the nervous substance and the portion of the vitreous body involved in the excavation of the pupil. The observation of this line enables us to appreciate the amount of nervous fibres which have resisted the destructive effect of the disease, and the amount of retina yet remaining intact.

SUMMARY.

Acupressure.—Hamilton on Employment of Acupressure. (Edinb. Med. Journ., Jan. With a Discussion.)

Amaurosis.—Lancereaux on Amaurosis from Degeneration of Optic Nerves in Cerebral Disease. (Archives Gén., Jan. and Feb.)

Amputation.—Porter on Amputation through the Condyles of the Femur. (Dublin Journal, Feb.)—Schuh on Cases of Amputation by Gritti's Method. (Wien. Wochenschrift, No. 1. This consists in sawing through the condyles of the femur, and adapting the patella as a flap.)—Beck, Statistics of Amputations and Excisions. (Langenbeck Archiv, vol. v. No. 1. A statistical account of 74 cases of amputation, 77 disarticulations, and 44 excisions, or a total of 195 cases, 22 of the number proving fatal, as a consequence of the operation.)

Aneurysm.—Gayet, a Case of Arterio-Venous Aneurysm. (Gaz. Hebd. No. 11. A well detailed case, with autopsy, of an example of aneurysm involving the femoral profunda artery and vein.)—Corneo, Case of Subclavian Aneurysm. (Gaz. Med. Lombardia, No. 3 and 4. A case of traumatic aneurysm of the subclavian, for which the subclavian and common carotid were simultaneously tied, fatal secondary hæmorrhage occurring eight days afterwards.)

Anus.—Lemaistre. (Gaz. des Hôp., No. 14. Case of Imperforate Anus successfully treated by the Trocar.)

Cataract.—Küchler, Application of the Binoculus after Extraction. (Deutsche Klinik, 1863, Nos. 47, 48.)

Dislocation.—Holm, Cases of Paralysis after Dislocation of the Humerus. (Schmidt's Jahrb., Jan. p. 82.)

Ear—Triquet, Discharges of Blood from the Ear. (Gaz. des Hôp., No. 7. A review of the various circumstances under which this takes place.)—Lucæ, Contributions to the subject of Disease of the Ear. (Virchow's Archiv, vol. xxix. No. 1. An elaborate essay on the pathology of the ear, with critical remarks on the works of Toynbee, v. Troeltsch, and Voltolini.)

Elephantiasis.—Ballingall, Operation for Elephantiasis Scroti. (Trans. of Bombay Med. Soc., No. 8, p. 231. In continuation of a former paper, and referring to 10 additional cases, making in all 24 cases of operation with 2 deaths.)

Excision.—Porter, Case of Excision of the Ulna. (Dublin Journal, Feb.)—

Lehmann, Preservation of the Teeth in Excision of the Alveolar Process of Upper Jaw. Deutsche Klinik, No. 3.)—Hueter, Longitudinal Incision for Excision of the Knee-joint. (Langenbeck Arch., vol. v. No. 1.)—Creus y Manso, Subperiosteal Excision. (Gaz. Hebd., No. 2. Case in which, for osteitis of the tibia, the whole of its diaphysis was successfully removed by subperiosteal excision.)

Fingers.—Eultenberg, Contraction of the Fingers. (Deutsche Klinik, 1863, No. 50.)

Fracture.—Caspary, Division of Malformed Callus. (Langenbeck Archiv, vol. v. No. 1. The author adopts a modification of A. Wagner's procedure, and gives a view of an extension-bed.)

Glaucoma.—Homberger, Epilepsy of the Retina in relation to Glaucoma. (Amer. Med. Times, Nos. 5 and 7.)

Hernia.—Guyton, Mechanism of Strangulated Hernia, and the employment of Chloroform in its Reduction. (Gaz. Méd., Nos. 4, 5, 7, and 9. The author believes that strangulation is chiefly due to the exaggerated action of the abdominal muscles, and regards chloroform as a useful agent in overcoming this. He gives a few cases.)—Debout, Support of Hernia in Young Children. (Bull. de Thérap., Jan. 15. Debout objects to the application of vulcanized caoutchouc as causing irritation and too much constriction. Whenever it is possible, spring-trusses should be employed, for, if carefully applied, they will effect a cure in a few months.)

Iridectomy.—Homberger, New Method of Performing Iridectomy. (Amer. Med. Times, vol. vii. No. 25.)

Iridodesis.—Alfred Graefe, Iridodesis. (Archiv f. Ophth., vol. ix. No. 3. —Warlomont, Iridesis, and its Application to Central Cataract. (Annales d'Oculistique, Jan.)

Jaw.—György, Closure of the Jaw. (Wien Wochenscrift, 1863, Nos. 45-50. Figures an instrument for opening the jaw in anchylosis.)

Joints.—Rauchfuss, Inflammation of the Joints in Young Children. (Petersburg Med. Zeitsch., 1863, No. 10. Illustrated by 17 cases occurring in the author's practice.)—Klein, Loose Cartilages in the Joints. (Virchow's Archiv, vol. xxix. No. 1. A fatal case, following extraction, is given in this paper.)

Lachrymal Tumour.—Lacaze, New Mode of Treating Lachrymal Tumour and Fistula. (Union Méd., No. 9. This consists in injecting a few drops of iodine after incision.)

Laryngoscope.—Elsberg, New Instrument for Laryngoscopy, Rhinoscopy, and Operations on the Oral Cavity. (Amer. Med. Times, vol. vii. No. 26.)

Leprosy.—Carter on Symptoms and Morbid Anatomy of Leprosy. (Trans. Bombay Med. Soc., No. 8.)

Lithotrity.—Nélaton and Civiale on Lithotrity. (Gaz. des Hôp., Nos. 1, 5, and 13. Nélaton's protest against the injustice of Civiale's comments upon the mode of performing lithotrity by other surgeons, and Civiale's rejoinder.)

Œsophagotomy.—Sonrier, Case of Œsophagotomy. (Gaz. des Hôp., No. 17.)

Ophthalmoscope.—Knapp, Advantages of the Binocular Ophthalmoscope. (Annales d'Oculistique, Jan.)

Osteomyelitis.—Duguet, Case of Spontaneous Myelitis, followed by Fatal Purulent Infection. (Union Méd., No. 4.)

Palate.—Szymanowski, Adhesion of the Soft Palate to the Pharynx. (Prag. Vieteljahr., No. 1. Szymanowski gives an account of 3 cases, which have occurred in his own practice, and figures a knife suited for operating detachments. He refers to 5 other published cases.)

Plastic Operations.—Debout, Facial Autoplasty by the Indian Method. (Bull. de Thérap., Dec.)—B. Langenbeck, New Method of Executing Total

Rhinoplasty. (Berlin Med. Wochenschrift, No. 2) Cochu, Periostic Uranoplasty. (Gaz. Hebd., No. 1.)—B. Langenbeck, Additional Experience in relation to Uranoplasty. (Langenbeck Archiv, vol. v. No. 1.)—Notta, Operation for Restoration of the Subpubic portion of the Urethra. (Union Méd., No. 27.)

Polypus.—Damon, Polypus of Rectum in Children. (Boston Med. Jour., vol. lxix. No. 15)—Delore, Naso-Pharyngean Polypi. (Bull de Thérap., vol. lxv. pp. 349, 397, 507.)—Turck, New Modification of Polypi Forceps. (Allg. Wien. Med. Zeit., 1863, No. 49. For the removal of polypi of the larynx.)

Pyæmia.—O. Weber, Origin and Treatment of Icorræmia or Pyæmia. (Langenbeck Archiv, vol. v. No. 1.)

Skull.—Stephen Smith, Contusion of the Skull-bones. (American Med. Times, Nos. 1, 3, and 5.—A case is related in which a portion of the frontal and parietal bones separated, measuring eight inches by four and five.)

Strabismus.—Knapp, Results of the Operation for Strabismus. (Annales d'Oculistique, Jan. Knapp reports very favourably upon the ultimate results of this operation, founding his observations on 70 cases which have come under his care at Heidelberg.)—Meyer on a Strabometer. (Archiv für Ophthal., vol. ix. No. 3.)

Suspensory.—Milano, Graduated Compression Suspensory Bandage. (American Med. Times, Jan. 9. In this ingenious bandage the pressure can be easily regulated in amount and direction, according to the progress of the case.)

Syphilis.—Scarenzia, Contributions to Syphilitic Diseases of the Nervous System. (Omodei Annali Universali, Dec., p. 577. Fourteen cases of syphilitic retinitis and retino choroïditis.)

Testis.—Debout, Truss with a Bifurcated Pad in Delayed Descent of the Testis. (Bull. de Thérap., Jan.)

Trachea.—Bourdon and Moissenet, Two Cases of Stricture of the Trachea. (Union Méd., No. 10 and 22.)

Tracheotomy.—Buchanan, Cases of Tracheotomy in Croup and Diphtheria. (Glasgow Med. Journ., Jan.)—Barbosa, Tracheotomy in Croup. (Union Méd., No. 23. Barbosa speaks favourably of this operation as practised at Lisbon.)—Spence, Tracheotomy in Diphtheritic Croup. (Edin. Med. Journ., March.)—Porter, Excision of a Portion of the Windpipe in Tracheotomy. Dublin Journ., Feb.)—Demarquay, Tracheotomy in the Adult. (Union Méd., No. 21.)—North on Bronchotomy. (American Med. Times, vol. vii. Nos. 21-23. A statement is given of the results of 43 cases, chiefly American.)

Transfusion.—Blasius, Statistics of Transfusion of Blood. (Deutsch Klinik, 1863, Beiträge No. 11. A tabulated statement of 116 cases is furnished.)

Vesico-vaginal Fistulæ.—Ulrich, Cases of Vesico-vaginal Fistulæ. (Wochenblatt d. Wien. Gesellschaft, No. 49-52.)—E. Rose, The Operation for Vesico-vaginal Fistula. (Berlin Charité Annalen, vol. xi. No. 3. A critical review, with 22 original cases and lithographs.)

QUARTERLY REPORT ON MIDWIFERY.

By **Robert Barnes**, M.D. Lond., F.R.C.P.
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I. The Unimpregnated State.

1. *The Source of Hæmorrhage in a Case of Fibrous Tumour of the Uterus.* By Dr. Matthews Duncan. (Edin. Med. Journ., Jan. 1864.)
2. *A Case of Inversion and Excision of the Unimpregnated Uterus.* By Drs. Keiller and J. G. Wilson. (Ibid.)
3. *The Surgical Treatment of Amenorrhœa.* By Dr. H. R. Storer. (Amer. Journ. of Med. Science, Jan. 1864.)
4. *On the Curability and Means of Cure of Chronic Metritis.* By Professor Faye. (Monatsschr. f. Geburtsk., 1863.)

1. Dr. Matthews Duncan describes the autopsy of a patient who died of hæmorrhage from fibrous tumour of the uterus. The source of the hæmorrhage was revealed. A globular fibrous tumour, three inches in diameter, occupied the fundus uteri. Venous sinuses that would admit a small crow-quill were seen permeating it in various directions. It was everywhere surrounded by a covering of the proper tissue of the uterus. In the layer of uterine tissue covering the inner aspect of the tumour, there was developed a reticulation of enormous uterine sinuses, such as are observed in the same part in pregnancy. About the centre of the part projected by the underlying tumour, was a little clot protruding through a small round opening. The opening was $\frac{1}{16}$″ in diameter, a probe could be easily passed through it into the uterine sinuses. Dr. Duncan observes, that the reason why these sinuses do not bleed even more freely than they do, is because they are generally so flattened that their walls are in contact.

2. Dr. J. G. Wilson reports a case of an irreducible inverted uterus. The inversion occurred in a middle-aged woman, who had not been pregnant for several years. The tumour showed signs of commencing gangrene, and the constitutional symptoms were severe. The tumour was therefore removed by écraseur. She appeared to be doing well for eleven days when, receiving suddenly bad news, prostration returned, and she died next day. Examination showed that the uterus had been amputated at the juncture of the lower and middle thirds, and that by far the larger portion of the mass removed consisted of a fibroïd tumour which had grown at the fundus.

3. In his paper on the Surgical Treatment of Amenorrhœa, Dr. Storer describes a class of cases in which the uterus is imperfectly developed. In these, and in other cases, it is indicated to stimulate the lining membrane of the uterus. For this purpose he advocates the use of a galvanic intra-uterine pessary. The stem of this should be made of copper and zinc. The amount of action produced, he says, is shown by the fact that, upon withdrawing the instrument from the uterine cavity, while the copper portion remains almost entirely free from deposit, that of zinc is found encrusted with a thick layer of foreign matter, which, upon chemical analysis, resolves itself into the metallic salt usual under similar circumstances.

4. Professor Faye, referring to the common experience of the difficulty of cure of chronic metritis, explains the treatment he adopts. He enjoins abstinence from sexual intercourse; he especially commends the use of "Neptune's girdle"—i.e., the cold-water belt; occasionally leeches to the

cervix uteri; a daily application of a bolus formed of mercurial ointment, iodide of potassium and extract of belladonna; douches of water or oil; small lavements, consisting of cod-liver oil, and five or six drops of tincture of iodine, tincture of iron, and laudanum. When the patient can get about, he advises the use of an abdominal belt provided with a perineal pad. When there is intumescence, with obstinate catarrh, he has injected water and glycerine with small additions of iodine and iron, with decidedly good effect. When there is hypertrophy of the cervix and ulceration, he speaks well of the potassa cum calcê.

II. PREGNANCY.

1. *A Tubo-Uterine Gestation: Transmigration of the Ovum.* By Dr. B. S. SCHULTZE. (Würzb. med. Zeitsch., 1863.)
2. *Case of Extra-Uterine Pregnancy.* By Professor SIMPSON. (Edin. Med. Journ., Jan. 1864.)
3. *Cases of Extra-Uterine Gestation, with Polypi of the Uterine End of the Fallopian Tube.* By Prof. BRESLAU. (Monatsschr. f. Geb., 1863.)
4. *Saccular Enlargement of the Posterior and Lower Segment of the Uterus.* By Dr. W. FRANKE. (Monatsschr. f. Geb., 1863.)
5. *The Glands of the Decidua and the Hydrorrhœa Gravidarum.* By Dr. A. HEGAR. (Monatsschr. f. Geb., Dec. 1863.)
6. *Contributions to the Pathology of the Ovum, and of Abortion in the Early Months of Pregnancy.* By Dr. A. HEGAR. (Ibid.)

1. Dr. Schultze's case of tubo-ovarian gestation occurred in a woman aged twenty-two. She had her last menstruation in June, 1862. On the 30th of March, 1863, pains set in. On the 31st of March, an attack of convulsions and sopor; then returns of consciousness; attacks were repeated; fœtus presenting in first position; urine, albuminous. Patient died undelivered, in collapse. Cæsarean section performed ten minutes afterwards. About a pint of watery blood flowed from peritoneum when cut into; uterus flaccid, without trace of contraction; child was dead. When child was extracted, the uterus contracted. The placenta adhered to the posterior wall of the uterus. There was an aperture through the uterus which had permitted the extrusion of a foot of the fœtus. The right tube terminated towards the abdomen in a closed sac, showing no trace of fimbriæ, and covered with numerous pseudo-membranous cords. The peritoneal insertion of the tube in the uterus was four lines distant from the right edge of the uterine rent. The left ovarium contained a corpus luteum at its outermost angle. The left tube was adherent to the uterus by delicate adhesions throughout the outer third of its course; it was impervious throughout this portion. Dr. Schultze gives the following interpretation of the case, which is made more clear by a drawing of the parts. The seat of the rent showed a deficient muscular coat, forming a sort of pouch, as if out of a dilatation of the uterine end of the right Fallopian tube. This pouch had been covered by decidua and peritoneum, and had only burst during labour. This pouch was the seat of the early development of the ovum, which had been received from the left ovary and tube, and had crossed over the fundus uteri towards the saccular dilatation formed by the uterine mouth of the right tube. The complete closure of the abdominal end of the right tube must have entirely excluded the reception of the ovum in this tube; but the obstruction of the outer third of the left tube might have occurred subsequently to the passage of the ovum from the left ovary, which contained the corpus luteum.

2. Dr. Simpson's case of extra-uterine gestation concerned a woman, aged thirty-three, who had borne six children. She menstruated on the 25th of

December, 1862. Six weeks afterwards she had symptoms of peritonitis. Similar attacks were repeated until April, 1863, when she died. Dr. Simpson saw her on the 26th, suffering from a paroxysm. Behind the cervix was a fluctuating tumour; the fœtal heart was heard over abdomen. A puncture was made in recto-vaginal space by trocar, in the hope of evacuating the liquor amnii, and effecting the death and decomposition and expulsion of the fœtus. She died two days later.

Autopsy.—Peritonitis. The fœtus lay in a pouch formed by enlarged uterus, broad ligaments, pelvic walls, and sigmoïd flexure of colon. The ovaries could not be traced. The fœtus was of six months' development. The placenta was attached to posterior surface of the uterus, and other parts forming the cyst.

3. Dr. Breslau relates two cases of extra-uterine gestation:—

(1.) A woman, aged thirty, fell ill six months after marriage, with symptoms of abdominal hæmorrhage, and died in six hours. Considerable effusion of blood was found in the abdominal cavity. At the point of entry of the left tube into the uterus, half belonging to the tube and half to the uterine parenchyma, was an empty reptured sac, still showing chorion-villi mixed with coagulated blood. The embryo was not found. The internal uterine mucous membrane did not exhibit the common hypertrophied or hyperplastic condition known as decidua. But just below the uterine opening of the left tube was a thin-stalked, oval, mucous polypus, the size of an orange-pip. The author believes this polypus obstructed the entry of the ovum into the uterus. The closure of the Fallopian canal was not complete.

(2.) Dr. C. F. W. Uhlig found in the body of a woman aged sixty, a contracted cicatrix in the right lumbar region. The abdomen was distended, and in the right lumbar region was felt a large hard tumour. The uterus was hypertrophied, and attached to it was a tumour the size of a child's head, and in its cavity were several polypous excrescences of the mucous membrane; at the opening of the right tube was a polypus, which, however, permitted a fine sound to pass. Just below this opening was a second opening leading into the tumour attached to the uterus. In this tumour was a mummified embryo the size of a 4-5 months fœtus. It was encrusted with chalk-like matter, as well as the sac itself. The woman had borne a child in her thirty-fourth year, had several times aborted, the last time when fifty years old. About twelve years before death an abscess had broke in the seat of the cicatrix.

4. Under the title of Saccular Enlargement of the Posterior and Lower Segment of the Uterus, Dr. Franke describes a condition in which, towards the end of gestation, the fundus uteri is found in the normal situation, the os uteri high up behind the pubes, and the posterior segment expanded out into a sac filling the Douglasian space.

[It seems, from the description and references to be inferred, that this sacculation takes place at an advanced period of pregnancy. It is, however, more in accordance with the Reporter's observations, that in these cases there is original retroversion, and that the part which grows secondarily is the fundus or anterior wall of the uterus, which is developed upwards in the abdomen, leaving the original retroverted portion in the pelvis.—R.B.]

In the other case, occurring in September, 1862, the patient had been treated at four months' gestation for acute retroversion of the uterus. There was a large smooth tumour filling the hollow of the sacrum. It was slightly moveable. By compressing it, it became possible to reach the child's head. The Cæsarean section was performed; a copious venous hæmorrhage attended, and a large coil of small intestines came forward. Peritonitis followed, and extreme meteorism. The meteorism was relieved by the œsophageal tubes, and by passing a tube per rectum. The patient died on the seventh day. There was found gangrenous peritonitis, no trace of cicatrization of the uterine wound. The pelvic tumour was as large as a child's head, and was of fibro-cellular structure. It must have grown within four months and a half, since, when the reposition of the retroverted uterus was performed by the author himself, nothing was found of it.

5. Dr. Alfred Hegar has carefully investigated the pathology of hydrorrhœa gravidarum. He begins with an anatomical investigation of the condition of the glands of the uterine mucous membrane or decidua during pregnancy. Examination of various ova expelled at different periods of gestation, satisfied him:

(1.) That the glands of the decidua vera are still found in the fourth month as simple canals provided with well-preserved epithelium, and are seen as string-like formations on the rough surface of the detached membranes in aborted ova. In the serotina the glands in the vicinity of the placental-margin were formed up to the third month

(2.) In the fifth and sixth months the glands of the decidua vera were present in the same outward form. They were less thickly-set. The interior showed no obvious epithelium. But since only deciduæ, affected with advanced premature fatty metamorphosis, fell under examination, it is probable that perfect glands are present in the vera in the fifth and sixth months.

(3.) In the middle layer of the vera, the glands run in spiral windings, like sweat-glands.

(4.) In that layer of the mucous membrane which lies next to the free surface, the presence and course of the glands is the most difficult to recognise.

(5.) The mucous membrane of the uterus separates itself, in abortion, or through artificial detachment, in sections of very different thickness even in the same pregnancy.

(6.) The hydrorrhœa gravidarum depends upon a hyperæmia, and increased vascularity of the uterine mucous membrane. Especially are the glands very numerous and large. The copious effusions are connected with this anatomical condition.

(7.) The decidua vera is to be regarded even in, and after, the middle of gestation, as a working secreting organ. In favour of this view, speak the gradations from the small degrees of hydrorrhœa, and the false waters to the most marked forms.

In two well-observed cases related in detail, it is recorded that hæmorrhages alternated with, or coincided with the discharges of water.

6. Dr. Alfred Hegar contributes an elaborate memoir on the pathology of the ovum in relation to abortion. He commences by a summary of the normal anatomy of the maternal and embryonic membranes. In this part there is nothing to arrest attention. He then describes the pathological changes observed by him in the examination of various aborted ova. The decidua vera is liable to atrophy, hypertrophy, cystic formation, blood-extravasations, and exsudations. The cystiform alterations are most deserving notice. He found on several aborted ova small sacs on the outer surface of the decidua vera filled with a yellowish-colloïd mass, and unaccompanied by any other change in the tissues of the membrane. In other cases there was concurrent excessive development of the glands. He ascribes this formation of cysts and the hypertrophied glands to a catarrhal inflammation of the uterine mucous membrane. In the case referred to, there had existed before and after conception a catarrhal inflammation of the mucous membrane of the uterus.

The blood-extravasations in the decidua are well known. Hegar describes a form of extravasation associated with cyst-like formations of the decidua. He observes that the reflexa and serotina at times exhibit like conditions. He also makes the interesting observation, that the mucous membrane of the non-pregnant uterus may, in like manner, suffer apoplectic disorder. He says, this is a cause of obstinate and painful dysmenorrhœa; that at the time of menstruation, it is not a simple effusion of blood from the surface of the membrane that takes place, but also there is extravasation of blood in the deeper layers, which brings about a detachment of the membrane. This Hegar calls dysmenorrhœa membranacea apoplectica. He has also observed this apoplectic destruction of the uterine mucous membrane in the obstinate floodings of the climacteric age.

The blood-extravasations of the reflexa he distinguishes—1. As extravasations on the outer surface. 2. Between reflexa and chorion. 3. In the parenchyma.

Hypertrophy of the serotina leads generally to abortion. The development of the uterus maintains no equal pace with the growth of the ovum. Draggings of the utero-placental vessels, extravasations and detachment of the ovum ensue. The serotina is the most frequent seat of primary blood-extravasations, otherwise blood-extravasations are seldom the primary causes of abortion. They seldom arise from sudden hyperæmia. Commonly, long-standing abnormities of the decidua and of the proper fœtal structures, precede the bleedings. Among these conditions, Hegar reckons abnormally small and abnormally large circumference of the serotina, atrophy of this membrane, and hypertrophic development of the glandular structure.

With regard to the diseases of the chorion, Hegar refers to one case of hydatiginous degeneration. The decidua, he says, was in the highest degree atrophied. It appears to him very probable, that a cause of cystic degeneration lies in this, that the chorion-villi shoot into a diseased decidua.

[This conjecture is, however, quite opposed to the observations of the Reporter. See this Review, 1855-6.]

Hegar always found the amnion present. The cavity, excepting its proper fluid contents, was generally empty. At times there were found thin membranous bridges or threads between the walls, or freely floating. In 5 cases the cavity contained a small umbilical cord, without trace of embryo. In 2 cases a small embryo was found. A well-preserved fœtus, whose development corresponded to that of the entire ovum, and of the estimated duration of pregnany, was observed once only.

The memoir concludes with the detailed narrative of 14 cases of abortion, and descriptions of the ova. Several excellent lithographic representations of diseased ova are given.

III. Labour.

1. *Two Cases of Variety of Form in the Uterus, with Pregnancy.* By Dr. Birnbaum. (Monatsschr. f. Geburtsk, 1863.)
2. *Report of Two Cases of Inversio Uteri.* By A. L. Kelly, M.D. (Dublin Medical Journal, January, 1864.)
3. *A Case of Missed Labour.* By Dr. McClintock. (Dublin Quarterly Journal of Medicine, February, 1864.)

1. Dr. Birnbaum describes two cases of pregnancy in which there was a remarkable variation from the normal form of the uterus.

Case 1.—Twin pregnancy, with uterus bicornis unicollis. A primipara, aged twenty-six. At the end of pregnancy, the greatest prominence of the uterus was in the region of the umbilicus, which was very much pointed and protruded. Above this point, the uterus presented a marked saddle-like depression, so that the whole uterus was heart-shaped. One division of the uterus was traced rising up from the saddle-shaped depression high in the left hypochondrium, forming a sharper fundus. A second division could be traced into the right hypochondrium, but it did not rise so high as the left division, and the fundus was broader. A head presented in the much-thinned lower segment, deep in the fibres. The excessive quantity of liquor amnii prevented tracing the other fœtal parts; but the presence of twins—one in the right horn, the other in the left—could not be doubted. The pains first acted by lessening the bulk of the right uterus; the waters had escaped. The expulsion of the first child was painful and tedious, in spite of energetic contractions. A live girl, weighing 5¾lbs., was born. The second child followed in ten minutes, the left horn of the uterus coming more into the centre, and contracting. The second child, also a girl, weighed 5¾ lbs. The placenta required the introduction of the hand to extract it. It was single, with common chorion, and divided amnion. The uterus maintained, so long as it was observed during the puerperal state, its broad and centrally-depressed form.

Case 2.—The pregnancy was simple, but the form of the uterus was much more striking and peculiar. The subject was a healthy primipara. There was seen, as well as felt, a complete division of the uterus into a large, lower, but broadly-arched part on the left, and a smaller one seated obliquely in the right; this was above the other, and was ball-shaped. The first division was estimated to be of the size of a uterus in the eighth month of gestation; the other of one about four months pregnant. The head lay on the lower segment of the uterus, which was much thinned, the cervix much shortened, and directed to the right and backwards. When labour set in, both divisions of the uterus contracted together, the double form being rendered more distinct. The membranes had to be ruptured artificially. Labour was still difficult; the patient was bled, and the forceps used. Free hæmorrhage attended; cord three times round child's neck; child stillborn. The placenta required introduction of hand to extract it. The irregular shape of the uterus was observed during the puerperal period.

Dr. B. points out the relation of these cases to cases of tubo-ovarian gestation.

2. Dr. Kelly relates two cases of inversion of the uterus.

(1.) A young woman, aged nineteen, in first labour, which was lingering so as at length to require aid by forceps. A large gush of blood followed delivery of child. Sighing, restlessness, weak pulse, prostration followed. The uterus was found quite inverted, the placenta adhering. The reduction was effected by pressure with the hand doubled up, without detaching the placenta. This was separated afterwards. Recovery was uninterrupted.

(2.) Attended in labour by a midwife, who admitted having "pulled the cord, and felt it yield." Hæmorrhage not considerable; pulse small; general prostration; pain in belly; felt "just going to die." The placenta was found attached to the inverted uterus. Steady pressure effected reduction; the fundus went up with a sudden and distinct bound; the placenta was then withdrawn. Recovery.

3. Dr. McClintock relates a case of "missed labour." The patient, aged forty-five, suffered from a "most horribly offensive" and very profuse discharge fourteen months after her last labour. There was no hæmorrhage. The uterus was then of the size of one four months pregnant. Bone was diagnosed by the uterine sound, and afterwards by the finger passed into the cervix. It appeared that she had conceived very soon after her last labour, that the fœtus probably died at seven months, that effort at labour set in at the end of the ninth month, but without expulsion of fœtus. Five weeks after, a bone was found in the vagina. She was admitted into hospital, and repeated attempts were made to remove the bones. She died under symptoms of pyæmia. No post-mortem examination was permitted.

IV. The New-born Infant.

1. *A Contribution towards the Verification of the Law of Hofacker and Sadler concerning the Relation of the Sexes in relative Difference of Age of the Parents.* By Professor Breslau. (Mon. f. Geburtsk., 1863.)
2. *Case of Labour in which a Child born by the head died after breathing, although the trunk was extracted immediately.* By Dr. Edward Martin. (Mon. f. Geburtsk., Sept. 1863.)
3. *On Catheterization of the Trachea in Asphyxiated New-born Children.* By Dr. V. Hüter. (Mon. f. Geburtsk, Feb., 1863.)

1. Professor Breslau has submitted the law concerning the relative proportion of the sexes laid down by Hofacker and Sadler to examination. This law may be thus expressed: (1) If the father is older than the mother—the common condition—there will be more boys than girls; (2) If the parents are of equal age, there will be fewer boys than girls; (3) If the mother is older than the father, there will be more girls than boys. The following are the results of Dr. Breslau's analysis of the birth-register of the Canton of Zurich for the year 1861:

Total births.		Father older than mother.		Father and mother of equal age.		Father younger than mother.	
B.	G.	B.	G.	B.	G.	B.	G.
4172	3912	2955	2842	297	288	920	782
or		or		or		or	
1066 to 1000		1939 to 1000		1031 to 1000		1176 to 1000	

These results are at variance with the law expressed above.

In a second contribution, Professor Breslau analyzes the births in the Canton of Zürich for the year 1862. The following is a summary of his results:

Total of all births.		Father older than mother.		Father and mother same age.		Father younger than mother.	
16,492		11,762		1201		3529	
B.	G.	B.	G.	B.	G.	B.	G.
8561	7931	6069	5693	623	578	1869	1660
=		=		=		=	
1079 : 1000		1066 : 1000		1077 : 1000		1125 : 1000	

Hence it appears that, under all relations of ages of parents, there is excess of boys over girls. These births occurred during 1862 in the Canton of Zurich.

2. In Dr. Martin's case, which is of considerable forensic importance, a child was born by the head by natural labour; the face, as is not uncommon, became cyanosed; breathing took place; and, although the trunk was immediately released, the child died. The cord was round the neck. Dissection showed a considerable effusion of blood on the posterior quarter of the right parietal; the longitudinal sinus filled with blood; vivid injection of the dura mater; at the basis of the brain, under the tentorium cerebelli, about two drachms of fluid blood; the firm brain-substance throughout marked by distended vessels; the grey substance very hyperæmic. The death of this child is attributed to cerebral apoplexy occurring after breathing, in consequence of pressure of the cord round the neck.

3. Dr. V. Hüter, in an elaborate memoir, discusses the subject of asphyxia in new-born children. He lays particular stress upon the circumstance, now

sufficiently proved, that when the function of the placenta is interrupted before the child is born, inspiratory efforts are made, which have for effect the sucking-in to the air-passages of liquor amnii, meconium or genital mucus. The first step towards respiration, he contends, is to clear away these impediments. This he does by introducing a flexible catheter into the trachea, and then by aspiration performed by his own mouth. In the second degree of asphyxia in which respiratory movements occur, this clearance is all that is necessary; but in the third degree, where there is nothing but feeble heart-pulsation, artificial respiration by blowing air into the chest, may be necessary. [The use of the tracheal tube for this latter purpose has long been familiar in this country; but the preliminary aspiration for the purpose of clearing out the air-passages, although previously insisted upon, is the logical indication drawn from the modern researches, which show that in asphyxia there have been intra-uterine efforts at inspiration causing the ingestion of fluids.]

The following papers are indicated by title on account of their general interest. They are not analyzed either from want of space, or because they are published in journals readily accessible to the English reader.

The Forceps in Craniotomy. By Thomas Powell. (Dublin Quarterly Journal of Medicine, February, 1864.)

An account of 1206 Midwifery Cases, in 977 of which the Forceps was used 101 times. By Dr. M. Ryan. (Dublin Quarterly Journal of Medicine, February, 1864.)

On the Mechanism of Labour in much-Contracted Rhachitic Pelves. By Dr. Feist. (Monatsschr. f. Geburtsk., 1863.)

Case of Prolapsed Uterus: Removal of the entire Organ. By T. Francis Edwards, Esq., of Denbigh. (British Medical Journal, February, 1864.) The case was one of complete procidentia. The uterus had often been replaced, but at last this became impossible, and the uterus threatening to sphacelate, it was removed by ligature. The patient made a perfect recovery.

The Operation for Vesico-Vaginal Fistula. By Dr. E. Rose. Annalen d. Charité, Berlin, 1863.) In an elaborate memoir, illustrated with several engravings, Dr. Rose relates the histories of 22 cases, in which operations were performed with success.

On the Measurement of the Fœtal Head. By W. M. Turnbull, M.D. (Australian Medical Journal, July, 1863.) Dr. Turnbull gives results of measurements of fifty fœtal skulls at birth.

Case of Puerperal Convulsions treated by Chloroform and Morphia. Recovery. By R. T. Tracy, M.D. (Ibid.)

Remarks on Four Cases of Placenta Prævia. By G. Clendinning, M.B. (Ibid.)

Obliquely Distorted Pelvis in consequence of injury. By Prof. Laforgue, of Toulouse. (L'Union Méd., 1863.)

Hypertrophic Elongation of the Cervix Uteri of twenty-six years' standing, with projection of the enlarged os beyond external organs: Ulceration, Hæmorrhage, Operation, followed by complete Cure. By Dr. Jackson Warren. (American Journal of Medical Science, January, 1864.)

CONTRIBUTIONS TO MEDICAL LITERARY HISTORY.

The want of some convenient receptacle for fragments of medical literary history appears to have been often felt, and various attempts have been made to supply the deficiency.[1] The latest works of the kind on a large scale were the original 'Janus,'[2] edited by the late Dr. Henschel, of Breslau, and the second series of the same work,[3] edited by Drs. Bretschneider, Henschel, Heusinger, and Thierfelder. Both of these were very valuable repositories for contributions to medical literature, (though of course it could not be expected that all their contents should equally suit the tastes of all their readers,) but it was found impossible to continue the publication on account of the limited amount of support which they received; and accordingly the experiment may perhaps be considered to have proved the impossibility of carrying on a journal devoted *solely* to medical literature. But the want expressed above still continues to exist, and because of it there is no doubt that many valuable scraps of medical literary history are lost beyond recovery. In attempting to furnish some such receptacle, we propose to devote occasionally a few pages of this Review to medical history, biography, and literature, as well ancient and mediæval as modern; and we believe that in doing so we shall be carrying out one of the objects for which the Review was originally started. We have only to add, that the contents of this department will vary much both in length and in the matters treated of, and that we invite and expect to receive contributions from scholars abroad as well as from our own countrymen.

RECOVERY OF THE MISSING BOOKS OF GALEN'S PRINCIPAL ANATOMICAL WORK.

It has lately been mentioned in the 'Guardian' newspaper[4] that there is about to issue from the Oxford University Press "the Arabic translation of the principal anatomical work of Galen, great part of which does not now exist in the original Greek, and has never been published in any form." As the interest and importance of this announcement to medical scholars is considerable, (perhaps it is hardly overstated by the writer in the 'Guardian,' when he says it "is about as great as would be the discovery of the lost books of Livy to all lovers of classical literature,") it seems advisable to give a full and authentic account of the whole matter, especially of what has been proposed to be done, both in this country and on the Continent.

It is now nearly twenty years since the following notice was sent by Dr. Greenhill to the 'London Medical Gazette'[5]:—

"A very interesting and valuable discovery[6] has lately been made at Oxford, which it seems right to lay before our medical brethren, though we are almost afraid that its importance will be better understood and more justly appreciated in France and Germany than in Great Britain.

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[1] See Choulant's 'Biblioth. Med.-Hist.,' Lips., 1842; and Rosenbaum's 'Additamenta' to the work, 2 parts. Hal. Sax., 1842 and 1847.

[2] 'Janus. Zeitschrift für Geschichte und Literatur der Medicin,' 3 vols. Breslau, 1846-8.

[3] In 2 vols. Gotha, 1851-53.

[4] Dec. 23rd, 1863, p. 1205.

[5] Dec. 6th, 1844, p. 329.

[6] With respect to the use of the word "discovery" in this place, it applies, not to the *existence* of the MSS. in question, (for, as Dr. Wetzstein very properly remarks, they are plainly mentioned in Uri's Catalogue, and therefore cannot be said to have been ever lost,) but rather to their *interest and importance*, which for about a century and a half had been entirely overlooked. Dr. Daremberg calls the circumstance a "découverte de nouveau, pour ainsi dire," which is strictly accurate in every sense. 'Œuvres de Galien,' tome i., Preface, p. xi.

"It is well known that Galen's principal anatomical work, called Περὶ ’Ανατομικῶν ’Εγχειρήσεων, 'De Administrationibus Anatomicis,' consisted originally of fifteen books, of which only eight and part of the ninth have come down to us. The contents of each book are mentioned by himself, 'De Libris Propriis,' cap. 3, (tom. xix. pp. 24, 25, ed. Kühn) from which account it appears that the last six treated of the eyes, tongue, œsophagus, larynx, os hyoides, the nerves belonging to these parts, the arteries, the veins, the nerves arising from the brain, those arising from the spinal marrow, and the organs of generation: so that Galen's account of several of the most important parts of the body is contained in the lost books.

"In Ackermann's 'Historia Literaria' prefixed to Kühn's edition of Galen (p. lxxxiv.) we find the following notice:—'E Golii Arabico codice libros xi usque ad xv editurum se promiserat Thomas Bartholinus, 'De Libris Legendis,' Dissert. iii. p. 75 [p. 58, ed. 1711]. Erant Galeni 'De Administr. Anatom.' libri sex postremi cum adnotationibus Jacobi Golii in Bibliotheca Narcissi, Archiepiscopi Dublinensis, n. 1787.' No further information on the subject could Ackermann (who was a most diligent and accurate inquirer) obtain; nor apparently could Kühn himself, who, in the last volume of his edition of Galen, corrects some errors and supplies some omissions.

"In turning over the pages of a very different work, J. G. Wenrich's dissertation 'De Auctorum Græcorum Versionibus et Commentariis Syriacis, Arabicis, Armeniacis, Persicisque,' (Lips. 1842, 8vo.) we noticed that two copies of the Arabic translation were said (p. 245) to exist in the Bodleian Library at Oxford, one consisting of fifteen books, the other only of the last six.

"Upon referring to Uri's Catalogue of the Oriental Manuscripts of the Bodleian (p. 135), we found that the latter manuscript was said to be in the handwriting of Golius himself, that it had belonged at one time to Narcissus Marsh, Archbishop of Dublin, and was therefore probably the very MS. spoken of by Ackermann; and the actual examination of the two MSS. in question has shown us that the modern one was copied from the other, the pages of the original being marked in the margin of the transcript.

"The original MS. is written on oriental paper, and by an oriental scribe, and contains the complete work of Galen in fifteen books. It was bought at Constantinople for forty-eight florins, (rather a large price,) but by whom is uncertain; nor is anything else known of its history, except that it once belonged to the Archbishop of Dublin, though it does not appear in the list of his MSS. contained in the 'Catalogus Librorum MSS. Angliæ et Hiberniæ,' printed in 1697. It appears to have been seen and used by Golius (a celebrated Arabic scholar at Leyden), who must have known that the Greek copies of the work contained only nine books, and who accordingly copied the remaining six with a view to publication. He did not, however, transcribe the remainder of the ninth book, which is wanting in the Greek copies, and which is about twice as long as the portion hitherto known in Europe. The transcript was either given as a present by Golius, or bequeathed as a legacy at his death in 1667, to Thomas Bartholinus the elder, Professor of Anatomy at Copenhagen, and was in his possession in the year 1672, when he wrote his work 'De Libris Legendis.' Probably after his death in 1680 it came into the hands of Narcissus Marsh, Archbishop of Dublin, and appears in the catalogue quoted above. From him it came, either by gift or legacy, to the Bodleian Library at Oxford, where it still remains, together with the original MS. from which it was transcribed.

"It should be added that (as far as we are aware) no other copy of the Arabic translation is to be found in any European library; nor do any of the old Latin translations contain the last six books of the treatise."

The preceding notice was also inserted in the 'Classical Museum,'[1] and it was translated into German by Dr. Wetzstein, a well-known orientalist of Berlin,[2] and into French by Dr. Daremberg of Paris.[3] The fact of the existence of the Arabic translation was mentioned in several works, both medical and literary.[4]

Very soon after the translation was brought to light, preparations were made for the publication of the work. The Sydenham Society (whose "particular attention" had been directed to the subject by the editor of the 'London Medical Gazette') undertook to publish an English translation, to be executed by Dr. Adams, of Banchory, and Dr. Greenhill. The Arabic text was to be edited, together with the Greek original as far as it goes, by Dr. Greenhill, and was to be published either at the Oxford University Press, or at the French Royal Press, (there was a *king* at Paris in those days,) as part of Dr. Daremberg's 'Bibliothèque des Médecins Grecs et Latins.'[5] It was to form the commencement of a complete edition of Galen's works, to be executed with the assistance of various medical scholars both in this country and on the Continent.[6]

Dr. Daremberg caused a copy of the Arabic version of the missing books to be made, which is now in the Imperial Library at Paris, and from which M. Dugat of Paris (whose name is well known to Oriental scholars) made a rough French translation.[7] Dr. Wetzstein wished, "aus patriotismus," that one of his own countrymen should take part in the publication of the work, and even ventured to recommend the fittest man—viz., Dr. Sontheimer, of Stuttgard, the translator of Ibn Baitár.[8]

It was stated above that nearly twenty years have elapsed since the MS. was resuscitated, and brought before the notice of the public in the 'London Medical Gazette'—a time which is, indeed, amply sufficient to "work strange alterations," and which has had an important influence on the fortunes of this work. Dr. Greenhill's labours were interrupted by change of residence and occupation; Dr. Daremberg's (still more roughly) by the French Revolution of 1848, and the subsequent political troubles; Dr. Sontheimer died a few months after he was named by Dr. Wetzstein; Dr. Adams lived some years longer, but is now gone; the old Sydenham Society is extinct; and the Royal Press at Paris first became *National*, and is now *Imperial*. To which it may be added, that three of the Journals in which the discovery was mentioned (viz., the 'London Medical Gazette,' the 'Classical Museum,' and the 'Janus',) have ceased to exist, and in the last case its learned editor, Dr. Henschel, also. Where shall we find such a series of (seemingly) untoward events, except in the case of the *Great Eastern?—Quod abominamur*, and therefore we gladly hasten to state, that, after an interval of many years, the work has at length been resumed by Dr. Greenhill, in conjunction with Dr. Daremberg, and that it has been offered to the Delegates of the Clarendon Press at Oxford, and has been undertaken by them.

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[1] October, 1845, vol. iii. p. 306.

[2] In the 'Zeitschrift der Deutschen Morgenländischen Gesellschaft,' Leipzig, 1847, p. 203.

[3] First in his 'Voyage Médico-Littéraire en Angleterre;' 'Gazette Médicale,' Paris, Sept. 4, 1848; and afterwards in his 'Plan de la Collection des Médecins Grecs et Latins,' p. xxx., prefixed to the first volume of Oribasius. Paris, 1851.

[4] Among others, in Smith's 'Dict. of Greek and Roman Biography,' vol. ii. p. 212, 1846; in the 'Journal des Débats,' July 24th, 1847; in Henschel's 'Janus. Zeitschrift für Geschichte und Literatur der Medicin,' vol. ii. p. 396, Breslau, 1847; in Haeser's 'Geschichte der Medicin,' p. 144, Jena, 1853.

[5] See his Prospectus, 1847, pp. 27, 28. [6] See Henschel's 'Janus,' vol. i. p. 439.

[7] See 'Œuvres de Galien,' 1854, Preface, p. xi.; Oribasius, vol. iii., Preface, p. xviii.)

[8] 'Zeitschrift,' &c., p. 206.

According to the present plan, it is to consist of the whole of Galen's principal anatomical work, which has never hitherto been printed in a complete form in any language; the first eight books, and part of the ninth, are to be in Greek, the rest in Arabic, with a Latin translation to the whole.[1]

Before concluding this notice, we cannot help adding a few words on the singular history of this work, which, if it had been known to D'Israeli, would surely have secured it a place in his 'Curiosities of Literature.' It was written in the latter half of the second century after Christ, and burnt during the author's lifetime, together with some of his other writings, in the great fire at the Temple of Peace at Rome. It was then rewritten in a much improved form, and continued whole till at least the ninth century, when it was translated into Arabic by Honain Ibn Ishak, by whom so many other Arabic versions of Greek medical works were executed. It was probably the chief source from which all the anatomical knowledge of the Middle Ages was derived more or less directly; but its bulk prevented its being very frequently transcribed, so that at present only four or five Greek MSS. are known to exist, all of which are imperfect. Of the Arabic version only one MS. has been found, which, in the latter part of the seventeenth century, was in the hands of Golius and Thomas Bartholinus, both of whom were aware of its literary value, and proposed to publish it. Then for a hundred and fifty years it lay unnoticed and (practically) unknown in the Bodleian Library at Oxford, although its title is plainly printed in the Catalogue of Oriental MSS. It has now been again brought to light, and can hardly fall back into its former obscurity and neglect. Dr. Wetzstein asks indignantly, if it is still to remain unpublished, and "to slumber on till the twentieth century?" Under present circumstances we think we may venture to assure him, that such an accident is (to say the least) highly improbable; for the work is now so far advanced, that, if the present editors are not allowed to complete it, there will be no difficulty in finding others to finish what remains to be done.

"*Returns*" *of Diseases and of the Weather from the Great Cities of Europe.*

We find it stated in a recent weekly return of births and deaths in London, issued by authority of the Registrar-General, that this officer "has recently brought under the notice of the authorities of the great cities of Europe a plan for securing returns of diseases and of the weather, simultaneous with those of London. Vienna, which is the seat of one of the greatest medical schools of Europe, has already responded to his application, and he expects to receive from that city a regular series of returns, which cannot fail to afford interesting comparisons with those of London. Vienna is about to adopt many of the sanitary improvements which have proved efficacious in England; and Dr. Glatter, a most zealous officer of the City Council, will be able to measure their effects on the reigning disease and the rate of mortality." We hope that this proposal, so important as it must prove, if carried out, towards furthering the study of "vital statistics," will so find acceptance by the authorities of other cities as speedily to be acted upon in many directions.

Note referring to our recent Article " On the Structure and Functions of the Spinal Cord."

Our readers will remember that in the fore-mentioned article we considered pretty fully the observations of Mr. Jacob Lockhart Clarke, in conjunction with those of others. In doing so we, however, regret to find that, whilst treating of the "connective tissue" of the spinal cord, we noticed the labours of Virchow and Owsjannikow, but omitted, through inadvertence, to allude to the investigations of Mr. Clarke in the same direction. We feel it due to the latter gentleman to state, that he recognised the existence of the connective tissue in the grey matter of the spinal cord *prior* to the publication of the papers of the above-named observers, as may be at once seen by reference to the "Philosophical Transactions for 1851."

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[1] In preparing the latter part of this, great use will be made of M. Dugat's French translation mentioned above.

BOOKS, &c., RECEIVED FOR REVIEW.

Proofs of the Non-existence of a Specific Enthetic Disease. Addressed to the Secretary of State for War. By D. Maclaughlin, M.D. London, Churchill and Sons. (Pamphlet.)

An Essay (Historical and Critical) on the Mechanism of Parturition. By W. Leishman, M.D. London, Churchill and Sons. 1864. pp. 129.

Mémoires sur la Chromhidrose ou Chromocrinie Cutanée. Par le Dr. Le Roy de Méricourt, Professeur à l'Ecole de Médecine Navale de Brest. Par le Dr. C. Robin. Paris, Baillière. 1864. (Pamphlet.)

On Sufficient and Insufficient Dietaries, with special reference to the Dietaries of Prisoners. By W. A. Guy, M.B. Cantab., F.R.C.P., &c. (Reprint from Journal of the Statistical Society. Vol. XXVI.) London, Harrison and Sons. 1863.

A Practical Treatise on Diseases of the Skin in Children. (From the French of Caillault.) Second Edition. By R. H. Blake, M.R.C.P. London, Churchill and Sons. 1864. pp. 331.

A Vindication of the Present State of Aural Surgery. By a Member of the New Sydenham Society. London, Churchill and Sons. 1864. (Pamphlet.)

Observations on Some New Remedies (made in the Edinburgh Royal Infirmary). By J. Watson, M.D. Inaugural Dissertation, August, 1863. (Reprint from Edinburgh Medical Journal, January, 1864.)

Phthisis and the Stethoscope: or the Physical Signs of Consumption. By R. P. Cotton, M.D., &c. London, Churchill and Sons. 1864. Third Edition. pp. 104.

A Handbook of the Practice of Forensic Medicine, based upon personal experience. By F. J. Caspar, M.D. Vol. III. Translated by G. W. Balfour, M.D., for the New Sydenham Society. London, 1864. pp. 417.

Elements of Physics, or Natural Philosophy. Written for general use, in plain or non-technical language. By N. Arnott, M.D., F.R.S. Sixth and Completed Edition. Part I. London, Longman and Co. 1864. pp. 400.

A System of Surgery. By J. Miller, F.R.S.E. Edinburgh, A. and C. Black. 1864. pp. 1387.

Agricultural Education. Series of Lectures delivered at the Cirencester Agricultural College. London, Longman and Co. 1863. pp. 167.

The Restoration of a Lost Nose by Operation; exemplified in a Series of Cases illustrated with Wood Engravings. By J. Hamilton. London, Churchill and Sons. 1864. (Pamphlet.)

De L'Inosurie. Par le Doct. N. Gallois. (Mémoire Couronné par l'Institut (Académie des Sciences). Paris, Baillière. 1864. (Pamphlet.)

Transactions of the Medical and Physical Society of Bombay. No. VIII. New Series for 1862. pp. 274, with Appendix.

The Prescriber's Analysis of the British Pharmacopœia. By J. B. Nevins, M.D. London, Churchill and Sons. 1864. pp. 91.

Biographical Sketch of Sir Benj. Brodie, late Sergeant-Surgeon to the Queen, and President of the Royal Society. By H. W. Acland, Regius Professor of Medicine in the University of Oxford. London, Longman and Co. 1864. pp. 31.

A Manual of Diet and Regimen for Physician and Patient. By H. Dobell, M.D. Churchill and Son. 1864. pp. 36.

On the Relation between Anasarca and the Renal Affection, &c. Thesis for Medical Act in University of Oxford. By J. Andrew, M.D. Oxon. London, Adlard. 1864. (Pamphlet.)

The Law of Lunacy as it Affects the Insane charged with Crime. By J. G. Davey, M.D. (Reprint from Journal of Mental Sciences.)

Insanity and Crime: a Medico-Legal Commentary on the case of George V. Townley. By the Editors of the Journal of Mental Science. London, Churchill and Sons. 1864. (Pamphlet.)

Notes on the Climate of the Swiss Alps, and on some of their Health Resorts and Spas. By Hermann Weber, M.D., F.R.C.P. (Reprint from the Dublin Quarterly Journal of Medical Science, Feb. 1864.)

Confessions of the Faculty, with Comments. By a Medical Practitioner. London, Clayton. 1864. (Pamphlet.)

Excision of the Scapula. By J. Syme, F.R.S.E. Edinburgh, Edmonton and Douglas. 1864. pp. 35.

Goethe's Essay on the Metamorphosis of Plants. Translated by Emily M. Cox; with Explanatory Notes by Maxwell T. Masters, M.D. F.R.S. 1864. (Pamphlet.)

Functional Diseases of Women: Cases illustrative of a New Method of Treating them through the Agency of the Nervous System by Cold and Heat. Also Appendix illustrative of a new method of treating Epilepsy, Paralysis, and Diabetes. By John Chapman, M.D. London, Trübner and Co. 1863.

Three Hundred Consultations in Midwifery. By R. Lee, M.D., F.R.S. Churchill and Sons. 1864. pp. 207.

Etudes Cliniques de Médecine Militaire, &c. &c. Par M. Léon Colin, Médecin-Major de Première Classe. Paris, Baillière et Fils. 1864. pp. 304.

On the Calabar Bean: its Action, Preparations, and Use. By Mr. Nunneley, F.R.C.S.E. (Reprint from Lancet. Pamph.)

On the Characters, Action, and Therapeutic Uses of the Ordeal Bean of Calabar. Graduation Thesis, &c., Aug. 1862. By T. R. Fraser, M.D. Edin. (Reprint from Edinburgh Med. Journal, 1863.)

The Second Step in Chemistry; or the Students Guide to the Higher Branches of the Science. By R. Galloway, F.C.S., &c. London, Churchill and Sons. 1864. pp. 771.

The Science and Practice of Medicine. By W. Aitken, M.D. In two volumes. Second Edition. London, Griffin and Co. 1863. pp. 1095.

The Half-Yearly Abstract of the Medical Sciences. Edited by W. Ranking, M.D., and C. B. Radcliffe, M.D. Churchill and Sons. July to Dec. 1863. pp. 372.

Braithwaite's Retrospect of Medicine, July to Dec. 1863.

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The Tranversalis Pedis in the Foot of the Gorilla. By W. Thomson. (Read before the Medical Society of Victoria.)

On the Diseases of Women. By F. Churchill, M.D., &c. Fifth Edition. Dublin, Fannin and Co. 1864. pp. 937.

Anatomy, Descriptive and Surgical. By H. Gray, F.R.S. Third Edition. By T. Holmes, M.A. Cantab, &c. London, Longman and Co. 1864. pp. 788.

Annals of Military and Naval Surgery, and Tropical Medicine and Hygiene: being an Annual Retrospect, embracing the Experience of the Medical Officers of Her Majesty's Armies and Fleets in all parts of the World. Vol. I. for 1863. London, Churchill and Sons. 1864. pp. 376.

Inaugural Address. By R. Dundas Thomson, M.D. (From Proceedings of the Brit. Meteorological Society for Nov. 1863.)

On the Nature and Cause of the Respiratory Murmur. By Hyde Salter, M.D., F.R.S. (Reprint from the Med.-Chirurgical Review.)

Journals, Reports, &c.

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The Quarterly Journal of Science, No. 1, Jan. 1864. London, Churchill and Sons.

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Journal of the Scottish Meteorological Society. New Series. No. 1, Jan. 1864.

The Journal of Mental Science. Edited by C. L. Robertson, M.D., and H. Maudsley, M.D. Jan. 1864.

The Australian Medical Journal, Jan. 1864.

Thirty-sixth Annual Report by the Directors of James Murray's Royal Asylum for Lunatics, near Perth. June, 1863.

President's Report of the Cambridge University Natural Science Society. Easter Term, 1863.

Reports on the Nature of the Food of the Inhabitants of the Madras Presidency, and on the Dietaries of Prisoners in Zillah Jails. By W. R. Cornish. 1863. Madras.

NOTICE TO READERS.

The Editor is particularly desirous of having all Reports of Hospitals, Asylums, Sanitary Boards, Scientific Societies, &c., forwarded to him; as also Inaugural Lectures, Dissertations for Theses, Medical and Scientific Addresses, &c.

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